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CONTENTS BY NUMBERS.

JANUARY.

FEBRUARY.

MARCH.

APRIL.

MAY.

JUNE.

AUGUST.

OCTOBER

NOVEMBER.

DECEMBER.

ILLUSTRATIONS

PLATE I.

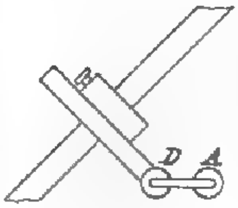


Fig. 1

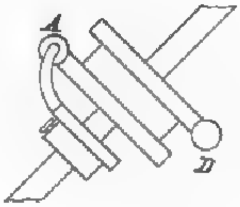


Fig. 2

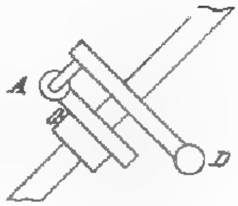


Fig. 3

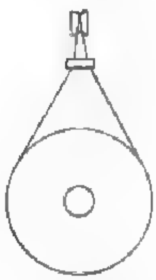


Fig. 4

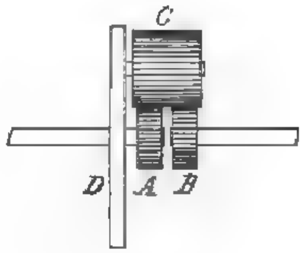


Fig. 5

Astronomy and Astro-Physics.

VOL. XIII, No. 1. JANUARY, 1894 WHOLE No. 121

General Astronomy.

TELESCOPE MOUNTINGS AND DOMES.*

WILLIAM H. PICKERING.

A recent brief visit to Europe has enabled me to make a study of a number of different telescope mountings in use in England and France, and to compare them with some of those employed in our own country. An especial study has been made of the arrangements for following a star in the telescope, and keeping it exactly upon the cross hairs. This is a matter of comparatively little consequence for visual work, but for photography it is one of vital importance, since owing to variations in the refraction with the altitude of the star, the driving clock can never follow with perfect accuracy, no matter how accurately the clock itself may run.

By far the most common method of following is by means of a slow motion screw adjusted by hand. This works in a nut in most of the American telescopes, while in the foreign instruments it usually fits into a worm wheel or sector similar to that actuated by the driving screw. In the Clark mounting, Figure 1, the driving screw D is attached to a nut working on the slow motion screw A, both being seen endwise. By turning the slow motion screw the driving screw is moved bodily forwards or backwards, thus turning the worm gear which is clamped to the polar axle. In the Warner & Swasey mounting which is somewhat similar in construction to that represented in Figure 3, the driving screw turns a worm gear which is loose on the polar axis. This worm gear carries the slow motion screw which fits into a nut attached to an arm which is clamped upon the polar axle. In the French method, Figure 2, as made by Gautier, there are two worm gears exactly alike, cut upon the same wheel, which turns loosely upon the polar axle. The slow motion screw A is clamped to the axle and is carried by the loose wheel, which is driven by the driving screw D. In the English mounting as

* Communicated by the author.

made by Grubb, Figure 3, the slow motion screw is attached to the loose wheel, and this screw turns a second worm gear which is clamped to the polar axle. Grubb usually employs sectors instead of complete wheels, as do also the other makers very frequently.

For photographic work the complete wheel for both driving screw and slow motion has a decided advantage over the sector, as the latter rarely permits of exposures of much longer than two hours' duration. Where several short exposures are to be made in one evening it is a great inconvenience to be constantly obliged to set back the sector, or else to have to consider whether it is likely to run out before the exposure is over. The only advantage of the sector is that it allows a greater radius to the worm gear than does the wheel, but if the gearing of the wheel is carefully ground, no appreciable irregularity in the motion will be introduced by this cause While both of the European methods employ an extra worm gear for the slow motion they have the corresponding advantage where a complete wheel is used that neither of the screws ever come to an end, but may be turned indefinitely. It seems to me therefore that a complete worm gear for each screw is most desirable, and that here if we use a slow motion screw at all, we cannot do better than follow the European example

The method of turning the slow motion screw is usually either by a double Hooke's joint, or by a cord passing over a large wooden wheel In the latter case the cord may be conveniently made to embrace the wheel by passing it through a small but heavy ring which hangs freely upon the cord by its own weight, Figure 4, and thence over a horizontal axis.* The Henry brothers use the Hooke's joint, with a long handle capable of reaching the hand when the observer is looking through the finder. We have employed the same method at Harvard, but have found it inconvenient and rather unsatisfactory. For very large telescopes it is almost out of the question Besides the cord, and the Hooke's joint, there is a third method of turning the slow motion screw, employed first by Repsold. This is the method of ring gears, in which the screw is connected directly by a system of mitre gears turning upon the polar and declination axles with a long handle running down the side of the telescope tube. Thus the declination and the right ascension adjustments are made by turning two handles placed side by side. This is a very convenient arrangement, but there is a certain amount of

* This arrangement is used in Dr. Huggins' Observatory.

back lash introduced by this adjustment, and for large instruments the ring gears are necessarily very heavy. Still another method of turning the slow motion screw which has occurred to me is to connect it through suitable gearing with a small electric motor. Wires could then be attached by which the current could be transmitted in either direction through the motor, causing it if properly arranged, to carry the screw either backwards or forwards, at will. This seems in some respects the most completely controlled method of turning the screw in case electric power is at hand.

But there are other methods of correcting the motion in right ascension which dispense with the slow motion screw altogether. One of these, known as the "mouse control," is employed by Grubb on many of his instruments. In Figure 3 let A and B be two gears which are just alike save that one of them has one more tooth than the other. Their respective axles are in line but disconnected. The left hand axle transmits the power, and the right hand one is connected with the driving screw. D is a loose pulley, carrying the loose gear or so-called "mouse," C, which connects the gears A and B. When the power shaft revolves, carrying the pulley D, the driving screw shaft also turns at the same rate. If now we stop D in any position, the differential gearing of A and B comes into play, causing the driving screw to move at a slightly increased or diminished speed as the case may be. D may either be turned backwards or forwards by a cord, increasing or diminishing the rotation of B, or else two separate mouse controls may be employed, one for increasing and the other for diminishing the speed. In the latter case the pulleys are stopped electrically by wires leading directly to the hand.

It seems to me that the "mouse control" is a decided step in advance in the direction of accurate adjustment, over the slow motion screw. In our 13-inch telescope an endwise push of the driving screw of $\frac{1}{6000}$ of an inch moves the photographic plate through 1″. This slight motion of the screw may be entirely masked by the back-lash, or other causes, and in practice we find such is the case, and that accurate following for enlargments is a very difficult matter. The mouse control avoids all this, by simply changing the rate of motion of the driving screw, in the simplest manner imaginable, and no slow motion screw whatever is needed.

There is a very convenient device of general application which is employed at Greenwich, but whose bearing in the present instance does not at first sight appear. When it is wished to

correct the error of the standard meantime clock, it is not done by adding or removing small weights from a scale pan, as is customary at many observatories. Instead of this there is attached to the front of the pendulum of the clock a verticle permanent bar magnet, measuring $5 \times \frac{3}{8} \times \frac{1}{8}$ inches. Just below the magnet is placed a helix attached to the clock case. It is connected with a battery of eight Leclanché cells, and has no iron core. By passing a current through this helix in one direction or the other the error of the clock can be changed by six seconds, inside of one hour. In talking this matter over with Mr. Douglass afterwards it appeared that this arrangement might be adapted to the control clock of an equatorial. It would probably be best in order to change the rate more rapidly to employ two helices, one above and one below the magnet, and to furnish them with iron cores. By this arrangement no slow motion screw and no "mouse control" would be necessary. An electric current would merely be passed through the helices the intensity and direction of which could be varied by turning a switch held in the hand. Like the "mouse control" this adjustment would never come to an end, and it would have an advantage even over it in one respect. If in following a star we found that it was gradually leaving the crosswires, we should not be obliged to continually bring it back again every few minutes, as soon as we were able to detect an appreciable error in its position. We should do better than that. We should pass a slight current through the coils and change the rate of the clock, so that no error of position should occur,—and this without disturbing the driving mechanism in the slightest degree. It seems to me that we have here a new principle of following,—instead of constantly correcting the error of the clock, as the star varies in altitude, we merely correct the clock rate.

At Greenwich two methods of correcting the equatorial are employed, a rapid one by the slow motion screw, and a slow one by the "mouse control." I should suggest a rapid correction by a quick acting "mouse control" run forwards or backwards by a small electric motor, and a slow correction by the clock, and a rejection of the slow motion screw altogether.

Leaving this subject, let us now turn to the automatic control. This control is of two kinds; first that which is permanently attached to the driving clock, and is in continuous action though at a varying rate, throwing additional work on the clock when it runs too fast, and relieving it when it runs too slowly. The second kind is that which acts intermittently, throwing in addition-

al work when the driving clock gets ahead of an independent control clock, but never relieving the driving clock of any of its own work. In the Grubb and Warner & Swasey mountings the continuous control is accomplished by means of a ball governor, which, when the balls fly out, increase the pressure and therefore the friction, upon a fixed smooth metallic surface. In the Clark mounting the continuous control consists of a rapidly revolving fan wheel, whose vanes are set at a fixed angle before starting the clock. In the Gautier mounting invented by Foucault, the balls of the ball governor are each set in the center of a fan, so that when the speed is increased the fans fly out, and a very large increase of atmospheric resistance ensues, which regulates the rate of the clock in a surprisingly perfect manner. This form of control has been employed upon some American chronographs. So perfect, indeed, in action is it when carefully made, that Gautier does not consider it necessary to employ any additional intermittent control, or any pendulum whatever about the telescope. Nevertheless the astronomers who have used it tell me that while the clock follows admirably for short exposures such as five minutes, as indeed I was able to prove to my own satisfaction, yet when the exposure is longer, slight corrections were constantly necessary. These deviations may of course have been produced by refraction, but the observers themselves seemed to think that the introduction of a pendulum into the mechanism would be an improvement. The Foucault adjustable vanes are it seems to me an improvement on the fixed vane system, and I am inclined to think would be more uniform in their action than any control depending on the friction of solid bodies. In connection with a "mouse control" this device seems to me admirably adapted to telescopes intended solely for visual purposes.

Regarding the intermittent control introduced by an external and independent clock, Gautier, as we have seen, employs none. Grubb again introduces mechanical friction, but in the Greenwich instrument substitutes as omewhat complicated form of "mouse control," while Clark permits a spiral spring to be wound up to a greater or less extent, which when electrically released at the end of each second delivers its absorbed energy in the form of a blow, which does not affect the speed of the driving mechanism. When the parts affected are made strong enough to withstand the repeated blows, the latter method gives very excellent results.

Regarding the electrical connections, I found little to be learned in Europe. The Greenwich method of closing the circuit was to let the end of the pendulum swing through a drop of mercury. This

method enjoys the advantage of simplicity, but in our own case we found that the mercury had constantly to be renewed, at some inconvenience, and that even then the action was not always certain. A second arrangement that we tried consisted of a pin inserted on the face of the pendulum rod, near the top, which at each swing of the pendulum lifted a light spring furnished with a platinum contact. This is a well known method, but we found that unless the clock is a strong one, it is liable to stop it. We next tried attaching a small permanent horseshoe maget to the face of the pendulum, and allowed it to swing over an armature attached to a spring. This device is also an old one but was described anew a few years ago in the *Sidereal Messenger* by Mr Gerrish. It works pretty well, but though there is no friction introduced whatever, there is a much larger amount of energy absorbed in the magnetic induction than one would at first sight by any means suppose. The next apparatus for the purpose that we tried was made for us by a Peruvian watch-maker, and is represented in Figure 6. A pin A attached to the pendulum lifts a little lever BC by passing under the loose wheel B. This breaks the circuit at D. This contact is easily reached and cleaned without disturbing the pendulum, and the apparatus furnishes on the whole the best make and break that we have yet found.

In bringing this article to a close I will take advantage of this opportunity to make a few suggestions that have occurred to me as a result of my investigations. They are particularly adapted to the mounting of a large telescope, but some of them may perhaps be of use in the case of a smaller instrument. It seems to me that the cheapest and lightest form of building that could be erected to cover a large telescope would be built upon the plan of a large railway depot. In Figure 7 which is a plan of the dome, a steel ring is first laid to form its base. On this a series of parallel steel arches are erected of suitable sizes. These are united and braced by a series of cross ties giving the structure its final form. For small domes wood could be used as a substitute for steel. The whole is then either covered with sheet metal, or perhaps better still with galvanized iron netting. Finally the netting is covered with water proofed canvas. It has been found at Greenwich that netting covered with papiermaché stands the climate very well, and we have found in Cambridge that the same is true of canvas stretched upon wood. It therefore seems that a covering such as is here suggested might withstand very well even the rigors of a northern climate. The shutters of the dome

should slide horizontally according to the Hough or Warner, Swasey plans. The dome should be mounted upon a cylindrical iron framework, covered with one thickness of sheet iron. It is almost universal in Europe to mount the domes upon a heavy base of masonry, which collects and retains the heat well into the night. This principle seems to me to be radically wrong. We have gone to the opposite extreme at Harvard, the buildings, supporting the later domes consisting of a light wooden framework covered with a single layer of thin sheathing. Our experience there and particularly at Arequipa I think fully justifies our method of reasoning.

Permanently attached to the side of the dome, opposite the shutter, should be placed the curved rails on which the chair is to be raised and lowered. The chair, so-called, is a large structure, more nearly comparable to a small room, or perhaps better still to a theatre box. It is furnished with an invalid's reclining rolling chair with book rack attached for making observations near the zenith, with several ordinary chairs, a case of drawers for eyepieces, etc., a table and book shelves for a recorder, mean and sidereal clock dials controlled electrically, and with suitable sounders which can be thrown into the circuit when desired. The motions of the dome, shutter, and chair, which latter is partly counterbalanced by weights moving upon the opposite portion of the dome, are all necessarily moved by an electric-motor, which may be controlled from the chair. The telescope would be clamped in right ascension by the pressure between an electro-magnet and its armature. The clamp could thus be instantaneously applied.

The telescope itself is not run by clock-work, but by a small motor. Since even a large telescope can be so balanced as to be readily moved with one hand, it is evident that only a very small motor would be required for this work, one for instance such as might be employed to run an ordinary sewing machine. Such a motor would hardly need a separate pier from the telescope, but if found best one could be provided for it. The speed of the motor would be regulated by a tuning fork commutator, which would be more than sufficiently uniform for the purpose, (Figure 8). The rate of vibration would be regulated by jaws pressing upon the prongs of the fork near the crotch, and the length of the prongs thus regulated by a micrometer screw. The tuning fork would be placed in the chair beside the observer, who could allow for the varying refraction of the star he was observing by a touch of the screw. This plan would have all the advantages of

the pendulum control above described, since the observer would change the rate, and not the error, of his driving mechanism. In short the motor would save expense, save space and save winding. There would be no slow motion screw, no pendulum, no revolving fans, no ball governor. Yet it seems as if it would give quite as good results as the driving clock, besides being far more easily and conveniently controlled.

HARVARD COLLEGE OBSERVATORY,
December 1, 1893.

THE NATIONAL ARGENTINE OBSERVATORY.*

JOHN M THOME, DIRECTOR

The National Observatory of the Argentine Republic was created by an act of Congress in the year 1869; it owes its inception, however, to the efforts of an American Astronomer, Dr. B A Gould, of Cambridge. It was the life-long desire and purpose of this distinguished scientist to carry out a systematic survey of the southern heavens between the parallels of 23° and 80°, and to this end he had already accumulated a fund and acquired an instrumental equipment sufficient to warrant his undertaking, when his project became known to the minister of the Argentine Republic in this country, D. Domingo F Sarmiento,—then a candidate for the presidency of that nation,—who embraced the idea with enthusiasm, and offered his powerful aid toward equipping the expedition in a more complete manner with the object of establishing it as a permanent national institution.

Immediately after his election to the executive chair, President Sarmiento procured the passage of the act already mentioned, accompanied by a modest appropriation for the purchase of instruments and the erection of a suitable building to contain them Dr Gould was at once appointed director of the new institution, and was authorized to nominate four assistant astronomers. I had the good fortune to be selected as one of the assistants, and arrived in Cordoba with my colleagues about the middle of September of 1870

The instruments purchased by Dr Gould for the Observatory, and which still comprise its equipment, were a meridian circle with $4\frac{1}{2}$-inch object-glass by the Repsolds, two twelve inch lenses

* Read at the Congress of Astronomy and Astro Physics, Chicago, August, 1893

by Fitz of New York, one of them corrected for the photographic rays, two chronographs, a portable equatorial by the Clarks with 4½-inch glass, a comet seeker, Zöllner photometer, a repeating circle with tripod by Pistor and Martins, a sidereal circuit breaking clock by Tiede, various chronometers, barometer, thermometer, etc.

The erection of the buildings was begun immediately after our arrival upon a piece of land ceded for the purpose by the Provincial government. Much delay was encountered owing to the difficulty of obtaining skilled workmen; and the installation of the meridian circle was not effected before the middle of the year 1872, owing to a blockade of the ship during the French and German war.

The Observatory building is 100 feet in width by 80 in length, and terminates in domes, of which the east and west ones are 18 feet in diameter, and are connected with the main building by meridian rooms. The two smaller ones on the north and south are situated directly over the corresponding entrances. Adjacent to the Observatory on the east and west are the dwellings of the director and assistants, respectively, and there are some four acres of land in the enclosure, which is now under irrigation. The situation is in the immediate neighborhood of the city of Cordoba, on the south margin of the valley in which the city lies, and distant one mile from the central plaza. Its elevation above this is 100 feet, and above mean tide in Buenos Aires 413 meters. On the west, at a distance of about ten miles, a range of mountains, whose mean elevation above the plain is 1800 feet, extends in a nearly north and south line, and distant some fifty miles more, there is a parallel range, with truncated summit, forming a nearly level plateau several miles in width, whose mean elevation above the sea is nearly 6,000 feet. To the north, east and south, there extends an unbroken plain, almost treeless and waterless, where the drouth is sometimes prolonged over six months at a time. It has happened, various times, that the river Primero, which is the natural outlet for all the water in the valleys between the mountain ranges which I have mentioned, has run dry immediately below the city.

It was owing to these conditions—a dry transparent atmosphere, and a generally cloudless sky—that Cordoba was selected as the site for the Observatory. It is besides, nearly centrally situated with respect to the boundaries of the Republic, being distant 23^m 19^s from Buenos Aires on the east and less than 20^m from Mendoza on the west. The climate is in general, exceed-

ingly pleasant, being equally free from the sultriness of the summer nights in these latitudes, and the extreme rigor of your long and tedious winters, with their consequent discomforts to the working astronomer. It is a nearly ideal location for an Observatory, in short, and the promise made by President Sarmiento and his Congress, to protect and maintain the institution, has never been broken nor ignored by any of their successors under whatever stress of circumstances. My long residence in the country has been invariably agreeable and pleasant, and my intercourse with the citizens of all classes in different parts of the Republic, enables me to testify to their many noble, generous and brilliant qualities.

During the period embraced between our arrival in Cordoba and the mounting of the Meridian Circle, our energies were directed towards the elaboration of a Uranometry which should contain the true magnitudes and positions of every star as bright as 7.0 mag. in the region of sky extending from the south pole to ten degrees north of the equator. The estimates of magnitude were based upon the scale employed by Argelander for his Uranometry of the northern heavens, in order that the results should be directly comparable, but our final magnitudes resulted from the mean of at least two independent estimates of the same star by different observers upon a scale of tenths. Every part of the sky was examined twice at least for identifications and magnitudes by different observers, and where the estimates of magnitude differed considerably, new ones were made. Great care, entailing additional labor, was necessary for the correct identification of the stars in or near the clusters, or where the magnitudes as given in the existing catalogues were erroneous, and considerable discordance arose between the estimates of the various observers in the cases of colored stars; but after we had gained the proper skill by persistent practice, the average difference of our estimates did not exceed two-tenths of a magnitude for the white stars. After the stars had all been platted upon the final charts, I again compared them directly with the sky, for possible errors of identification or omissions, and obtained new estimates of magnitude where these were desirable. The intimate acquaintance with the southern constellations which I acquired in this way has always remained one of the chief pleasures of my life.

Argelander's Uranometry represents the utmost reach of his vision, and I believe he prided himself on the fact that all his estimates of magnitude were made by his unaided eye. In the Uranometria Argentina, the lowest limit is a fixed round magnitude,

faintly visible on a clear night to a good eye, but not based upon any particular one, and our estimates for the fainter stars were made with the aid of an opera glass, necessarily. It would have been impossible otherwise, to make an exact alignment. The Urancmetria Argentina also gives the first attempt at bounding the constellations by meridians and parallels; or, in other words, giving them boundaries that can be exactly described and located. The work, although complete in its essential features by the end of 1872, was not entirely satisfactory until the year 1877, when I brought the maps to New York to be printed.

With the installation of the meridian circle in 1872, preparations were at once made for the great undertaking of the Observatory. At the time of our advent in the southern hemisphere, there was neither a trustworthy Uranometry nor catalogue of stellar positions that could lay claim to completeness and exactness in existence. Such catalogues as had been published were distributed almost at random among the brighter stars of that immense region. On the northern limit Argelander had pushed his zones across the low altitudes to the parallel of 30°, and the region lying within 20° of the south pole had been observed by Gillis in Santiago. The intermediate region was occupied by catalogues, which were either of a partial or defective nature, and of epochs ranging from Lacaille's, in 1750 to Ellery's excellent observations at Melbourne, but none of them gave even on approximate idea of what might be beyond the limit of visibility, and they were all totally inadequate for the requirements of determining the course of any movable body. Until the completion of our zone observations we were compelled, when observing planets and comets, to use anonymous stars for the larger part of our comparisons, and this still holds true, though in less degree, when we pass to the north of the limit of the Cordoba Zones. Within that limit, however, the requirement of a conveniently situated and well determined point of comparison for any given moment is now very nearly met; certainly in the large majority of cases.

At the express desire of Argelander, I believe, Dr. Gould began his zone observations with the parellel of 23°, and then gradually proceeded, by convenient distances in declination, with generous laps, to the northern limit of Gillis's circumpolar work. The duration of the zones averaged nearly two hours, and there were three of these observed upon every clear night. An indispensable requirement at the beginning and end of each night's work and during the intervals of rest by Dr. Gould between zones, was the rigorous determination of all the instrumental and clock correc-

tions. At the same time, such stars as were conveniently situated for observation during these intervals were included in the regular programme of the night's work. These were observed over eleven threads, all the four microscopes were read for the declination co-ordinate. The observations were made by two assistants detailed for the night, who alternated with the zones.

Only one microscope was read for the zone observations, and only one reading of the microscope was made for each star, but this was afterwards referred to the mean of all the four microscopes, which were carefully read for both north and south limit at the beginning and end of each zone. The transits were usually over one tally only, consisting of either three or five threads, but in the laps, or where it seemed desirable to re-observe a zone, many observations were duplicated and some stars have been observed as many as five times. All the observations at the telescope—transits, declinations and magnitudes—were made by Dr. Gould; the assistant made the microscope bisections and recorded them, the magnitudes, name of tally employed and approximate time of transit. The transits were made at any convenient part of the field, but the settings in declination were always referred to the zero of the two fixed horizontal threads. The transits were recorded directly upon the chronograph by the observer, and four complete records of as many stars could be made in a minute when the stars were at all evenly distributed.

By this method, each star was as independently observed as though it constituted the entire zone, and in all the various reductions up to the final one of mean epoch, it was so treated. Consequently, the only error affecting the adopted places is the inevitable one of observation, as affected by blurred or unsteady images or fatigue of the observer's eye. Taken as a whole, and considering the nature of the work, they seem to me to be a remarkably fine series. The zones were finished in 1875. More than 100,000 complete observations had been made in that time.

Growing out of the small beginnings between zones and the partially clouded nights, when some observations were impracticable, and advancing to full swing when these were completed, arose the structure of our General Catalogue of rigorous positions, which was finished in 1880. The observations and reductions for this catalogue were made, almost exclusively, by the assistant astronomers, and every operation was performed with all the care and deliberation required in fundamental work. In the beginning we employed three minutes of time and the services of two assistants to record a complete observation, *i. e.*: 11

transits for the catalogue and circumpolar stars, 17 for the time stars, and four microscope readings. As the work proceeded, this interval was reduced to two minutes, which was found to be amply sufficient to insure accurate work. Our working list of stars, which was collected by Dr. Gould, contained 1st, all the visible, Uranometry, stars; 2d, fainter stars from the published catalogues; 3d, anomyous stars which had been noticed during the course of the work, and finally, Zone stars. It is, therefore, a General Catalogue in the literal sense of the word, because it includes all the stars contained in every southern catalogue except the Cordoba Zones. The number of complete observations made for it exceeded 140,000, beginning with the middle of 1872, and ending with 1880.

Conjointly with the catalogues mentioned, a large number of planets and comets were observed, the geographical co-ordinates of the Observatory and of most of the capitals of the various provinces and the river ports were determined telegraphically, and an extensive series of photographs of the principal stellar clusters and double stars was obtained.

At the beginning of the year 1885, Dr. Gould, who had so admirably and successfully directed the affairs of the Observatory from its beginning, steadfastly guiding its course amid innumerable obstacles, difficulties and personal afflictions toward the accomplishment of the task which he had set for himself, was compelled to yield to the severe strain upon his health, and retired crowned with honors and the laurels of a service to astronomy which has probably never been surpassed by any other man. In his final memorial to the minister, he briefly outlined the course which, in his opinion, it would be most advantageous to follow in the interests of astronomy, and it has been my aim, as his successor, to conform to this as closely as circumstances would permit. Unfortunately the financial crisis, which had not then begun, has since grown into such proportions that we are now compelled to struggle along under a greatly reduced appropriation—nominally the same, but really two-thirds less.

Continuing the observations of comets and planets, of which we have already accumulated several thousands; the determinations of latitude and longitude in various parts of the republic; photometric and photographic work; and various special problems for the solution of which our aid was solicited by the foreign astronomers, our chief aim has always been to explore the region of sky which is distinctively our own, and to this end we have begun the extension of the northern Durchmusterungs from the limit to

which they had been carried by the astronomers at Bonn. Our observations at present number over one million, and give the approximate position and magnitude of every star down to the 10th magnitude in the belt of the heavens comprised between 22° and 42° of south declination, some 340,000 in number. The first volume of these results has been published and distributed, and the second, together with a series of 12 charts covering the region, will be finished about the beginning of the coming year. The unexpectedness of my leave of absence, and the suddenness with which I availed myself of it, interrupted an investigation of the distribution of the stars upon which I was engaged, and here I have had little inclination to take up any work during my short leave after 16 years of continuous duty. I think, however, it can be shown from our results that the law of equable distribution holds good down to our lowest limit.

Meridian observations for rigorous stellar positions have been continued as before, and these now number something over 60,000, all well advanced in the reductions. I have, besides, revised, corrected and published seven volumes of meridian observations in manuscript which Dr. Gould confided to my care, notwithstanding the great falling off in our appropriation. Sixteen volumes, in all, have been published, and there is material ready for five or six more.

I think most astronomers, especially those of maturer age and experience, will agree that our time and means have been well and diligently employed, whatever lack of brilliancy there may be in the achievement. What we have accomplished was done by concentrating our energies and directing them upon one distinctive object. There has never been more than one director at a time in our Institution and the principle seems to have worked well.

PROPER MOTIONS OF DOUBLE STARS.*

S. W. BURNHAM.

I.

There are many double stars, so-called, where the change in the position is obviously the result of proper motion. Strictly speaking these are not double stars so far as the distant components are concerned. Most of these examples are from the cata-

* Communicated by the author.

logues of the Herschels and the Struves. In some of these wide pairs, one or the other of the components has been found in recent years to be a close pair, thus making a real double star, and sometimes a most important physical system. The measures of these distant optical companions, however, serve the purpose of giving a very accurate determination of the relative proper motion. In some instances it is certain that this belongs entirely to the principal star, and in most of the remaining examples it is at least highly probable that this is true of them.

In my observations of new pairs, I have as far as practicable measured an outside star, when there happened to be one near enough for convenient measurement with the micrometer, in order to determine in the future any proper motion that might exist. Many of these stars have already shown unmistakable rectilinear motion, and in some instances the data is sufficient to give the amount and direction of this movement.

I have investigated many of these stars from the various catalogues, and propose to give here some of the results. We already know the proper motions of many optical and physical pairs from positions with the meridian circle, but only stars where the motion is large, and the annual change half a second and upwards. I shall therefore give here as a rule only stars whose proper motions have not been determined from meridian observations. While these quantities in most of the stars are small, and would be very uncertain if the values depended upon positions with the meridian circle, there can be no doubt of the substantial accuracy of the results when based upon measures with the micrometer by experienced observers, and it is not probable that observations in the future will materially change these movements. The graphical method has been used throughout.

No. 1. β 999 ω Andromedæ.

In 1872 I found with the 6-inch that a 9 mag. star in the field with ω Andromedæ had a small attendant; and in 1881 with the 12-inch at Mt. Hamilton I discovered a close companion to the large star. So we have a quadruple group of two pairs separated by a distance of 130″. The measures of the brighter component of the fainter pair commence in 1881, and end in 1891. There has been absolutely no change in the direction of C, but the distance has diminished 3″.3 during this time. From these measures we get 0″.300 in the direction of 110°.3 as the annual proper motion of A. The proper motion given by Stumpe from meridian observations is 0″.348 in 107°.0. I have included

this star in my list because of this change in the value of the proper motion The close attendant to the principal star certainly shares in this motion. It is therefore a physical system, and certain hereafter to be of special interest. It has already moved 8° in position angle around the other star, the distance remaining constant.

No. 2. β 1101. ψ CASSIOPEIÆ.

This star has two distant companions, discovered by Herschel in 1783. Struve gave the magnitudes 8.9 and 9.5. The brighter is now 28″ from ψ. These small stars (CD) form a 3″ pair. In 1889 I found a close companion of 13 mag. at a distance of 3″ from the bright star, so that now we have a quadruple group consisting of two pairs 28″ apart.

The measures of C cover a period of something more than a century. The measures of Herschel, Struve, Dembowski, Hall and Burnham give for the annual proper motion of A 0″.063 in the direction of 67°.5. There has been no change in the Struve pair (CD) These stars are relatively fixed, and probably make an optical pair. It is impossible at this time to say positively whether or not the new star has the same proper motion as A. The second set of measures in 1891 showed a direct motion in angle of about 4°, with a little diminution in the distance. This change does not correspond to the proper motion of A, and it is probable that these stars make a binary system.

No. 3. β 487.

The wide pair is Σ 17. The components are 8th and 9th magnitudes, and the distance 27″. B was discovered to be double in 1878, the new companion being very faint, and 2″ from the other. My measures of BC in 1891 show a small increase in the distance, but this may not be real The measures of the wide pair by Struve, Dembowski and myself give the annual proper motion as 0″.010 in the direction of 209°.4.

No. 4. β 643.

The wide pair is Σ 2342, the principal star being about 6 mag., and the companion nearly 9 mag., at a distance of 29″. In 1878 I found a much nearer and extremely faint companion about 9″ from A. The measures of C from 1830 to 1891 give a proper motion of 0″.052 in the direction of 144°.0. My measures of B in 1878 and 1891 show an apparent displacement of that star, in substantially the same direction as the other, of 0″.043 annually.

As this is a very faint star, and difficult to measure, and the observations cover only a short interval, it is safe to conclude that these companions are relatively fixed, and that the movement is in the bright star. The value and direction of this motion should be taken from the measures of C, with respect to both companions, therefore this star has no further interest as a double star.

No. 5. β 582.

This is another of the wide Struve pairs (Σ 1179). There is no sensible difference in the magnitudes of the components. Struve rated them 8.5 mag., and in my last measures in 1891 I called them 8.7 mag. The distance now is about 20″. In 1878 I found that B was double, having a very minute attendant at a distance of 3″.7. The measures of AB by Struve, Dembowski and Burnham give an annual proper motion of 0″.032 in the direction of 17°.0. As there is apparently no change in BC, the change in the wide pair is due to the motion of A.

No. 6. β 511.

This is very similar to the last named pair. A and B constitute Σ 171, each 8.5 mag. and now about 30″ apart. In 1878 I found a 12 mag. companion 3″.7 from B. The measures of the three observers mentioned in the preceding pair, give for the proper motion of A 0″.042 in the direction of 6°.2. There has been no certain change in BC.

No. 7. β 633. γ Draconis.

This is only an optical pair. The companion is 13 mag. The interval is too short to give as accurate a value of the proper motion as that found for the other stars in this list. Still it must be approximately correct. My measures give 0″.016 for the annual movement in the direction of 283°.0.

No. 8. β 825.

The wide pair is Σ 2268, the components being about 20″ apart. In 1881 I found a new companion between these stars. The change in the Struve star, as would be expected, is clearly due to proper motion. From the measures of Struve, Mädler, Dembowski, and three sets of my own measures from 1881 to 1891, I find the annual displacement of A is 0″.050 in the direction of 345°.9. The measures of B, which cover a period of ten years, do not appear to indicate any sensible change with refer-

ence to A, but the new star is only 13 mag. and such an annual motion might not be apparent in so short a time. If these stars are relatively fixed, it would tend to show that the proper motion was in C rather than A.

No. 9. β 600.

The wide pair was first noted by Sir William Herschel, and it was measured by South and Herschel in 1823, the stars then being 67″ apart. In 1878, the 18½-inch showed a close component to the principal star, which appears to have considerable angular motion. The only measure of the distant star, since those mentioned above, were made in 1878 and 1892, by the writer. These observations show an annual proper motion of 0″.016 in the direction of 99°.0. The principal star is just visible to the naked eye.

No. 10. β 815.

The principal star is nearly 8½ magnitude, and the companion 10½, at a distance of 6″.4 at the time of discovery. The relative motion is clearly rectilinear, and the value should be accurately known. The measures from 1881 to 1892 give an annual movement of 0″.15 in the direction of 142°.9.

No. 11. β 497.

This is a distant companion to a sixth magnitude star (B.A.C. 239). As the close double companion is comparatively a faint star, the motion probably belongs to A, which is 120″ from BC. My own measures and those of Engelhardt give for the annual motion 0″.206 in the direction of 330°.5.

No. 12. H. V 18. α Cassiopeiæ.

There are two companions nearer than that noted by Herschel in 1781. The nearest is 17″ distant, but the only measures are those made at Mt. Hamilton in 1889. For the distant star the measures extend over a period of more than a century. Herschel's positions seems to be substantially correct. The proper motion is, therefore, 0″.093 in the direction of 128°.8.

No. 13. Σ 23.

The principal star is 7.6 mag. and the companion 9.9 mag. When Struve first measured this pair, the distance was nearly 14″. It is now but little more than 6″. The best measures are

very accordant, and give a proper motion of 0″.114 in the direction of 14°.4.

No. 14. Σ 44.

The magnitudes of these stars according to Struve are 8.3 and 9.0. At the time of the first measures, they were nearly 8″ apart. Some of the observations are discordant, but the results found by the best observers show exact rectilinear motion. These measures give for the annual movement 0″.032 in the position-angle of 111°.0.

No. 15. Σ 86.

This is a 12″ pair of small stars, 8.0 and 8.7 mag. The observations are numerous, and the rectilinear motion very decided. These measures give 0″.049 in the direction of 274°.7 as the annual proper motion.

No. 16. Σ 118.

This is a 10″ pair of 8.5 and 9.4 magnitude stars. The measures down to 1888 give a proper motion of 0″.047 in the direction of 310°.3.

No. 17. Σ 133.

The principal star is 7 mag. and has two distant companions which were mentioned by Struve. The nearest at that time was 29″ distant. These stars are respectively 10.5 and 10.8 magnitude. From the measures of Struve, Dembowski and Hall we get for the proper motion of A 0″.019 in the direction of 221°.8. As both companions give exactly the same result, it is certain that the change is all in the principal star.

No. 18. Σ 143.

Struve gives the magnitudes as 7.7 and 9.0. The distance at the time of the first measures was about 30″. The observations of Struve, Dembowski and Burnham give a proper motion of 0″.085 in the direction of 137°.3. There was a time when these stars were only 1″.5 apart.

No. 19. Σ 197.

The measures cover a period of fifty years and are very consistent. From them we find a proper motion of 0″.102 in the position-angle of 50°.6. The respective magnitudes of 7.3 and 8.3.

THE PHOTOGRAPHIC CHART OF THE HEAVENS *

There is a resolution of the Permanent Committee that each co-operating Observatory shall annually report to the Committee the work accomplished during the year. Though as yet no such reports have been published, it is otherwise known that satisfactory progress is being made with the taking of the "Catalogue" plates, that is, the plates of short exposure (6 min., 3 min., and 20 sec.), which will "render possible the construction of a Catalogue." Many Observatories have as yet taken no plates of long exposure (40 min.) at all, and those Observatories which are carrying out the complete scheme have naturally found the number of Catalogue plates increase more quickly than the number of chart plates. It may therefore be hoped that the Catalogue series, at any rate, will soon be completed, although it may be some years before the Chart plates are all taken

It becomes necessary to consider the reproduction, measurement, and publication of the plates more definitely. Plans already proposed have been criticised somewhat strongly in this magazine on the ground that they were too ambitious. There is a wide distinction between what you would like and what you can get. Perhaps the former might be summed up as follows:—

Reproduction.—Enlarged copies on plate-glass of all the plates to be made by photography and distributed to all Observatories.

Measurement.—The coördinates and magnitudes of all stars on the "Catalogue" plates to be measured several times for all three images.

Reduction.—All the measures to be corrected for the various errors and reduced to R. A. and Decl. 1900.0. Incidentally a large number of meridian observations to be made in order to determine the places of at least five stars on each plate at the epoch 1900.0 to within 1″.

Publication.—All this work to be published in detail, with a final Catalogue of the 2,000,000 stars.

This programme is of course purposely made extravagant; and yet much might be said in favor of all the items. It is a *maximum* programme, and could never be carried out. It has the great merit of simplicity, and any deviation from it immediately raises questions. Such questions are best settled by having a definite issue propounded, and without further preamble (we have lately learned to look upon preambles with suspicion, as contain-

* From *The Observatory*, December, 1893. By the Editors.

ing what is inconvenient to put in the bill) we offer the following programme for criticism. It will be convenient to rearrange the order of the sections as follows:—

Measurement.—If it be admitted that accurate and complete measurement of all stars on the Catalogue plates is impossible, the work may be limited in two directions:

(1) Accurate measurement may be retained for a select number of stars.

(2) All the stars may be measured with a sacrifice of accuracy.

A third course would be to combine the two.

In this article the comparative advantages of the three courses cannot be dwelt upon. We propose the second for the following reasons:

An accuracy about equal to that of the Revised Durchmusterung, which, as M. Loewy has so well pointed out, should be our basis, can be attained with very little labor. The places of the stars may practically be read out by one person and written down by another nearly as fast as he can write. A scale (or scales) in the eyepiece of a microscope divided to 0.1 mm. or 0.05 mm. will enable an observer to call out at sight the place of star to 0.01 mm. or 0.005 mm. by estimation. Or the images might be thrown on a screen and measured with a foot-rule.

Reduction.—Mr. Turner pointed out last month that the rectangular coördinates of a star on a plate are for many purposes more useful than R. A. and Decl.; and has deprecated the waste of labor in reducing such coordinates to R. A. and Decl. unless absolutely necessary.

There is, however, one question to be settled. Each star occurs on two plates, and though its coördinates on one plate are transformable into those on the other by simple formulæ, it remains to be decided how much of this transformation is to be done before publication of the results, and how much is to be left to the future. Now it must be remembered that although the réseau lines of the two plates containing the same star are nearly parallel in the vicinity of the equator, they are sensibly inclined at a distance from the equator; and thus the differences of the rectangular coördinates of a star on two plates may vary by several minutes of arc, according to its position in the the common region. It would lead to great confusion if a star had two sets of coördinates sensibly differing; and hence it would seem reasonable to reduce one set to approximate coincidence with the other by correcting for the sensible inclination of the axis of coördinates, to each other and the distances of the two origins. The actual rule

to be adopted in practice must be to some extent arbitrary, but the following proposition has merits:

The rectangular coördinates of the upper half of each plate to be published as they stand. The coördinates of the lower half to be corrected by those corrections, which would independently of errors of observation and manipulation, bring them into accordance with the coördinants on the two overlapping plates.

Publication.—It is obvious that the real unit is not the whole plate but the quarter plate. A quarter plate contains 13 × 13 squares of $5^{mm} \times 5^{mm}$. Would it be too extravagant to have a form printed on a quarto-page containing 13 × 13 squares, and to express the position of a star by putting *in the corresponding square* numbers expressing its distances (in minutes and decimals) from the top and side? The publication of a Durchmusterung has hitherto been on the plan of limiting the Decl. to a zone of 1°, but allowing R. A. to take precedence within this limit. Some such plan as suggested above is an attempt to distribute the stars by their *two* coördinates instead of *one* only. It has the obvious disadvantage of wasting space in cases where there are only a few stars on the plate, and not being adequate when there are many. But, after all, are we not in the former case already wasting glass and gelatine just in the same way, and does a little paper much matter? And in the latter case, if it is necessary to enter several stars in one box, can we not increase the size of the box?* The great advantage of such a plan would be that it would give at once a general view of the plate; it would, in fact, be half way towards reproduction. (In the lower halves of the plates the boxes would not correspond to the réseau boxes, but would represent a corrected réseau.)

Reproduction.—If the above scheme of publication of measures of the Catalogue plates be adopted, this would be nearly sufficient. Each plate should perhaps be copied at least on glass, and the duplicates from all Observatories deposited at some Central Institution. For the Chart (long exposure) plates similar duplicates should be made; and paper prints would probably be sufficient for distribution.

The above is essentially a *minimum* programme, and practically represents the least that can be done to secure the perman-

* One of the disadvantages of a Durchmusterung of the accepted form is the impossibility of inserting new or omitted stars. The blank boxes of a form such as that here suggested might be utilized in many ways, e. g. for the insertion of fainter stars from the Chart plates in barren regions. Such additions might be made either before publication or in MS after publication. In fact the form would have this and other advantages of an actual map.

ence of the photographic record. If we had satisfactory evidence of the imperishability of the plates, their measurement might be omitted altogether. But gelatine plates have only been known for a small fraction of a century, and we cannot afford to trust to their permanence for one or more centuries. Hence it is advisable, if not imperative, to put in the form of a permanent record *as much as we conveniently can* of the information given by the plates. A most important condition to be observed in settling the limitation italicized is that the work should be completed in about the same number of years as are occupied in taking the plates; otherwise the plates of some part of the sky will be already old before they are measured. Hence an Observatory which takes some hundreds of plates a year ought to be prepared to measure at least a hundred plates a year, and complete measurement is out of the question. On the other hand, some plan of reading out the coördinates and magnitude of a star at sight—about as quickly as they can be written down—would seem to make it possible to get through the enormous amount of work at a proper pace.

Finally such work may be done quite independently of the complete and accurate measurement of a certain number of plates.

The present proposal is very far from being an adverse criticism of the admirable and energetic way in which the plates are being measured at Paris., which will produce results valuable in quite a different way. If any suggestion can with propriety be made as to the scheme of work at Paris, it would be that portions of the sky to be examined in this thorough manner should be chosen in different parts of the sphere, and not confined to a few zones; for in the present article it is assumed throughout that a complete measurement of the whole sphere is impracticable. But results still of the greatest value may be obtained for selected portions of it.

ASTRONOMICAL PUBLICATIONS. I.

WM. W. PAYNE.

It is a question of some interest to the astronomer of the present, how he is to find time to keep himself informed, in regard to what is being done in his own and kindred fields of investigation. He knows that he has not time at command to read and digest even a small part of the useful new publications that appear every month. There are too many of them, and besides that his best strength, mental and physical, must be given to his own

work in some particular line of astronomical investigation. To supply this need is presumed to be the special field of an astronomical journal.

A dozen years ago we began the publication of the SIDEREAL MESSENGER, animated, in the main, with this thought in mind. It was only intended for beginners in astronomy, for that was our own condition in regard to practical work, and we could not presume to instruct professional men in the science who had spent more or less of their lives in original research. Our surprise can hardly be imagined, when after a few months, we found on our subscription books, the names of a considerable number of the most learned astronomers in this country, with some prominent ones from across the Atlantic.

That was a trying place for a novice in the art of publishing to say nothing of the graver responsibilities of editing the MESSENGER, the namesake of that excellent magazine started in July, 1846, by the scholarly and honored O. M. Mitchell while director of the Observatory at Cincinnati. After many severe trials and much useful experience, we found that our venture would not be a failure for lack of support, if all legitimate aid within reach could be kept and gradually increased. In this we were not mistaken for the *Messenger* kept steadily increasing in usefulness for ten years, at the end of which time its size was nearly three times that of the first issue, and its contents improved, at least, in a corresponding ratio. This was more than could have been fully anticipated in view of the discouraging outcome of the attempt of Professor Mitchell,as will be remembered his magazine was discontinued after two years of trial for want of support. That fact in no way places discredit on the noble work of Professor Mitchel, for every one who knows anything about the character of that early magazine very well knows that his self-sacrifizing labor in this direction was not less than in others which may have had wider general notice.

It would be interesting to some of our readers, possibly, if we should give a fuller account of our publications, and also some notices of other similar ones now being published, especially for those who have not ready access to the sources of such information.

We also propose to make a brief review of astronomical journalism in subsequent numbers of this publication, and set out the needs and possibilities of this important field. We desire to say something to our co-laborers which we believe will be of general interest, to the professional astronomer and to the amateur.

THE POLAR RADIATION FROM THE SUN, AND ITS INFLUENCE IN FORMING THE HIGH AND LOW ATMOSPHERIC PRESSURES OF THE UNITED STATES.*

FRANK H. BIGELOW.

In two papers already published (*Amer. Meteor. Journ.*, Sept. 1893, *Astron. and Astro-Phys.*, Oct. 1893), a brief statement has been presented of the lines of evidence that tend to prove the following facts: (1) That the Sun emits two distinct types of radiant energy into the space outside its surface. (2) That the first is propagated radially in all directions, the part falling upon the earth, especially on its equatorial belt, being an electromagnetic wave, whose electromotive force $\int(Xu+Yv+Zw)d\tau$, by the law of conservation of energy, breaks up into the dynamic wave

$$\int\left(u\frac{dF}{dt}+v\frac{dG}{dt}+w\frac{dH}{dt}\right)d\tau,$$

partly inductive and partly magnetic in its instantaneous state, plus the static or potential stress

$$\int\left(u\frac{d\psi}{dx}+v\frac{d\psi}{dy}+w\frac{d\psi}{dz}\right)d\tau,$$

plus the irreversible energy of Joules' Heat

$$\int\frac{u^2+v^2+w^2}{C}d\tau.$$

The electromotive force probably originates in the atomic oscillatory discharges of the ultimate ponderable materials of the photosphere, is propagated with inappreciable loss through the frictionless ether to the atmosphere, where in passing through this gaseous envelope, a series of complex transformations take place. Thus the wave lengths are increased and frequencies are diminished, part of the waves penetrating to the surface of the earth, the remainder transferring their vibrations to the molecular constituents of the air, in the form of heat and of free electricity, which gives rise to the observed electric potential fall. The magnetic wave of induction, accompanying the electric wave in quadrature to it, acts as a vector upon the magnetic lines of force continuously surrounding the earth from its permanent magnetism, and generates a complex system of forces whose to-

* Communicated by the author.

pography has been marked out on my 30-inch globe model. (3) That the Sun also emits a polar magnetic field, emanated from some sort of a rigid nucleus, apparently quite steady in its general manifestations, which comes to the earth, concentrating according to the laws of magnetic conductivity in the oval region embracing the magnetic and geographical poles, though the influence of the same is clearly marked out to about 60° of magnetic polar distance. On the Sun the physical manifestations of this field are found in a heterogeneous distribution of the magnetism, having positive and negative poles, with meridians of greater and less intensity, which control to some extent the location of the sun-spots, the output seen as the corona, and possibly the faculæ and the prominences. At the earth the polar radiation of the Sun is perceived in the spasmodic magnetic storms, the outbursts of visible auroras, and in certain meteorological phenomena. A study of this polar field shows that the sun rotates in 26.68 days synodically, the period appearing persistently, even though masked by a series of overlapping effects, in all the magnetic and meteorological elements heretofore examined. The curves of relative numbers obtained from the study of the residuals in this period, are such as to give much confidence in the physical theories involved. They were derived by massing together many periods, the data embracing all regions of the northern hemisphere, roughly in the preliminary investigation, and were liable to some critical reservations as falling a little short of convincing evidence of the hypothesis. What is lacking is the strictly individual correlation of the solar cause and the terrestrial effect, day by day, and persistently through long ranges of time, such as is seen in gravitational, or in equatorial radiation phenomena. It was not easy to detect the precise way to do this, in the midst of the enormous mass of magnetic and meteorological observations, but such a method has at last been discovered, and the purpose of this paper is to explain the same.

Current Meteorological Theories. Ferrel's analysis of the meteorological phenomena of the atmosphere may be divided into two parts, namely the general motions and the local motions. To obtain the distribution of pressure arising from the general motion, two principles were developed, the first embodying the application of Euler's equations for relative motion, and the second assuming that difference of density arising from difference of temperature is the efficient cause of such motion. His working equations after final transformation have a pressure term on one side, and four terms on the other, expressing inertia, deflec-

tion, friction, and temperature gradient. The comparison of observation and computation assures us that this is substantially the system which exists in nature, although the broad features alone are brought out, the inclusion of details being rather unsatisfactory. It is observed that Ferris formed his equation as a sum of terms, rather than as a product, which the nature of the problem would more plausibly suggest; and that he secured his approximate results by suppressing certain terms as inertia and friction under specified conditions. He was perfectly clear that the motion was due to difference of temperature, and for the general motions took that thermal fall which is observed to exist between the equatorial and the polar regions, arising from the solar equatorial radiation, now called the electro-magnetic radiation. He says in the opening sentence of the chapter on the theory of cyclones, Coast and Geodetic Survey Report, "in the general motions of the atmosphere, the disturbing cause is the difference of density, arising mostly from the difference of temperature between the equatorial and polar regions of the globe." The working out of this idea on the hemisphere fixed in his mind clearly the formation of the polar cyclone surrounded by the tropical anticyclone, the two attended by belts of maximum and minumum pressure with the consequent zones of calms.

The persistence of this mental picture is evident in all the work that followed regarding small or local disturbances, and everything is bent a little, refracted to this point of view. In the next sentence Ferrel says, "in the ordinary, cyclonic disturbances of the atmosphere the causes are similar but more local, and consist in a difference of density arising mostly from a difference of temperature between some central area and the external surrounding parts of the atmosphere." This dominant idea has, I believe, proved fatal to Ferrel's successful development of his sound fundamental principles, and has greatly influenced many students to travel a road whose end has never been found. The set of papers in the Coast and Geodetic Survey Report is undoubtedly his most scientific production, for this reason, that he works out his results from the general equations strictly, merely assuming this difference of density between a central area and the region surrounding it. He was at the time evidently unable to satisfactorily account for the energy implied in the temperature difference required to do the work observed in the motions of the cyclones and anticyclones. Many expressions occur to this effect here and there by the way of acknowledging the want of this knowledge, or criticising such theories as were offered to

account for it. For instance, p. 183, "if for any reason there is kept up a continued interchange of air between the central and exterior part"; p. 201, "the condensation of aqueous vapor plays an important part in cyclonic disturbancess but is by no means either a primary or a principal cause of cyclones;" in Waldo's edition of Ferrel's earlier paper, p. 39, "the theory which attributes the whole of the barometrical oscillations to the rarefaction of the atmosphere produced by the condensation of vapor in the formation of clouds and rain cannot be maintained;" on p. 239 C. and G. Report he quotes Loomis as follows, "rainfall is not essential to the formation of areas of low barometer, and is not the principal cause of their formation or of their progressive motion," and remarks "this is strictly in accordance with the theory."

However, being hard pushed to find a cause for his central area temperature in cyclones, he gradually weakened from this position, endorsed Espy's condensation theory of development of latent heat by formation of clouds and rain, and in his last year could write in *Science*, December 19, 1890, "all this has been done in the condensation theory of cyclones, with results so satisfactory as to scarcely leave a doubt as to the truth of the whole theory." This was in reply to Hann's revolt against the sufficiency of this cause to produce the cyclones that were observed, who took the ground that these local gyrations are only subordinate parts in the general circulation which depend upon the effects of equatorial radiation alone, and are independent of any local cause. Hann even went so far as to conclude that "the motion of the atmosphere is not a product of the temperature, but is in spite of it; the temperature is a product of the motion," *Science*, May 30, 1890. Ferrel was loyal to the theory that temperature difference causes the motion always and everywhere, and Hann in adopting the inverse proposition has surely erred against first principles. All this would be a mere expression of opinion on my part, if it were not in my power to indicate an efficient source of the local temperature difference, which is just now the real stumbling block in the way of the advance of Meteorology as a science. Before coming to this point it is necessary to inspect carefully the facts of nature as displayed day by day on the weather maps.

A new interpretation of the weather maps of the United States. On page 223, C. and G. S. Report, Captain Toynbee is quoted as follows: "cyclone winds are formed, as it were, in the hollows of pressure, in which case the ridges traveled with cyclones and formed, as it were, part of them. In many cases the ridges, with

corresponding winds, extended over many more degrees of latitude than the cyclones themselves." Ferrel says of this, "it would be difficult to explain, upon any known principles, the existence of mere longitudinal ridges of high pressure; but that there is such a ridge surrounding every cyclone we have seen is strictly in accordance with well-known mechanical principles as developed in the theory of cyclones." I must admit freely that I am unable to see in the daily weather maps that formation as fundamental which Ferrel and meteorologists generally assume to be the primary state. The usual order or sequence of the phenomena has been regarded as follows, pressure, temperature, precipitation, the cyclonic and anticyclonic system due to unknown causes modifying the normal isothermals and the precipitation. I propose to see in temperature differences, arranged in ridges or waves, the true cause of the observed pressures and the antecedent of the precipitation. It is therefore necessary to account for cold and warm temperature waves passing over the United States.

It will be desirable to refer to easily accessible maps, in order to come within the proper limits of this paper. In Dunwoody's admirable charts giving a summary of the international meteorological observations, 1878 to 1887, the maps of normal pressure and temperature, with prevailing winds, for each month of the year are here employed. A line was traced along the crest of the maximum pressure, and for November its course is as follows; over the Atlantic in latitude 30°, then over north Africa, south Italy, north Black and Caspian Seas, central Siberia, Corea, Pacific Ocean near latitude 30°, central California, Colorado, Kansas, Tennessee, Carolina; there is also an atmospheric shunt, passing from central Siberia near the geographic and the magnetic poles, through British America, and Alberta, to Colorado. Thus Ferrel's polar cyclone is broken up into two permanent cyclones, in the North Atlantic and the North Pacific respectively, by the influence of the continental masses upon the temperature and the dependent densities of the air. This shunt persists from September to June inclusive, in nearly the same position, though oscillating across the polar regions with the season to some extent, but during July and August is reduced to a short spur projecting into British Columbia. Within the two cyclonic ridges the winds circulate anti-clockwise, outside the principle circuit they circulate clockwise, diverging from the central calm of maximum pressure, as Ferrel's theory of general motions demands. This outflow, in conjunction with the right handed deflecting

force due to the rotation of the Earth, deepens the permanent cyclones, heightens the maximum pressures, and lowers the equatorial pressure belt. The passage of the winds past each other in opposite directions tends towards local gyrations, which all drift eastward with the prevailing component in middle latitudes. All this depends simply upon the difference between polar and equatorial temperature, and is fully in accordance with the views of Ferrel and the latest expressions by Dr. Hann.

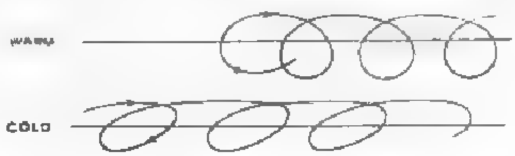
It is, however, necessary to go a step further to pass out of this generalized condition of affairs, into the specific individual case of a given cyclone. In order to fix ideas, I will describe a typical map, Thursday, P. M., November 16, 1893, remarking that the system indicated prevails from day to day, and whenever obscured by excessive action of certain components tends to return to this normal type. On examining the wind directions, it is seen that there are two curved lines extending from northeast to southwest across the United States towards which the winds are primarily directed. The first passes through Minnesota, Nebraska, Kansas, Oklahoma, New Mexico, near Duluth, Sioux City, Dodge City, El Paso; the other through western New York, western Pennsylvania, West Virginia, east Kentucky, east Tennessee, near Atlanta and Mobile. West of curve number one, the region is cooler; between the two, the region is warmer, and east of number two, it is cooler. Thus a warm section lies between two cooler. It will be observed that these division curves are about perpendicular to the crest of the maximum pressure; by following such curves from day to day it will be found that they drift across the United States, maintaining their general distance apart, until signs of disintegration set in. On the average the curve which cuts the maximum crest at Cheyenne is after 24 hours near Dodge City, after 48 hours near Oklahoma City, after 72 hours near Nashville, and after 96 hours just over the Atlantic coast. This represents the average eastward storm drift for October. Along these curves usually lies a region of calms or very variable winds, the existence of these often serving to locate the dividing curves. They are strictly associated with the cyclonic and anticyclonic pressures so well known.

The formation of these low and high pressure areas is the result of the existence of the warm or cold sections of waves lying athwart the maximum crest. Let us assume for a moment that such temperature differences exist, amounting to 20°-30° F. or even more, and trace the course of the winds. From first principles the warm and cold masses will be impelled towards each

other, because of the action of gravitation on media of differing density. They will tend to encounter along or near the ridge of greatest temperature variation. Take the eastward moving cold air first. North of the maximum ridge the surface component from the general motion is eastward, and there is also the northward component due to the normal temperature gradient, which maintains the ridge over the outflow on the sides, so that the winds flow in a northeasterly curve, even overcoming the right handed deflecting force of rotation; the result is seen over the Dakotas and New England as feeders to the cyclones there. Upon the maximum crest the lateral components are very small, and the air flows S. E. almost squarely up to the dividing curve, as seen in Nebraska, or New Jersey, and much better on the map for the following morning, Nov. 17, 8 A. M., Friday, where the system has moved in more nearly central to the map. South of the maximum crest, the south lateral component is met by the west general component which acts locally in the same direction as the right handed deflecting force of rotation, all three conspiring to produce the observed anti-cyclonic gyration.

Next observe the course of the warm winds flowing towards the northwest to meet the cool air beyond the ridge. There is the northwest temperature gradient, the right-handed deflecting force, and the general eastward surface component of the middle latitude, all of which act to turn the direction rapidly towards the right, as the prevailing arrows plainly show. There is no splitting to the left and the right of the maximum crest, as in the east flowing cool air, but all goes smoothly from S. E., more and more curving to the north. Along the line of greatest temperature change with cold air to the west and warm air to the right of it, the gyrating cyclone is formed, the couple existing from the system of causes thus described. Likewise, along the next ridge, with cold to the east and warm to the west, and often to the south of the maximum crest, the anti-cyclone is produced. It must be remembered that if the warm and the cold waves are formed, the flow will be both east and west from each successive section, with the view to produce temperature equilibrium. The discontinuous effects of large masses of air is such as to enable these waves to persist many days, possibly enough to go from Alberta, across the United States to Europe. A corollary remark is that the storm track along the north United States seems to be the effect of the general circulation to restore the permanent polar low belt which is interrupted by the continent. Another is that tornadoes and hurricanes are due to precisely the

same cause, namely the juxtaposition of masses of air having great temperature differences. The debris of tornadoes shows precisely the same distribution of air as that pointed out above; their tracks being along the course of the dividing line. Hence, the forward gyration, due to the feeding from the south and southeast sides, combined with the general eastward motion, gives them the apparent northeasterly curved path, and locates them on the south side of the cyclonic depressions. It is necessary not to extend this explanation further at this time, in order to arrive at the final step in the argument. This system gives us a surface phenomenon almost exclusively, the cyclonic outflow at a moderate altitude being caught up with the more rapid easterly component; and similarly for the anti-cyclone. The total effect of the upper and the lower circulation is that the warm and



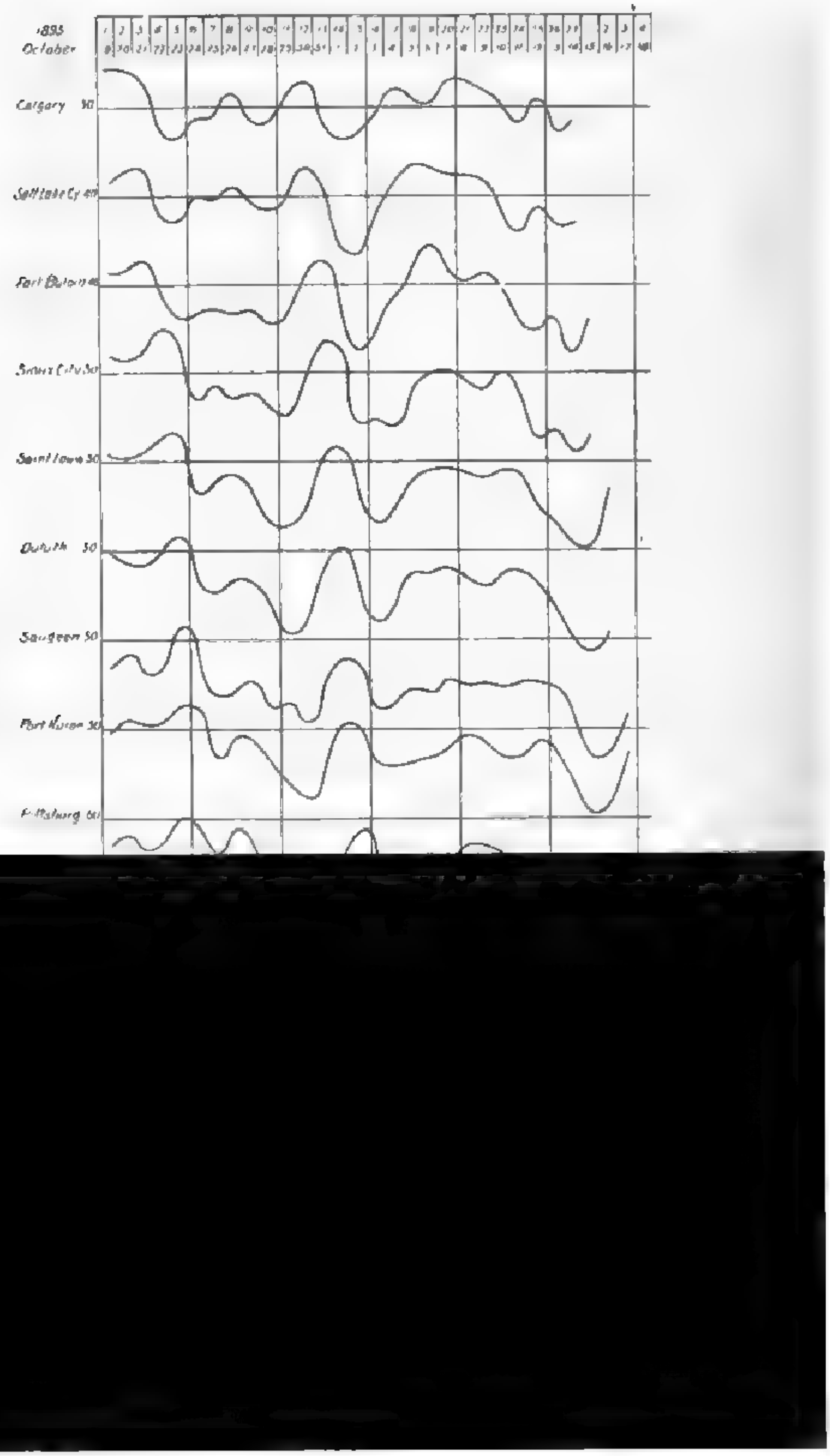
the cold layers are rolled over and over while they advance eastward, the greatest interchange of temperature taking place near the surface of the Earth at the line of contact Thus the eastward high component may readily deposit warm masses over cold, or cold over warm, as the case may be, according to the kind of air lying to the west of the lower mass in question. So far as I can see all goes on in conformity to Ferrel's principles, provided the warm and cold waves can be produced in the proper sequence, and referred to an efficient cause in nature. This will

The group stations are, Northfield, Washington, Key West, New Orleans, Corpus Christi, Pittsburg, Saugeen, Port Huron, Duluth, St. Louis, Sioux City, Fort Buford, Salt Lake City, Tucson, Roseburg, Yuma, Calgary including the Manitoba and Alberta Stations. A long roll of millimeter paper was laid out, with one centimeter per day, and the stations in nearly reversed order beginning west and working east, Y, R, C, S. L. C, F. B, S. C, T, C. C, S. L, D, S, P. H, P, N. O, K. W, W, N, so that a transverse wave will pass along certain groups in succession. The A. M. and P. M. mean temperatures for each group are then plotted, the dots joined, and the broken line allowed to grow. Beginning with summer, where the amplitudes are not wide and the form more or less featureless, as the season advances, marked typical formations appeared which persist, and which were recognized as those of the curve of magnetic intensity of the coronal field, whose derivation has been described in the papers mentioned above. Over the current calendar dates were placed the corresponding numbers of the magnetic ephemeris. The dates equivalent to the first day of the 26.68 day period, are for the present purpose: 1893, Aug. 27, Sept. 22, Oct. 19, Nov. 15, Dec. 11; 1894, Jan. 7, Feb. 3, and so on, adding 26.68 days for the interval. After two or three periods were thus developed it appeared that the magnetic curve, placed strictly according to the ephemeris, matched with the temperature curve at the northwestern districts, but that a lag took place in the districts further east, as the temperature sequences travelled along the highway of the maximum pressure. The lag is as follows, for October,

for Yuma, Roseburg, Calgary, Salt Lake City,	+ 0 days.
Fort Buford, Sioux City, Tucson,	+ 1 day.
Corpus Christi, St. Louis, Duluth,	+ 2 days.
Saugeen, Port Huron, Pittsburg,	+ 3 days.
New Orleans, Key West, Washington, Northfield,	+ 4 days.

The vigor of the temperature curves is greater on the north than on the south side of the axis of high normal pressure, since the action of the equatorial electro-magnetic field tends to diminish the distinctive action of the polar magnetic field. The accompanying diagram of curves, for the north districts, shows the progression of the temperature wave across the United States. The ordinate for the day is the mean 24-hour temperature nearly, being actually the mean of the 8 A. M. and 8 P. M. observations. The scale is 1 m.m. = 2° F., so that the fluctuations include all the temperature variations with which the problem of meteor-

Variations of Temperature in the United States



Temperatures of the United States by Districts.

North.

(From Oct. 19, to Nov. 14, 1893.)

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Calgary.	40	40	37	34	31	27	28	34	26	26	33	37	28	20	23
Salt Lake City	44	46	48	34	33	40	39	42	38	36	39	47	42	26	23
Fort Buford	45	47	37	31	32	33	32	33	29	32	42	47	32	21	29
Sioux City	54	57	63	57	42	46	43	44	40	38	48	59	56	36	[illegible]
St. Louis	53	57	57	41	44	46	41	33	32	38	52	54	41	33	38
Duluth	46	50	54	40	39	42	41	34	27	32	45	51	40	31	36
Saugeen	43	54	44	34	30	[illegible]	31	32	27	37	44	43	30	34	36
Port Huron	53	57	55	42	48	46	40	35	31	40	51	56	40	40	42
Pittsburg	54	60	58	51	57	51	43	37	38	42	50	57	48	48	46
Washington	59	60	55	48	57	52	42	38	38	44	50	54	43	42	46
Northfield	53	54	49	42	46	49	41	33	32	38	44	47	37	36	37
	49.5	51.9	50.6	40.4	41.3	42.7	38.3	35.9	32.5	36.6	45.3	49.6	39.7	33.4	35.7
	+ 9.3	+ 12.7	+ 10.4	+ 0.2	+ 1.1	+ 2.5	— 1.9	— 4.3	— 7.7	— 3.6	+ 5.1	+ 9.4	— 0.5	— 6.8	— 4.5
	+ 19	+ 25	+ 21	0	+ 2	+ 5	— 4	— 9	— 15	— 7	+ 10	+ 19	— 1	— 14	— 9

South.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Yuma	56	57	55	55	58	57	57	57	61	56	57	56	54	52	54
Roseburg	48	50	48	44	43	40	45	49	49	49	48	50	40	34	38
Tucson	53	55	54	51	46	50	49	46	44	46	51	53	50	36	40
CorpusChristi	63	64	67	67	68	66	55	57	55	56	61	66	67	61	58
New Orleans	62	64	65	66	63	58	53	50	51	55	[illegible]	65	64	58	54
Key West	66	66	66	66	69	63	55	50	53	55	60	63	65	63	59
	58	59	59	60	58	56	52	51	52	53	57	59	57	51	51
	+ 4	+ 5	+ 5	+ 6	+ 4	+ 2	— 2	— 3	— 2	— 1	+ 3	+ 5	+ 3	— 3	— 3
	+ 8	+ 10	+ 10	+ 12	+ 8	+ 4	— 4	— 6	— 4	— 2	+ 6	+ 10	+ 6	— 6	— 6

North.

	16	17	18	19	20	21	22	23	24	25	26	27		
Calgary	28	35	33	31	37	30	34	31	25	32	23	30		
Salt Lake City	35	44	40	48	46	46	43	35	29	37	32	33		
Fort Buford	37	42	51	41	41	43	30	29	28	30	21	30		-1 day
Sioux City	35	47	50	50	48	45	50	42	31	34	28	33		—1 day
St Louis	45	48	48	47	47	48	45	37	33	27	26	42		-2 days
Duluth	44	44	45	42	40	44	44	40	34	20	22	28		—2 days
Saugeen	35	30	37	38	37	39	35	37	20	17	19	29		-3 days
Port Huron	43	45	48	45	42	43	47	41	34	20	30	43		—3 days
Pittsburg	47	49	53	52	50	47	47	46	40	20	33	48		—3 days
Washington	50	40	47	39	39	43	49	45	36	32	43	44		—4 days
Northfield	33	38	34	27	33	33	37	40	38	29	32	40		—4 days
	39.3	43.4	45.0	42.1	41.7	42.3	43.3	35.5	32.3	28.7	28.1	36.1	40.2	
	— 0.0	+ 3.2	+ 4.8	+ 1.9	+ 1.5	+ 2.3	+ 2.8	— 1.7	— 7.9	— 11.5	— 12.1	— 4.1		
	— 2	+ 6	+ 10	+ 4	+ 3	+ 5	+ 6	— 3	— 10	— 23	— 24	— 8		

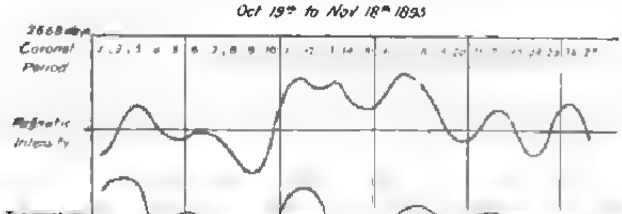
South.

	16	17	18	19	20	21	22	23	24	25	26	27		
Yuma	56	55	54	52	55	54	55	57	58	57	56	59		
Roseburg	46	50	53	49	53	49	45	46	45	40	43	42		
Tucson	42	44	47	47	40	43	40	32	34	36	30	40		—1 day
CorpusChristi	55	55	59	59	57	56	63	58	49	45	43	56		—2 days
New Orleans	55	56	56	58	56	50	56	50	43	44	57	62		—4 days
Key West	57	57	59	57	55	56	58	58	51	45	52	57		—4 days
	52	53	55	54	53	52	52	50	47	46	45	53	54	
	— 2	— 1	+ 1	0	— 1	— 2	— 2	— 4	— 7	— 5	— 6	— 1		
	— 4	— 2	+ 2	0	— 2	— 4	— 4	— 8	— 14	— 10	— 12	2		

Now by shifting the daily temperatures backward, by 1, 2, 3, or 4 days, so that the same phases of wave temperature fall into their own columns, the mean effect can be found for each wave, and thus the masking effect of successive waves at individual stations can be eliminated. It thus is clear that the application of the magnetic ephemeris and the lag interval to meteorological data, is the proper method to unmask the superposed forces that have hitherto concealed the true functional relations. The tracing of the lag of the curve across the Atlantic has not been undertaken; indeed it will be necessary to study the whole hemisphere in this graphical way, before the subject can be fully mapped out in its physical topography. The table of temperatures gives the actual readings, the lower districts all being shifted to the left as indicated. The stations are separated into north and south groups, divided by the belt of maximum pressure, as nearly as our data permit.

The temperature residuals are taken out from the mean of the 27 days, and multiplied by the factor 2, in case of plotting on half-millimeter paper. As they stand in the reproduction in Astronomy and Astro-Physics, 5 m.m. = 10° F.

Comparison of the Variations of Magnetic Intensity with the Temperatures of the United States Oct 19th to Nov 18th 1893



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units, and the scale of variation is 5 m.m. = 0.000020 C.G.S., so that the range of fluctuation is about 0.000050 C.G.S., the amplitude proper being about one-fourth of the total mean intensity. Underneath this is placed the two curves of temperature, one for the districts north, and the other for those south of the summit of the pressure crest. They represent the effect of one rotation of the Sun on its axis, carrying this variable magnetic field past the Earth, upon the temperatures of the United States. The harmony of the curves speaks for itself significantly. The residuals or relative numbers obtained from the use of about 200 revolutions of the Sun, of course eliminated the masking effects of superposition, while the residuals were much cut down in size. No evidence could be more striking than this, that the direct action of the solar polar magnetic field is responsible for the temperature waves which build up the cyclonic and anticyclonic system of pressures and winds in the United States.

The conclusion now follows simply from this set of premises. The solar magnetic field represents a type of radiant energy, probably circular or spiral rotation of the ether, which surrounds the Sun on all sides, but of variable strength in certain solar longitudes. In other words the earth passes through a series of hotter and colder regions as the Sun turns on its axis. One day is the equivalent of about 10,000,000 miles. Since the form of energy is magnetic, which of course means a special form of ether motion, this energy approaching the earth, itself a magnetic body capable of conducting the lines of force better in some directions than in others, is concentrated or focused in the magnetic ovals surrounding the magnetic and geographical poles. The form of the regions of concentration came out fully in my study of the equatorial radiant field. Thus the atmosphere around the polar regions is intermittently heated or cooled according as more or less of this polar energy falls upon it, the temperature being a direct function of the radiant energy.

It will be remembered that the atmospheric shunt crossing the polar regions passes directly through this area, which is thus subject to varying absorption of radiant energy and is therefore alternately heated and cooled, strictly in response to the solar field. This air is transported by the general motions of the atmosphere around the permanent cyclones, on the east side if we look from the United States, downwards into the well known storm belt, where the transverse temperature waves pursue the path already described; on the west side of the polar shunt the circulation is towards the East Asian coast and Pacific ocean, where a similar

series of temperature waves probably exists. The larger masses will, without doubt, be carried into the United States owing to the fact that the magnetic pole on the American continent disposes the radiation more on that side of the polar regions than on the other.

The case may rest here for the present while the consequent mathematical developments are being worked out. Only one period has been described, but a dozen lay before me on the sheet; and the constant recurrence of this solar curve during 15 years in the pressures of the northern hemisphere, makes it safe to accept this solar polar field as the true origin of the energy of Highs and Lows, so long sought in practical meteorology. Instead of being an obscure term in meteorology, magnetic energy comes to the front as the primary factor in all the weather conditions, certainly of the United States. The strict response of temperatures, upon which all else depends, to the peculiar variations of the magnetic field, on individual dates, makes it not only possible to forecast the weather conditions specifically, by means of the ephemeris and the lag, but it is the key to the reading of the weather maps. The intensities of the successive waves can be read off, the form of a wave can be located to the width of a single state, and from the waves the wind system and the accompanying cyclones and anticyclones can be laid down very closely. Of course the ragged edge of temperatures along the wave fronts, it will be hard to determine by any method now known, but this penumbra is limited and wholly unimportant. It will be necessary to study the lag sequence throughout a number of years to obtain its characteristics and, at the same time it will be important to learn the effect of the change of the Sun in declination. The waves flat out in summer, chiefly because the polar shunt disappears, and in the winter the circulation may become too violent to leave the waves in their integrity. The reversal of the whole system of temperatures is found to take place in December, when the Earth passes from the positive to the negative hemisphere of the Sun, and the curve must be inverted to follow the temperature variations. This can all be determined by some further experience. It is also to be observed that the areas of precipitation are closely associated with the lines marking the contact of the warm and cold wave, and that this is the product of condensation by cooling of mixtures, and not by cooling through dynamic expansion. Many of the features of physical meteorolgy will thus find in this system their true criticism. I have not yet drawn out the function of the magnetic field and tempera-

ture, because the former was taken from European data, which is too far removed from the concentration area. The polar stations of 1882-1883 will probably supply the requisite material

It should be noted that there may be found in this polar radiation the true cause of the great ranges of temperature in the polar regions, known in the Glacial Epochs. If the Sun through long periods of time changes the quantity of its magnetic output, and it will if it is a variable star, the effect would be to cool the poles of the earth, if it were less, and to heat them if it were more, a series of changes that Geology thinks has taken place. It is only necessary to add a long geologic period to the 11 year and the 27 day periods of the Sun. Also it may be noticed that this perodic temperature fluctuation which is followed by variations in the wind circulation, and to some extent in that of the water, may have something to do with the variations of terrestial latitudes observed to have been taking place. Across the atmospheric shunt more or less air will be drawn according as the velocity in the general cyclonic circuits is increased or diminished and this will be accompanied by the formation or melting of snow and ice, which may affect the movement of inertia of the Earth to some extent. It is seen, as stated heretofore in the preceding paper, that the intensity of the atmospheric circulation of the hemisphere increases or diminishes, as shown by the number of Highs and Lows on the synoptic charts, with the variations of the solar period. Chandler obtains 427 days as the period of the variation of latitude. Now $16 \times 26.68 = 426.88$, that is, sixteen solar periods are equal to the probable latitude period. This may be a coincidence and is not insisted upon, but it can readily be supposed that the great range of temperature on the polar regions, may have an important effect on the accumulation of ice and snow, in some short period, as well as in a very long glacial period. Correlative factors are at work on the Southern hemisphere, but I have no experience with them.

It goes now without saying that the development of magnetic observations in connection with meteorological stations will be necessary to derive a full advantage of this theory. Much can be done with the mean magnetic curve in forecasting the weather at long range or short range; but I advocate the extension of the European system of magnetic observation, or their practical equivalent, to the American continent. I thought I observed that individual observatories were sensitive to variations of the magnetic field a thousand miles away but that can be determined by experience. The subjects of magnetic storms, auroral

displays, earth currents, are evidently subordinate manifestations of this solar polar magnetic field.

There is now certainly great encouragement to the meteorologist to review the rich material at his disposal, with the object of making a scientific analysis of the subject, which has not yet passed beyond the empirical stage. There will always be a nebulous fringe of uncertainty on weather forecasts, owing to the fact that we deal with a rapidly moving fluid, but I believe that a great improvement in weather predictions, both at long and short range, is in sight if not at the doors. There are now several steps to be taken to improve meteorology.

1. Substitute the solar magnetic ephemeris for the common calendar.
2. Employ the mean magnetic curve as a guide to interpret all variations.
3. Determine at each station the lag of the wave from the polar region.
4. Determine the law of the magnetic reversals of temperature.
5. Form the equations of local motions, and work out the physical constants.

A NEW STAR IN NORMA.*

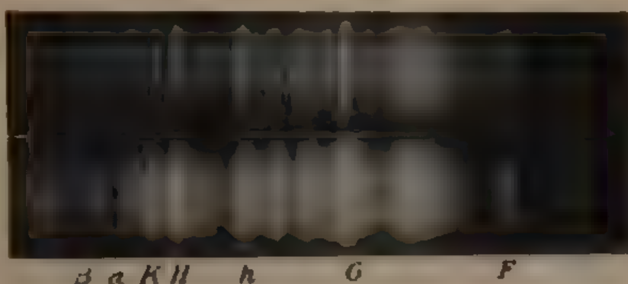
EDWARD C. PICKERING.

A new star appeared in the constellation Norma during last summer. It was discovered by Mrs. M. Fleming on October 26, when examining a photograph of the spectra of the stars in its vicinity. The photograph was taken on July 10, 1893, at the Arequipa station of this Observatory by Professor Solon I. Bailey. The spectrum appears to be identical with that of the new star which appeared in Auriga in December, 1891. Comparing the spectra of the two stars taken with nearly the same dispersion, about a dozen lines are visible in each and are identical in wave-length. The hydrogen line F, which is bright in both stars, is more intense in the star in Norma than in that in Auriga. It is, in fact, more intense in the former star than any other line in the spectrum, while the G line is generally the strongest in Nova Aurigæ. A photograph taken June 21, 1893, showed the spectra of stars of the 10th magnitude, but no trace of the new

* Communicated by the author.

PLATE IV.

Nova Aurigæ, February 6 1892



Nova Normæ July 10 1893

Astronomy and Astro-Physics, January 1894

star was visible upon this plate, although it covered the same region and was in other respects like that taken on July 10. Photographic charts of the same region taken on June 6, June 10, July 21, 1889; May 16, May 16, June 10, June 23, June 23, 1891; May 7, and May 27, 1893, show no image of this object although stars of the fourteenth magnitude are visible upon some of them. We may therefore conclude that the star appeared within ten days of the first of July and that previously it was either invisible, or extremely faint. The position was found by comparing the ends of the hydrogen lines, G and *h*, which are bright in this star, with the corresponding dark lines in the adjacent stars A.G.C. 20940 and 21006. They give the approximate mean position for 1900 in R. A. $15^h\ 22^m\ 12^s$; Dec. $-\ 50^\circ\ 13'.8$. A more accurate position can be found if photographic charts can be taken showing this star. Professor Bailey has been notified of this discovery and if the star is still bright enough he will doubtless obtain photographs showing its position and spectrum.

The similarity of the spectra of these two new stars is interesting, first, since it has proved a means of discovering one of these objects, and secondly, because if confirmed by other new stars it will indicate that they belong to a distinct class resembling each other in composition or physical condition. The star was approximately of the seventh magnitude photographically on July 10 since it was about equal to A.G C. 20910 magnitude 6.9 whose spectrum is of the second type The nearest catalogue stars are A.G C 20940, magn. 8, which has a spectrum of the first type, and A.G.C. 20926, magn. $8\frac{3}{4}$, which has a spectrum of the second type. The new star lies nearly midway between these two.

CAMBRIDGE, Mass., U. S. A.
November 9, 1893.

MR. LANGLEY'S RECENT PROGRESS IN BOLOMETER WORK AT THE SMITHSONIAN ASTRO-PHYSICAL OBSERVATORY.*

All the best work in this direction has hitherto been done by micrometric measurements of the position in which the bolometer indicates heat or cold, a method which in this application, is so slow, that between two and three years of assiduous labor were

* Abstract of a communication made to the Philosophical Society of Washington, May 27, 1893.

required formerly, to make a curve showing only the leading absorption lines of the infra-red spectrum.

The first and most important feature of the new method consists in an arrangement by which the means of visual observations, or micrometric measures of the galvanometer deflections, is done away with, and the record of the movements of the needle as the bolometer thread travels through the spectrum, is made photographically, the result being a curve showing the variations in heat at every point of the invisible spectrum corresponding to the (invisible) absorption lines; and this process has been found so incomparably more rapid than the former, that more lines can be located by it in single day than by the old process in a year's assiduous work.

The new process implies the use of a more delicate bolometer and galvanometer than the old one, and this has been attained to such a degree that the delicacy of the new apparatus is about a hundred-fold greater than the already delicate means used by Mr. Langley, in the researches previously published.

To secure the desired accuracy, it has also been necessary to employ more massive as well as more delicate apparatus, and to pay great attention to instrumental details.

The new, and extremely large and accurate spectro-bolometer, which carries the prism or grating, is provided with azimuth circle, reading to 5 seconds of arc, which can by application of special clock-work to its tangent screw, be given a slow and perfectly uniform motion of rotation, such that the successive portions of the spectrum fall in uniform sequence upon the bolometer thread. The utmost care has been taken to mount all the portions of the apparatus and especially the delicate galvanometer, so that accidental disturbances are, as far as possible, eliminated.

Owing to the accurate clock-work which has just been described as moving the spectrum past the bolometer thread, we can be sure that the movement of the galvanometer, (which records the contact of this thread with any invisible line, and the cold produced thereby) corresponds at any given second of time, to the passage before the thread of some definite portion of the spectrum, which though wholly invisible, can be identified at once, and its wave-length instantly determined by the corresponding reading of the circle, taken in connection with the well determined constants of the great rock salt prism, by which the invisible spectrum is formed. To preserve an automatic record, of this correspondence between the time and amount of the galvanometer deflection, and the coincident circle reading, a sheet of sensitized pa-

per or glass is made to move by the same clock-work vertically before the galvanometer, and a beam of light reflected from its minute mirror, (which is but 2 mm. in diameter,) falls upon the moving sensitive surface, thus recording automatically the time and extent of the deflection. In other words, owing to the synchronism of the movements of the circle, the galvanometer, and the recording films, the abscissa and ordinate of any part of the photographic curve correctly represent the position and intensity of the absorption lines of the invisible spectrum. Such curves were exhibited by Mr. Langley, showing every detail of the thermal variations from the upper end of the visible spectrum down to a wave-length of about 7 microns, that is, through a space nearly 15 times as great as the spectrum known to Sir Isaac Newton.

Each of these curves was obtained automatically in less than a day's time, but in spite of this, each presents a multitude of details such as would have escaped the most laborious personal observations, and by preserving for future study, many such which even an experienced investigator might have overlooked, or if seen, have omitted as being probably due to accidental or unimportant sources of disturbance, these curves have wonderfully increased the power and range of our investigations. By securing so complete a result in so short a time, Mr. Langley has rendered it possible and easy for us to investigate the absorption lines due to the earth's atmosphere; and that too, in all parts of the spectrum, since we have only to compare among themselves, the bolographs made during the morning, midday and evening hours respectively. It may therefore be said that both the chemical constitution and the thermal absorbing power of the atmosphere as a whole, are now brought within the range of scientific study; and this culmination of Langley's work must be hailed as being as important to meteorology as it is to the solar physics; for since the greater proportion of all the new lines mapped, are probably due to absorption in our own atmosphere, and represent with hitherto unheard of precision the localities and extent of that absorption as exhibited in the invisible spectrum, the effect is nearly the same as if he had discovered some chemical sensitive to the whole ultra-red portion of the spectrum, in the way that silver salts are to the vibrations of the violet end.

Finally through some automatic process which he did not explain, but which would seem to be a simple application of the Woodbury, or gelatine process in photogravure, this bolograph record of the infra-red spectrum is converted into a linear spec-

trum, showing the invisible absorption groups as the lines and bands, so that the final presentation of the invisible heat spectrum resembles, on a smaller scale, the magnificent photographs of the visible solar spectrum that have recently been published by Rowland, and from these, any incidental variations if such exist, are eliminated by a process resembling that of composite photography, but again entirely automatic, and independently of the personality of the observer; the final sheets are thus being prepared for publication,—a publication, however, which is expected to involve another year of labor before entire completion.

Mr. Langley mentioned the obligations of the Observatory to the late Dr. Kidder, and to the generosity of Mr. Alexander Graham Bell, and alluded to the assistance he had received from Messers. Hutchins and Hallock, and especially from Mr. F. L. O. Wadsworth.

THE OBJECT-GLASS GRATING.*

L. E. JEWELL.

During the last two years, experiments in another line of study, have led me to the consideration of methods by which a photographic grating adapted to large telescopes might be constructed. The object sought was not a substitute for either plane or concave gratings ruled upon speculum metal, but a satisfactory grating for obtaining the spectra of stars, where only a fair amount of dispersion is desired, but intensity of light is of the utmost importance. At present the object-glass prism is used, but it is subject to some serious defects and the dispersion obtained is small. A photographic object-glass grating would answer the purpose admirably if certain difficulties could be overcome. Without considering at length these difficulties let us see how the desired results may best be secured.

The plan now proposed will, I believe, be found to be the most satisfactory. It is to photograph a series of images of a long narrow slit, as perfect as one can be made. This can best be done by having the slit and photographic lens fixed, and the photographic plate movable, by placing the latter upon the carriage of a dividing engine. The carriage is moved along by means of the revolution of a perfect screw, which is revolved through a certain angle, and then the exposure made. This process is re-

* Communicated by the author.

peated until the desired number of exposures have been made, when the plate is developed and fixed, the result being a photographic grating.

Such being the general plan of operations, we will now consider the details. The first thing necessary is as perfect a slit as it is possible to secure. It must have no irregularities, and should be a perfectly straight line. I believe the best results will be secured by photographing a wire stretched against a uniformly bright background. It is absolutely necessary to have a photographic plate that is practically structureless, so as to avoid halation, and to so develop it that the image of the wire shall be perfectly clear, while the background is as dark as it is possible to make it. Probably some form of collodion emulsion would give the best result. Whatever kind of film is used, it must be perfectly clear and uniform, or it will be useless for delicate work. In addition the film must be hard and firm, and possibly be capable of taking a polish without destruction of the image. This might be secured by a second coating of very hard collodion which could be polished, when the grating is to be placed in front of an object-glass. It may be found best even with a structureless plate to somewhat underexpose and then intensify it after development. It will also be found best to have the photographic plate as long as a perfectly prepared plate can be obtained, and conveniently handled; for we can then use a comparatively large wire, and thus avoid diffraction effects. It will not be necessary either to have the image of the slit so very narrow. Consequently we can make a more perfect slit, and by placing it farther from the photographic lens obtain the same width of lines in one grating that we would obtain were we to use a narrow slit closer in the lens; and the lines would also be sharper and clearer.

This matter of obtaining a perfect slit is of the utmost importance; and time occupied in making a satisfactory one would be well spent. The photographic lens should be as perfect and well suited for the purpose, as it is possible to secure, and should preferably be of long focus, and a flat field, with the utmost possible sharpness of image.

For properly spacing the distances between the lines or images of the slit, a ruling engine of similar construction to those of Professor Rowland, but larger and with some changes made necessary by the carriage holding a large photographic plate, and the automatic regulation of exposures as well as the spacing, would answer admirably.

The photographic plate which is to be used for making the grating should be of the same character as the plate used for making the slit, and should be as large as can be perfectly made, and satisfactorily handled.

The utmost efforts should be made to obtain a perfect grating of as large a size as possible, for having secured a perfect original, an indefinite number of copies can be made either of the same size or smaller.

Having decided upon the number of lines we desire the original grating to have, the width of the lines compared with the width of the spaces can be regulated by adjusting the distance of the slit, and to some extent by regulating the time of exposure. It is best to regulate the width of the lines by adjusting the distance of the slit from the lens and to make the time of exposure just what is necessary to obtain the sharpest and blackest line. In this way we can secure that width of line in relation to width of spacing, which will give the purest and most brilliant spectrum.

Having secured a satisfactory grating of as large a size as can be conveniently made, a large number of copies can be made, and if the lines and spaces are of equal width, it will not be necessary to make a negative of the original grating and then positive copies from that, for copies made directly from the original will answer perfectly, as there would be no material difference between a negative and a positive of the original grating. Also a large grating can be used for making smaller ones with the same number of lines per centimetre as the original, or a larger number. However it would probably be found desirable to make several large original gratings having different numbers of lines per centimetre, for having secured a few original gratings of this kind, they can be used for copying almost indefinitely if well taken care of, and the copies should be equal to the originals if we use as perfect a copying lens as can be secured, and have the original grating set up with a uniformly bright background behind it. We should make the copies with the same kind of photographic plates as that used for the originals, and more pains should be taken to secure optically perfect plates of glass upon which the copies are to be made, than is necessary with the original gratings.

I believe that gratings thus made will answer best for the purpose for which they were originally designed, *viz.*, for placing in front of the object-glass of a telescope, to be used in the same way as an object-glass prism. One considerable advantage of an object-glass grating over an object-glass prism would be in meas-

uring the displacements of lines caused by the motion of the luminous body in the line of sight. The grating would give a spectrum on each side of the central image of a star; consequently, displacements could be readily measured and could probably be determined with more certainty than with a slit and prism, while the object-glass prism gives us no point of reference.

However there are many other uses to which these gratings may be put. In the case of a ruled grating the grooves may be of almost any shape, they may be of an unsymmetrical shape, and instead of single grooves they may be multiple grooves, or notches, or adjacent grooves may overlap, and as a consequence of these irregularities, gratings so made may be very irregular in the distribution of light in their spectra. In fact nearly all gratings are likely to have some eccentricity of this kind, so that in the study of the distribution of light or heat in the spectrum, a ruled grating is liable to give erroneous results. This is not likely to be the case with a photographic grating made in the manner described, for the lines will be single and symmetrical and the width of the lines compared with the width of the spaces can be so regulated as to give the most brilliant spectrum without any irregularity in the distribution of the light. As to how great a dispersion may be obtained along with lines sufficiently perfect to produce a pure spectrum, that will have to be determined by experiments, as it depends upon the degree of perfection of the photographic plates used. However, the dispersion will undoubtedly be sufficient for the purpose for which this form of grating was originally designed. Whether sufficiently narrow and at the same time perfectly sharp lines with clear spaces can be produced for use in gratings of greater dispersion, depends upon the capabilities of photographic methods and this is yet to be determined.

The photographic gratings described can not well be used except when we transmit the light through them and in connection with a lens or mirror, and in case a slit is used, an additional lens is necessary. But may not a copy of the original grating be photographed upon a metallic mirror having any desired curvature? It can unquestionably be done by making the mirror in question a daguerreotype plate. As to whether or not such a grating would give a sufficiently brilliant spectrum, can best be determined by experiment. It may be objected that a daguerreotype grating will tarnish and fade. This is perfectly true and the only remedy is to make another one which can readily be done at small expense and with but little trouble. It may also be that other photographic processes will give better results. If so, a

photographic grating upon any surface may be made as readily as upon a plane surface, and this will be an advantage over a ruled grating in one respect at least; for in ruling a concave grating the grooves are not likely to be of the same form in the middle and at the sides. Another advantage would arise from the large size of the original grating, the spacing of the lines being from this cause necessarily more regular, and this advantage would be retained when the size of the grating is reduced in copying. It is very doubtful, however, if at present a photographic grating could be made that would successfully take the place of a ruled grating where large dispersion is desired.

MARIETTA, Ohio.

ON THE NEW STAR IN AURIGA.*

H. C. VOGEL.

II.

Before I proceed in the next section to a consideration of the most important hypotheses which have been advanced to account for that wonderful celestial phenomenon, the blazing forth of a new star, I have still to mention some results, particularly those of the photometric observations, which were in this case specially characteristic of the apparition.

At the time of its discovery by Dr. Anderson of Edinburg, on Jan. 24, 1892, the star was between the 5th and 6th magnitudes. From the numerous photometric observations which were made at different places after the discovery became known, it appears that between Feb. 1 and March 6 the Nova fluctuated in brightness between the 4th and 6th magnitudes. A first maximum fell between the 3d and 6th of February, a second occurred, on the 18th of February, and a third on the 2d of March. The first minimum was on Feb. 16th, a second on Feb. 23d It may be assumed that more frequent but smaller fluctuations occurred, particularly in the interval between the 3rd and the 9th of February. From the 6th of March to the 1st of April the light-curve falls off very abruptly to the 13th magnitude.

According to the observations made at the Lick Observatory, the light-curve shows a still steeper descent in the early days of

* Continued from the December number. The greater part of section II is omitted, as it consists of an abstract of observations which have already been printed in full in ASTRONOMY AND ASTRO-PHYSICS. Some remarks on the omitted portions will be found in Astro-Physical Notes.—*Tr.*

April, but falls off somewhat less rapidly from April 8th to April 26th, (the date of the last observation with the 36-inch refractor), when the brightness of the star had sunk to the 16th magnitude.

It is a matter of great interest that several photographs of that part of the heavens in which the Nova appeared were made at the Harvard College Observatory in December, 1891. The Nova does not appear on a plate taken Dec. 1, but it does appear on the next one, taken on Dec. 10th, as a star of the 5.4 magnitude. From the 10th of December, 1891, to the 20th of January, 1892, twelve photographs were taken, which show that the Nova reached a maximum of brightness (4.5 mag.) on the 20th of December.

It is to be regarded as a very fortunate circumstance that a plate of the same part of the sky, taken by Dr. Wolf in Heidelberg, falls precisely in the great gap in the Cambridge photographs. It was taken on Dec. 8, 1891, and does not contain the Nova, which must therefore have been fainter than the 9th magnitude. According to this, the outburst of the star must have taken place very suddenly.

Many determinations of the brightness of the Nova have been made photographically, and they are so far interesting that they show a more rapid diminution of light than the visual observations. In this they are in harmony with the spectroscopic observations, according to which the light fell off very rapidly in the violet, as would be expected in the spectrum of a cooling body.

On Aug. 17, 1893, the Nova was rediscovered at the Lick Observatory as a star of the 10.5 magnitude.* The star had therefore diminished in brightness in October and November, but in December of last year and the beginning of this it had again reached the 10th magnitude.

At the occasion of the Nova's rediscovery, several of the Lick Observatory astronomers observed that the appearance of the star was different from that of other stars of the same magnitude, but the Moon was near and the observations were difficult on account of the brightness of the background. With the 36-inch refractor on August 19, Barnard† found the Nova to be a nebula 3″ in diameter with a 10th magnitude star in its center. This appearance has not materially changed throughout the further observations that have been made. This brightness of the nucleus, as well as that of the nebulous envelope, has under-

* Publications of the Astronomical Society of the Pacific, Vol. IV, p. 243.
† Pub. A. S. P., p. 244; A. N. 3143.

gone fluctuations, but the diameter has remained constant. In all, sixteen observations by Barnard, from Aug. 19 to Dec. 5, have been published.

At the Pulkowa Observatory, Ring and some other astronomers observed a similar aspect of the Nova. The Nova appeared as a minute star, surrounded by a nebulous aureole.*

The photographs taken by Roberts† with his 20-inch reflector, on Oct. 3, 1892 (exposure 110 min.), and on Dec. 25, 1892 (exposure 20 min.), do not show a nebulous envelope, which shows that the nebulosity could not have been more than 21″ in diameter, this being the diameter of the star image on his first plate.

Since it cannot be assumed that so excellent an observer as Barnard could have been deceived, his observations are well worth a more careful consideration; it would surely be of the highest interest to determine with certainty whether the Nova suddenly changed into a nebula, or whether its stellar character was retained in its second appearance.

I think that I can now give a very simple explanation of the peculiar appearance of the star as seen with the great refractors.

In the refractor of the Lick Observatory, the distances between the focal planes for rays of different wave-lengths are very considerable; for example, the difference of focus for F and $H\gamma$ is, according to Campbell, 37 mm., and for $H\gamma$ and $H\delta$, 34 mm. Although the magnitude of these differences is somewhat striking, they are relatively no greater than in other visually achromatized telescopes, as may be seen by referring to my investigation of the circles of chromatic aberration in different telescope objectives.‡ The achromatism of the 36-inch objective seems to differ but little from that of the objectives by Grubb, and under this assumption the foci for rays having the wave-lengths 495 and 486 $\mu\mu$ (F) would differ respectively from the focus for λ 500 by 0.00010 and 0.00025 of the focal length, or in the case of the Lick telescope, by 1.7 mm. and 4.3 mm. Placing the circle of aberration for λ 500 = 0, computation gives for the circles of aberration of the rays λ 495 and λ 486 the values 0.09 mm. = 1″.1 and 0.23 mm. = 2″.8 respectively. Remembering now that the spectrum of the Nova at that time was discontinuous, consisting of a line at λ 500 with intensity 10, a second at λ 495 with intensity 3, and a third at λ 486 with intensity 1,§ it is evi-

* A. N., 3119.

† *Monthly Notices* R. A. S., Vol. LIII, No. 3.

‡ Monatsber. der Königl. Akad. der Wiss. zu Berlin, April, 1880, Publ. des Astrophys. Observatoriums, No. 11.

§ Publications A. S. P., Vol. IV, p. 245.

dent that with the focus adjusted on the brightest rays λ 500, a stellar point would be seen, snrrounded by a circular halo about 1″.1 in diameter, which with less magnification would blend with the star, and around that a second bluish halo about 2″.8 in diameter. In a telescope without chromatic aberration, *i. e.*, a reflector, on the other hand, all rays would be united in a single point, and the Nova would appear as a star.

The other violet lines in the spectrum of the Nova are very faint in comparison with the line at λ 500, and give such large circles of aberration that the latter, from their faintness, would be invisible. The group of lines the brightest of which (intensity 0.7) has the wave-length $463\mu\mu$, would give a circle of aberration 12″ in diameter, and the line at λ 436 (intensity 0.8) a circle 24″ in diameter. The above considerations and a note by Barnard in *A. N.* 3114 and 3118, appear to confirm my view that the observed nebulous envelopes of the star are nothing more than circles of chromatic aberration. The note reads "the nebulosity, which was pretty bright and dense, was found, by the micrometer to be 3″ in diameter. Surrounding this was a fainter glow perhaps half a minute in diameter." In the Pulkowa refractor the appearance was quite the same.*

Further confirmation of the correctness of my explanation is found in notes on the observations themselves. Barnard says; † "October 25: I do not think the nebulosity has decreased in extent. November 4: The nebulosity and nucleus are bluish white. November 18: Nebulosity dense and bluish. . . An inspection of these notes shows that in determining the brightness of the Nova in its present condition much will depend upon the telescope and magnifying power. With a low power on a telescope inadequate to show its true nature the Nova is brighter than star F. With a high power on an instrument capable of showing it well, the star itself (or more properly, nucleus) is decidedly fainter than star *F*."

With a high power the circle of aberration for λ 495 appears distinctly separated from the star; the star formed of rays of wave-length $500\mu\mu$, is estimated to be fainter than when, with a lower power, the circle and star blend together from insufficient magnification.

It appears from the following notes, moreover that the Nova at its first appearance as seen in the Lick instrument differed from

* The same explanation of the appearance of the Nova in large refractors was also given by Newall (*Nature*, Nov. 3, 1892). See also Astro-Physical Notes in this journal, February, 1893.—*Tr.*

† *A. N.* 3143.

other stars according to the prominence of the bright lines as compared with the continuous spectrum. In this case, however, the difference was less marked, because several lines occurred between C and F, and the relative intensities of the lines were different. "1892, April 4: Nova is somewhat nebulous." "Nova seems to be fuzzy at times. Is it in focus when the other stars are?"* The last note is particularly convincing, for it is evident that the focusing was difficult because the differences of brightness of lines were not very great, while in the present spectrum, in which the line λ 500 dominates, the focus can not be well adjusted except on the point where rays of this wave-length are united.

III. *Hypotheses in Regard to the Nature of the New Star.*

In spite of the inconsiderable brightness of the new star, the application of improved instrumental means, and particularly of astronomical spectrum photography, has resulted in a collection of observational material so rich that observations of all previous occurrences of the same character appear poor by comparison.

A considerable advance in our knowledge of these celestial phenomena is therefore to be expected, and in my opinion it will be found to be substantially this: that we can no longer regard the assumption of a single body as sufficient in any explanation of the occurrences which we are considering. Although in earlier cases it was possible to advance hypotheses which were sufficient to explain the imperfect observations, and particularly appearances which, owing to unfavorable circumstances, were never clearly developed (for it should not be forgotten that it is to be regarded as an especial piece of good fortune that in this case the components of the motion of the bodies in the line of sight were great enough to allow their spectral lines to be separated), the real cause of the sudden, tremendous catastrophe, to which was ascribed the outburst of incandescent gas from the interior of a partly cooled body, remained hidden in total obscurity. It is therefore easy to understand why, among the numerous attempts which have been made to explain the phenomena presented by Nova Aurigæ, the assumption of a single body occurs in only quite sporadic cases.

I believe that I am justified in passing over all attempts at explanation which rest upon such an assumption as this; for exam-

* Publications A. S. P., p. 228.

ple the suggestion that a celestial body has suddenly erupted gaseous matter from its interior with such violence (of course in a direction as nearly as possible opposite to the Sun), that the gaseous matter has been thrown off from the body, and that the two parts are separating with a relative velocity of more than 460 miles per second. I will also merely state in the words of the author himself, a hypothesis of Sidgreaves (whose excellent observations I have mentioned in a previous section), since I have not succeeded in forming a perfectly clear picture of the action he conceives to have taken place: "The widening of the lines must be attributed to circular velocity in a plane or planes not greatly inclined to our sight line, and the advancing parts of the whirling gases must be covered by a sufficient depth of absorbing medium to give the dark bands. A great cyclonic storm of heated gases rushing towards us in the lower atmosphere of the star, trending upwards and returning over the star's limb in the higher regions, would satisfy all the requirements of the spectrum, and might meet with favor if only we could accept the form of disturbance, the high velocities and six weeks' (?) duration as probabilities. But if we estimate possibilities in the heated atmosphere of a giant star by the velocities and durations of some of the destructive cyclonic hurricanes in the cold atmosphere of our little Earth, we can hardly deny possibility to this origin of the spectrum."*

Lockyer† sees in the phenomena of the Nova a confirmation of his meteoritic hyphothesis, and explains them as the result of the collision of two meteoric swarms. a somewhat dense swarm, moving in the direction of the Earth with great velocity, passes through a less compact swarm moving in the opposite direction. But why all the particles of the denser swarm, or at least most of them should give spectra with dark (absorption) lines, and the particles of the sparse swarm, for the most part, spectra with bright lines, is not further explained; nor is the question investigated, how the enormous relative velocity of over 460 miles per second can persist after the mutual penetration of two cosmical clouds or meteoric swarms, involving the close passage and inevitable collisions of particles whose masses are of the same order and the transformation of their energy of motion into heat.

More carefully considered, and more in accordance with probability and the observed facts are the views which Huggins‡ has

* Memoirs of the Royal Ast. Soc., Vol. LI, p. 34.
† Proc. Roy. Soc. Vol. 50, p. 435.
‡ Proc. Roy. Soc., Vol. LI., p 493.

advanced. He starts with the assumption that two bodies exist, which are either gaseous or provided with gaseous atmospheres, and that these bodies, after approaching very closely, are moving in parabolic or hyperbolic orbits, whose axis lies nearly in the direction of the Sun. It is readily conceivable that the components of the motions of the two bodies in the line of sight, after their passage, should be as great as those observed in the Nova, and also that the velocity should change but little in a long time. Unfortunately we have no information with regard to the motion at the time of the occurrence which caused the sudden outburst of the star, since the first spectrum observations were made some forty days after this event.

Huggins remarks further, that the close approach of the two bodies might be regarded as a periodical disturbance repeated at long intervals, as in the analogous hypothesis relating to long-period variables, but that in the case of the Nova the great velocities of the components seem rather to indicate that they are not mainly due to the mutual attractions of the bodies, and the assumption is preferable that two bodies, already possessing great velocities, have accidentally met. In any other case one is compelled to assume masses which are enormously great in comparison with the Sun. The same result was also reached by Seeliger (A. N. 3118), who made the relations just considered the subject of a more elaborate computation.

A direct collision of two celestial bodies is not regarded by Huggins as an admissible explanation of the Nova; a partial collision has little probability, and the most that can be admitted is perhaps the mutual penetration and admixture of the outer gaseous envelopes of the two bodies at the time of their closest approach. A more probable explanation is given by a hypothesis which we owe to Klinkerfues, and which has more recently been further developed by Wilsing, *viz.*: that by the very close passage of two celestial bodies enormous tidal disturbances are produced and thereby changes in the brightness of the bodies. In the case of the two bodies which form the Nova, it must be assumed that these phenomena are displayed in the highest degree of development, and that changes of pressure have been produced which have caused enormous eruptions from the heated interior of the bodies; the eruptions are perhaps accompanied by electrical action, and are comparable with the outbursts in our own Sun, although they are on a much larger scale.

In such a state of things, all the conditions for the reversal of spectral lines, and for subjecting them to continual changes, are

fully developed, and since such relations are apparent in the bright and dark lines in the spectrum of the Nova, it can hardly be denied that the above assumption has ample justification.

Huggins is of the opinion that the luminous source which gave the continous spectrum, crossed by dark lines strongly displaced toward the violet, always remained within an envelope of cooler absorbing gas, forming with the latter the body which approached the Earth. The reason that the receding body emitted bright lines, while the approaching body gave a continuous spectrum with dark bands, Huggins believes is to be found in the different stages of development of the two bodies, and consequently in the accompanying diversity of density and temperature which must exist in them.

Finally, Huggins directs attention to the variations of light which took place in the beginning, and the rapid diminution of light which followed; also to the fact that the spectrum showed no changes in the relative brightness of the principal lines as long as they could be observed. Here also he finds support for the views which he holds. After several oscillations, the tidal disturbances were followed by a state of quiescence, the outer and cooler gases again completely enclosed the bodies, and the transparency of the atmospheres diminished as the distance between the bodies increased.

The doubts which oppose themselves to these views of Huggins, and to all similar hypotheses, arise mainly in the improbability of the meeting of two bodies moving in opposite directions with such abnormal velocities. If, with Huggins, we regard the broad, bright lines as single (broadened) lines, and the maxima which appeared in them as phenomena of reversal, the displacement of the middle of the lines as compared with the corresponding lines of the terrestrial source is easily determined, and the result is a motion of about 275 miles per second away from the Sun for the one body, and a motion toward the Sun of about 460 miles per second for the other. If we consider further that these are only the components of motion which lie in the line of sight, and that the real motions may be much greater, the improbability becomes even greater than before.

The fine, bright lines which appear in the dark lines of hydrogen, I have regarded as reversals from the first, but to refer the maxima of intensity in the bright lines to a similar cause seems to be hardly justifiable according to my observations, and this constitutes the second objection which I have to make to the views advanced by Huggins. In the normal course of develop-

ment of reversal phenomena in bright lines, a narrow dark line first appears in the middle of the strongly broadened bright one; this broadens with increasing density of the vapor, and a fine bright line appears in its center, constituting a double reversal. It may be that an unsymmetrical disposition of the parts with respect to the middle of the reversed line, and unequal intensities, may occur; but I have never observed them, even when the lines were much more strongly widened than the bright lines in the spectrum of the Nova.

Now all observers agree that the distribution of light in the bright lines of the Nova, with respect to the middle of the lines, was decidedly unsymmetrical, and that this aspect of the lines did not change materially throughout the entire time of the first apparition. It would therefore be necessary to suppose that the component of the Nova which gave the bright-line spectrum did exhibit such an unsymmetrical formation of lines, and a peculiar abnormal distribution of light in them.

Finally, the objection can still be made, that sensible tidal action cannot be assumed to last for any considerable time, as on account of the great relative velocity of the bodies, they would separate at the rate of 46 millions of miles per day. Seeliger* proves that considerable tidal disturbances could in fact have lasted but a few hours. It should not be forgotten, however, that a whole chain of phenomena and oscillations of the most gigantic kind, which might endure for weeks and months, would follow these disturbances, and it is from this point of view that I believe Huggins' hypothesis should be interpreted.

Belopolsky† gives his views in regard to the Nova in the following paragraphs: "As an explanation of the whole phenomenon there remains only the assumption that we have to deal with the superposed spectra of two or more bodies. One body, with an extensive hydrogen atmosphere, and relatively low temperature was approaching us with enormous velocity, while a second, whose temperature was high, and in whose spectrum the bright lines of hydrogen appeared, moved with a velocity which perhaps varied during the time of observation, so that the body first receded from, and then approached us."

"The latter body perhaps consisted of several smaller ones moving in directions variously inclined to the line of sight. The constancy and enormous magnitude of the velocity of the first

* A. N., 3118 ASTRONOMY AND ASTRO-PHYSICS, Dec., 1892.

† Spectrum der Nova Aurigæ, Mélanges mathém. et astron. T VII, St. Pétersbourg, 1892, p. 297.

body allows the conclusion to be drawn that it was the chief body of the system, and that its velocity is to be ascribed to initial momentum, and not to the attraction of other bodies. The second body (or system of bodies) was then the one that was rendered incandescent in the atmosphere of the first. In comparison with the first, its mass was smaller, and hence by its passage through the atmosphere of the first, a sufficient quantity of heat might have been evolved to convert it into glowing vapor. The occurrence must have been analogous to the explosion of a meteor in the atmosphere of the Earth, (or to a comet at perihelion), whose small mass is rendered incandescent, and reduced to glowing vapor, without making luminous the atmosphere itself."

"This small mass probably described a hyperbolic orbit around the principal body. After it passed out of the gaseous envelope of the latter its brilliancy would be rapidly extinguished as we saw was actually the case. A secondary brightening is often observed in meteors and comets, as well as a continual fluctuation of light during the latter part of their period of visibility."

It is not immediately clear how Belopolsky arrives at the assumption that the body with bright lines in the spectrum was perhaps moving away from us at the beginning of the observations, and afterward toward us. This assumption is, however, a consequence of the supposition that the amount of the displacement of the bright lines cannot be correctly determined, on account of the unsymmetrical form, due to the presence of the dark lines, and it is based upon a small change which Belopolsky observed in the intensity curve of the bright $H\gamma$ line between the first three and the last three observations.

[*To be Continued.*]

ASTRO-PHYSICAL NOTES.

All articles and correspondence relating to spectroscopy and other subjects properly included in ASTRO-PHYSICS, should be addressed to George E. Hale, Kenwood Observatory of the University of Chicago, Chicago, U. S. A. Authors of papers are requested to refer to last page for information in regard to illustrations, reprint copies, etc.

Monsieur Cornu's Spectroscopic Investigations.—A visit to M. Cornu's laboratory at the Ecole Polytechnique in Paris is full of interest to the spectroscopist. Much has been written from time to time on the so-called "secondary" spectrum of hydrogen and the difficulty of obtaining the lines of Balmer's series in the vacuum tube is well known. Professor Cornu's earlier photographs of the ultra-

violet hydrogen spectrum show a very large number of lines, and it is difficult to distinguish in them the lines of Balmer's series. The most elaborate precautions were then taken to obtain the hydrogen in a state of absolute purity. The gas was produced by electrolysis, and carefully dried and purified before being admitted to the vacuum tube. The mercury pump (Sprengel) was separated from the vacuum tube by drying media and vessels filled with copper and sulphur. The pump was frequently cleaned with ozone, and sparks were allowed to pass through it from time to time. In the vacuum tube the electrodes ordinarily employed gave place to exterior metallic sheaths. The result of all these precautions was most satisfactory. The "secondary" spectrum almost entirely disappeared, leaving the lines of Balmer's series strong and well-defined in the photographs. The rythmical grouping of the lines is noticeable at a glance, and no one would hesitate for a moment in selecting the members of the series.

M. Cornu's collection of photographs showing the reversal of metallic lines in the electric arc and spark is very extensive, and the fidelity with which certain phenomena of reversal well-known in the Sun repeat themselves in the arc is most striking.

G. E. H.

Spectroscopy at Stonyhurst College Observatory.—Even to the casual visitor the beautiful old college at Stonyhurst holds out a wealth of attractions, but to the astro physicist a day spent on this Lancashire hill-side is a pleasure indeed. During a recent visit we were fortunate enough not only to examine the extensive buildings of the College under the guidance of Father Sidgreaves, but also to make a careful inspection of the work of the Observatory. In addition to magnetic and meteorological observations, this consists of researches in solar and stellar physics. Photographs of the spectra of various parts of the solar surface—particularly of spots and faculæ—are made with a large spectrometer which has been fitted by Hilger with quartz objectives and a camera tube. An image of the Sun is formed on the slit by an objective receiving light from a heliostat placed on the roof of the Observatory. Cords within easy reach of the hand of the observer give him complete control of the heliostat. The dispersion piece of the spectrometer is an excellent Rowland grating. The sharpness of the photographs leaves nothing to be desired. The double reversals of the H and K lines in faculæ are shown in great perfection, and Father Sidgreaves has even succeeded in obtaining faint double reversals of these lines in the general light of the Sun, *i. e.*, when sunlight reflected from the heliostat is received upon the slit without the interposition of a lens. The Sun is hence a "bright-line star," though hardly so in the ordinary sense of the term, for if it were removed from us to the distance of the nearest star, its light would be so enfeebled as to render the detection of the bright lines almost, if not quite, impossible.

In stellar spectroscopy Father Sidgreaves' most important investigation is fully described in his well known Memoir (published by the Royal Astronomical Society) on the spectrum of *Nova Aurigæ*. The spectroscope used has two direct-vision prisms, with collimation and short photographic telescope, and has been used in connection with an 8-inch refractor. The novelty of the arrangement is in the absence of a slit, the star being made to drift slowly in a direction parallel to the refracting edges of the prisms. The resulting spectra are surprisingly sharp, and the exposure is much less than that required with a slit spectroscope of the same dispersion. By inclining the photographic plate, rays having their foci at different points along the axis of the refractor are focussed simultaneously. Curiously enough, it has not been found that the definition of spectral lines is materi-

ally affected by scintillation. A circumpolar star photographed when near the horizon and again at its upper culmination gave equally sharp lines in the two cases.

The tube of the 8-inch refractor has recently been enlarged and lengthened to fit it for a 15-inch objective just completed by Sir Howard Grubb. The mounting has fortunately proved itself capable of carrying the increased weight, and the instrument in its improved form will stand as a most suitable memorial to the late Father Perry. In the able hands of Father Sidgreaves the greatly increased light-grasping power of the telescope will be of important service in his photographic investigations of stellar spectra. G. E. H.

Professor Lockyer's Researches on Stellar Spectra.*—The well-known opinions which Professor Lockyer holds with regard to the origin and constitution of the different classes of heavenly bodies have been largely founded on the observations of others, as his own work has until quite recently been confined to the sun and to laboratory investigations. During the last two years, however, spectroscopic observations of the brighter stars have been carried on under his supervision at Kensington, and the principal results are embodied in a memoir which has just been published in the Philosophical Transactions of the Royal Society. The observations are discussed with special reference to their bearing on Professor Lockyer's meteoritic hypothesis, the objects of the inquiry, as stated in the memoir, being as follows:—

(1). To determine whether the hypothesis founded on eye observations is also demanded by the photographs.

(2). In the affirmative case to discover and apply new tests of its validity, or otherwise.

The reader of the memoir will naturally look with interest for Professor Lockyer's deductions. For a complete statement of the argument the paper itself must be referred to, but some of the matters which enter into the discussion are considered below, not perhaps in all cases, on account of their bearing on the main line of the argument, but because they have been the subjects of more or less controversy.

Various instruments were employed in the investigation, the most effective form being the object-glass spectroscope originally used by Fraunhofer, and recently revived with so much success at Harvard College Observatory. The largest of these instruments had an aperture of ten inches; other telescopes with six inches aperture were however more frequently used, and some photographs were taken with a spectroscope having a slit and collimator. In enlarging the original negatives, the breadth was increased by a device quite similar to that used by Dr. Scheiner, the only difference being that the reciprocating motion across the length of the spectrum was given to the negative instead of to the plate on which the enlargement was made. While it is doubtful whether there would be any appreciable difference in the results, if the apparatus in each case was well constructed, the advantage, if any, would seem to lie with the arrangement of Dr. Scheiner, as with his construction any irregularity of fitting or motion of the slides would have less effect on the enlargement. The method employed at Kensington had been in use for some time before Dr. Scheiner's method was announced.

In all, 443 photographs of the spectra of 171 stars were obtained. As the

* On the Photographic Spectra of some of the Brighter Stars. By J. Norman Lockyer, F. R. S., Philosophical Transactions, Vol. 184 (1893), pp. 675-726.

...t was to secure detailed spectra of a comparatively few stars rather than spectra of many, several stars were photographed quite a large number of ...es. Some greatly enlarged and highly successful photographic reproductions ...elected spectra are given in five large plates which accompany the memoir.

The material thus obtained by observation was then classified, the stars be... arranged in four tables (A, B, C, D), with reference to the amount of contin...ous absorption in the upper part of the spectrum. Each table contains ...umerous subdivisions. These different classes are afterwards rearranged with ...eference to their places in the order of development according to the meteoritic ...ypothesis, giving rise to a somewhat involved notation that considerably em...barrasses the reader, who is under the necessity of frequently comparing the differ...ent tables. The stars considered in the memoir are included in the first three of Secchi's types, those in table D (bright line stars and Nova Aurigæ), being re...served for a future discussion. Bright-line stars of the Wolf Rayet type and stars of Secchi's type IV, were unfortunately beyond the powers of the appliances at Kensington.

It will, of course, be remembered that Professor Lockyer's system of stellar classification provides for both an ascending and descending branch of the tem...perature curve, and in this respect it certainly has advantages over other systems which claim to have a rational basis. It is therefore interesting to observe that the photographed spectra are found to bear out the interpretation of the visual observations on which the classification was founded, at least, in the types of stars to which the photographs are restricted. On attempting to arrange the spectra in regular sequence, "one important fact comes out very clearly, namely, that whether we take the varying thicknesses of the hydrogen or of the lines of other substances as the basis for the arrangement of the spectra, it is not possible to place all the stars in one line of temperature. For example, in the stars of table A, sub-division α, the hydrogen lines are of the same average thickness as in the stars of Table A, sub-division β, but the remaining lines are almost entirely different; and the two sub-divisions cannot be placed in juxtaposition. It is, therefore, necessary to arrange the stars in two series."

In the tabular arrangement which follows, *α Andromedæ* is the representative of the highest temperature conditions. In tracing the sequence of increasing tem...perature, it is found that "From *α Herculis* to *α Andromedæ*, we thus have a continuous series of spectra, the dark flutings first disappearing, and afterwards most of the lines of the more common metals such as iron and manganese, lines of unknown origin gradually replacing them. At the same time, the amount of continuous absorption is gradually diminishing, and the lines of hydrogen are in...creasing in intensity."

In the same way there is found to be a perfectly continuous series of spectra from *α Andromedæ* to *Arcturus*, on the descending branch of the temperatur... curve.

It is evident that it must be a very delicate matter to decide whether a star belongs to one or to the other branch. "The difference between these (lines in stars of decreasing temperature) and the lines seen in stars of increasing temper...ature should be one due to the different percentage composition of the absorbing layers, so far as the known lines are concerned." The difference in the appearance of the spectra would therefore be small, and the classification would hardly be possible without such a long series as was obtained at Kensington.

The existence of carbon absorption in stars near the middle of the descending temperature branch has an important bearing on the meteoritic hypothesis, but

in general it could not be studied in these spectra, as they did not extend sufficiently far into the violet for the purpose. In the case of *Arcturus*, however, the only star in which this region was photographed, the solar carbon band at λ 3883 was apparently present, but had nearly the same intensity as in the solar spectrum.

With regard to the presence of carbon in stars of Secchi's third type, (placed by Lockyer in Group II, with ascending temperature), the evidence of the photographs does not seem to be in favor of that important part in determining the character of the spectrum which it is assumed to have in the meteoritic hypothesis. In the spectra of *α Herculis* and *α Orionis*, photograped on isochromatic plates, the bright edge of a fluting agrees closely with the brightest edge of the carbon fluting at λ 5165, but the coincidences of bright places in the star spectra with the two secondary maxima of the same fluting are more than doubtful in the case of *α Herculis*, (the discrepancies amount to about eight tenth metres), and apparently no bright places corresponding to them exist in the spectrum of *α Orionis*. It is possible, as Professor Lockyer suggests, that the secondary maxima may be masked by some of the dark lines which appear in the same region. Other flutings of carbon are not represented in the spectra of these stars, but there is said to be strong evidence of the existence of the carbon group commencing at wave-length 4215.6, in a photograph taken by Professor Pickering. Although the evidence, taken as a whole, will probably appear pretty slight to the reader, Professor Lockyer considers it sufficient to establish the presence of bright carbon flutings in these stars.

In all the photographs of stars of this type taken at Kensington, the sharp edges of the dark bands have the same positions. The slight differences found by Dunér and Vogel are to be ascribed to the difficulties of eye observation, and perhaps, in a small degree, to differing velocities in the line of sight.

The iron lines in the spectrum of *α Orionis* are intermediate in character between the lines of the arc and those of the flame spectrum, and the inference as to the temperature of the regions in which the absorption takes place is obvious.

Stars of Secchi's fourth type could not be photographed at Kensington, and for reasons already given, the bright-line spectra which were obtained are not considered in the memoir. There is, however, a very clear statement of the appearances to be expected on the hypothesis, as compared with the appearances actually observed, which covers the whole range of stellar development. In this some modifications of former views will be noticed. Thus, the explanation on p. 710 of those lines (D_3, λ 4471) in the spectra of the nebulæ which are considered to indicate a high temperature,—namely that they are due to the relatively small number of end-on collisions among the meteorites of the swarm,—must be regarded as very much more satisfactory than the vague suggestion that "the degree of fineness which is brought about by temperature in the case of the Sun, is brought about in the spaces between meteorites by extreme tenuity." (*Proc. Roy. Soc.*, Vol. 48, p. 139, 1887). We find also an explanation of the reversal of the hydrogen lines in such stars as *Pleione*.

Professor Lockyer is reluctant to abandon the magnesium origin of the chief nebular line, while holding that it is not of fundamental importance in his argument. Commenting on the observations made at the Lick Observatory on the character and position of this line, he says, "The Lick telescope is perhaps the ideal telescope *not* to employ in such an inquiry." This is doubtless a survival of the idea that a short-focus telescope gives brighter spectra than a long-focus one, and illustrates the tenacity of life in a fallacy which has once obtained wide-

spread belief. The brightness of the spectrum as seen in a spectroscope cannot be determined by the brightness of the image on the slit-plate without considering the dimensions of other parts of the instrument, the true criterion of brightness being the effective aperture of the spectroscope; thus the spectrum obtained with the long-focus telescope may very likely be the brighter. The longer telescope has moreover an advantage, which, as it involves a physiological effect, cannot be expressed mathematically, but which is nevertheless of very considerable importance; and that is the larger scale of the image on the slit-plate, and consequently greater length of the lines seen by the observer. This advantage is obviously greatest for small objects, and is reduced to nothing for objects exceeding a certain (indeterminate) angular magnitude. As a matter of fact, the Lick telescope as much exceeds in precision any instrument which had been used for the same class of observations, as the meridian circle exceeds the sextant.

A more important question, with reference to its bearing on the meteoritic hypothesis, is the presence of bright carbon flutings in the spectra of nebulæ and bright-line stars. For stars of the Wolf-Rayet class it seems to have been pretty definitely settled in the negative; for other stars the existence of carbon radiation cannot be said to have obtained general acceptance; but Professor Lockyer has reserved this branch of the inquiry for another paper, which will be looked for with interest.

Some other points brought out in the memoir may still be mentioned. Fine lines appear in the photographic spectrum of *α Aquilæ*, in addition to the particularly hazy lines which are most conspicuous; hence the explanation suggested by Professor Pickering (*Annals of Harvard College Observatory*, vol. 26, p. 21), that the haziness is caused by a very rapid rotation of the star, must be rejected. A comparison of photographs of the spectrum of *α Tauri* taken at Harvard College Observatory and at Kensington shows some evidence of slight changes in the relative strength of the lines. The presence of the series of hydrogen lines in the ultra-violet is not in itself evidence of a very high temperature. Cornu has obtained the complete series of lines with an ordinary spark without jar. The high temperature of such a star as *Sirius* is not indicated by the fact that its spectrum shows the whole series of hydrogen lines, but by the fact that there is bright continuous radiation far in the ultra-violet.

It will be observed that the stars considered in the memoir, or rather the stars

New Stars.—Some fifteen years ago Professor Bickerton published in the "Transactions of the New Zealand Institute" a monograph bearing on the origin of new stars which may throw some new light on the outbursts last year in the constellation of *Auriga*. He there attributes new stars to a "grazing" collision of stars like the sun. The temperature developed is independent of the amount of grazing, and in similar substances depends only on the velocity destroyed, so that the resultant body would be as if the whole sun collided. An intensely brilliant body may be produced for a few hours, the high molecular velocity of the intensely hot body quickly carrying off the particles into space beyond the influence of the mass's gravity. If rather more than a "grazing" collision occurs, the molecular velocity will be the same, but the mutual attraction greater, and thus a hollow globe of gas, or planetary nebula, will be formed.—*Journal of the British Astronomical Association*, Vol. III, No. 11.

New Variable Stars.—According to Wolsingham Circular No. 37, an anonymous red star, observed at R. A. $19^h\ 7^m\ 16^s$, $+ 25° 46'$, (1855), is variable. Its magnitude was 9.0 on Aug. 21, and had diminished to 11 on Nov. 14. Photographs taken with the 8-inch Compton telescope confirm the variability of Espin 429 (R. A. $19^h\ 59^m\ 6^s$, Dec. $+ 36° 25'$.)

Dr. Anderson announces that D. M. $+ 26°.13$ is variable. The range of variability seems to be small, the estimates of brightness at different times ranging from mag. 9.0 or 9.2 in the D. M. (1855) to 8.7 in 1878, but another observation at Bonn in 1855 makes it 8.3. The place for 1894 is R. A. $0^h\ 16^m\ 51^s.5$, Dec. $+ 26° 24' 27''$.

New Nomenclature for the Hydrogen Series.—Hitherto the lines of hydrogen forming Balmer's well known series have been referred to under various names. The lines called by Fraunhofer *C* and *F* are rarely given other titles, but the line near *G* is frequently called $H\gamma$. The remaining line in the visible spectrum is usually known as *h*, while that near *H* (calcium) has been designated $H\epsilon$. The Greek subscript α was applied many years ago to the ultra violet member of the series next above *H* and *K*, and the remaining lines in the "stellar series" were denoted by subscripts formed from the successive letters of the Greek alphabet. The duplication of names has naturally given rise to some confusion, and it is satisfactory to learn that Drs. Huggins and Vogel intend in the future to call the hydrogen line coincident with *C*, $H\alpha$, that at *F* $H\beta$, and so on through the entire series, thus doing away with the meaningless distinction between the visible and ultraviolet lines. We trust that all astrophysicists will unite in the adoption of this new and convenient system. G. E. H.

New Scientific Terms Wanted.—Professor Newcomb, writing to *Nature*, points out the desirability of having some short and convenient word to express the entire radiation of a body without reference to the manner in which the existence of different rays is made evident. As the now universally accepted view, that the only difference of kind in the rays of the spectrum are those of wave-length, is of quite recent origin, a fitting word has not yet been invented. "Radiant energy," the most accurate of the expressions in use, is subject to the objection of being a description rather than a name. The word "light" is properly applied only to ether waves whose lengths fall between certain limits, and there is no corresponding word for other waves, hence its use although convenient, is unscientific.

Professor Newcomb suggests the term "radiance" as one well fitted to supply

the want in question, and thinks that "The vague and poetic idea hitherto associated with it is an advantage, because it enables us to adapt it to the case in hand with greater readiness than we could adapt a word which already had some well-defined meaning."

To "radiate" would then mean to emit radiance, "radiometry" would mean the measure of radiance, etc. Terms corresponding to transparency and diathermancy would be transradiant or transradious.

The suggestion of Professor Newcomb seems to be an excellent one. We have already many words, such as "energy" with a strictly definite meaning in science and a much wider one in ordinary use. The admission of radiance to this class would be a great convenience to writers.

M. Deslandres, in an article on the solar faculæ in *Knowledge*, proposes the use of the term "flammes faculaires" for the masses of calcium vapor which are found over faculæ and produce the double reversal of the K line. Mr. Ranyard objects to the word "flame" as seeming to imply chemical action and therefore tending to beg the important question whether chemical changes producing light and heat are going on upon the Sun.

Refractive Index of Liquid Air.—The recent work of Professors Liveing and Dewar, on the refractive indices of nitrogen and air, is both novel and interesting. The skill which these gentlemen have shown in handling substances near the absolute zero is well known. They find for liquid air the refractive index

$$\mu = 1.2062$$

for liquid nitrogen at $-190°$ C, they get

$$\mu = 1.2053.$$

The method employed was that of total reflection as devised by Terquem.

These results furnish an interesting verification of the law of Gladstone and Dale that

$$\frac{\mu - 1}{d} = \text{constant}.$$

where d is the density of the substance. Mascart's value of this "refraction constant" derived from measures on nitrogen at *atmospheric pressure* is 0.237 while for *liquid* nitrogen Liveing and Dewar find

$$\frac{\mu - 1}{d} = 0.225$$

This constant, it will be remembered, has a remarkably simple interpretation. It represents probably an optical constant of the gaseous molecule, and a function of the refractive index of the molecule.

The values of μ given above are each for the D line. The next problem which these distinguished workers have assigned themselves is to determine "the dispersive powers of liquid oxygen, nitrogen, and air at a temperature as low as $-200°$ C."—*Phil. Mag. Oct. 1893.*

Spectroheliograph for the Yerkes Telescope.—As the 40 inch Yerkes telescope is to be used during the greater part of the time for astro-physical investigations, three spectroscopic attachments are to be provided for it. Of these the star spectroscope has been completed by Mr. Brashear, and the construction of the solar spectroscope and spectroheliograph will shortly be undertaken. The solar spectroscope will differ from existing instruments of the same class mainly in its increased size and in certain novelties of construction. In the nature of the

case the general design of such a spectroscope must follow certain well-known conditions, which do not admit of wide deviation from existing types. But the designer of a spectroheliograph has more freedom of choice. The instrument is susceptible of decided variation in form, as I have already had occasion to point out. The various types of this instrument described in my paper entitled "The Spectroheliograph" (see ASTRONOMY AND ASTRO-PHYSICS, March, 1893, p. 241)* have each certain advantages and disadvantages, and these must be carefully weighed against one another in designing a large and important instrument for a special purpose.

The 40-inch Yerkes telescope has a focal length of about 64 feet, and will consequently give a solar image nearly 6.5 inches in diameter. A moment's consideration of the facts presented in the paper just referred to will make clear the impracticability of constructing a fixed spectroheliograph with moving slits large enough to allow the whole image to be photographed on a single plate without loss of light near the limb. Suffice it to say that the telescopes of such a spectroheliograph would have an aperture of about 9 inches. In spite of the great weight of such an instrument this large aperture would not of itself be an insuperable objection, were it not for the fact that the ruled surface of the grating (or the edge or base of the prisms, in case they were chosen) would be between 12 and 18 inches long. Unfortunately we have yet to see gratings or prisms (of large angle) of these dimensions.

Nevertheless, it was deemed of the utmost importance that the entire extent of the sun-spot zones be secured on a single plate. That is, it is desirable to photograph an equatorial zone about 4 inches in width. Manifestly this could not be done with a fixed spectroheliograph (like that successfully used with a 2-inch solar image at the Kenwood Observatory) provided with slits moving in the focal planes of the telescopes. Hence the form of spectroheliograph described in the last paragraph on page 256 of the article already referred to has been adopted, substituting, however, one, two, or three simple prisms, with or without a plane mirror, for the grating and plane mirror there described. The arrangement of the prisms and mirror is such as to give a deviation of 180° for the K line. The collimator and telescope of the spectroheliograph are placed with their axes parallel, and a straight slit is fixed in the focal plane of the collimator objective. A similar slit in the focal plane of the telescope is of the same curvature as the K line, upon which it is accurately set by a screw moving the slit-plate in the plane of dispersion. The collimator and telescope are of equal aperture and focal length. The entire instrument is moved at right angles to the optical axis of the 40-inch telescope on wheels with ball bearings, running on knife-edges. The frame which carries the knife-edges is attached to the equatorial by means of four steel tubes 4 inches in diameter. Provision is made for rotating the frame in position angle, so that the motion of the spectroheliograph can be made parallel to the solar equator. The photographic plate-holder is fixed to the frame, and the second slit moves close to the surface of the stationary plate.

The most important advantages of this form of spectroheliograph is the large field photographed. The *length* of the field is evidently determined by the length of the knife-edges on which the instrument runs. This may conveniently be as much as ten inches, or even more. The *width* of the field is determined by the length of the slits, the *height* of the prisms, and the aperture of the collimator and telescope. For with the slit fixed in the axis of the collimator the *width* of

* The presence of the letter F in the second equation on p. 253 is due to a printer's error.

the illuminated portion of the collimator objective remains—with a given focal length—constant. It is always concentric with the objective. The base of the prism is thus comparatively small. For instance, if the ratio of aperture to focal length in the equatorial is 1:18, and the collimator is 36 inches long, the length of one side of a 60° prism would be about four inches. Prisms of this size may readily be obtained, and as an increase in the width of field means simply an increase in the *height* of the prism, and not in the size of base, a very large field can thus be photographed. In order to photograph in a single exposure the 6.9 inch image given by the Yerkes telescope a collimator and telescope of 8.5 inches aperture and 36 inches focal length, with 60° prisms 4 inches on an edge and 5 inches high, would suffice. As such an optical combination would, to say the least, be extremely difficult to realize it is probable that we must be content with photographs showing a zone not wider than four inches. G. E. H.

Mr. Campbell on the Accuracy of His Measures in the Spectrum of Nova Aurigæ. —As Professor Vogel, in his Memoir on Nova Aurigæ, expresses doubt whether the changes in the wave lengths of the bright lines, as observed at the Lick Observatory, should be regarded as established Mr. Campbell, has published a note in A. N. 3192, giving in full the results of a series of measures of the three nebular lines, for the purpose of illustrating their accuracy. The table of measures which he gives shows that the probable error of observation was remarkably small. The comparisons of the three brightest lines of the star with metallic lines gave respectively velocities of — 55, — 51, and — 61 miles, referred to the Sun.

As for the possibility of constant errors, experiments showed that the observed change in the position of the lines was much greater than any error that could be made in estimating the middle of the broad line. The wave length of the chief line in the spectrum of the Orion nebula, measured in the same way, and on the same night, was 5007.39, differing from Keeler's mean result by only 0.05 tenth-metres.

Mr. Campbell says of the results in his table: "It will be seen that the three lines observed in the Nova are almost equally displaced from the normal positions of the three lines in the nebulæ. In fact the results agree a little more perfectly than I expected they would. But I would undertake at any time to repeat these observations, with perfect confidence that the displacements of the two principal lines would differ from each other considerably less than one tenth-metre." The comparisons already referred to, of the changes in the wave-length of the line with the amount of error in estimating the position of its center, leave no basis for the suspicion that the observed changes of wave length might be due to variations in the distribution of the light within the broad line.

Professor Vogel on the New Star in Auriga.—In our translation of Professor Vogel's monograph, concluded in the present number, a considerable part of Section II is omitted, for the reason that it consists mainly of an abstract of observations which we have already printed in full. But Professor Vogel occasionally comments on the observations and makes his own deductions from them, and these, coming from so distinguished an authority, cannot fail to be interesting, particularly as they are sometimes at variance with the deductions of the observers themselves.

With regard to the observations of Dr. and Mrs. Huggins, Professor Vogel attaches considerable importance to the note that the displacement of the D line toward the red was not so great as that of the F line, although the observers regard it as having but little weight on account of the difficulty of the compari-

sons. Still more important is considered to be Dr. Huggins' observation of the composite structure of the supposed nebular lines after the reappearance of the star, and the contrast which these groups of lines presented both with flutings (magnesium) and with the fine sharp lines in the spectrum of the Orion nebula.

The observations of Pickering, Copeland and Becker, and Maunder do not need to be specially considered here, as they are not in disagreement with those made at Potsdam. The explanation of the Nova proposed by Lockyer is referred to in the translation.

In reviewing the observations of Belopolsky, Professor Vogel says, with reference to the fine lines found by that observer in the (first) spectrum of the Nova, and the assertion that no iron lines were present, "I must remark that the assertion that there were no iron lines in the spectrum of the Nova is in direct contradiction to the results which have been obtained by other observers and also by myself; I can also not admit that all other lines except those of hydrogen can be characterized as faint and fine. On the contrary I wish to assert that very many lines were quite similar in appearance to the hydrogen lines, and the observations of Huggins and Campbell are in accordance with this view."

The great amount of detail on the photographs taken at Pulkowa is regarded with suspicion by Professor Vogel, who says in this connection:

"I am constrained to remark, with reference to these observations, that the extraordinary wealth of detail on the spectrograms may have some relation to the extraordinarily long exposure of five hours which was given them, and which may have given rise to small displacements of the spectrum on the plate. According to my experience, displacements of this kind are perceptible even in exposures of one hour, and at temperatures under 0° C, their effects being to broaden the lines, or even easily to give rise to the impression of their being double."

Belopolsky's views on the character of the hydrogen lines are criticised as follows:

"Belopolsky is of the opinion that it is possible to speak of displacement in the case of the dark line Hγ only, as it alone has sharp edges and a symmetrical form. The bright Hγ line is unsymmetrical, which is no doubt to be ascribed to the adjoining dark line. 'The latter (the dark line) probably covers (verdeckt) the second edge of the bright line, and therefore we cannot decide whether the bright line is displaced or not, or determine the amount of the displacement, if there is any.' To this I must remark, that the view that in superposed spectra dark lines in one spectrum can occult (überdecken) bright lines in the other cannot be correct, for the dark lines are nothing positive, but only places of less luminosity in an otherwise continuous spectrum, and therefore cannot occult and extinguish the bright lines of another spectrum as dark, opaque bodies would. Aside from these considerations, the assumption that the bright hydrogen lines are unsymmetrical on account of the proximity of the dark lines is not valid, for a great number of broad bright lines in the spectrum of the Nova, which had no dark companions, were likewise sharply bounded on one side and diffuse toward the less refrangible end of the spectrum."

Having given Professor Vogel's criticism, it is only fair that we should devote the necessary space to Herr Belopolsky's reply. In *A. N.* 3184, at the end of an article on the motion of ζ Herculis in the line of sight, Herr Belopolsky says:

"The following circumstances vouch for the reality of the details on the Pulkowa plates. Since, at the appearance of the Nova, the spectrograph was

attached to the 15 inch refractor in a hasty and provisional manner, all the arrangements for keeping the star in the same part of the slit were defective; the driving-clock ran badly on account of the severe cold and imperfect balancing and the handles of the slow-motion screws in R. A. and Decl. were too short."

"For this reason three quite isolated spectra are found close together on the plate of March 2, each of which, therefore, received an exposure of 1 hour 40 minutes.* The details in all these spectra are the same, hence they are either real, or the errors of the instrument have affected each of the spectra in precisely the same way."

"Moreover, the characteristic details of the Pulkowa spectrograms (resolution of the bands into lines) are to be found on the excellent photographs taken at Harvard College Observatory." . . .

"Finally, it is difficult to assume an error producing influence that could leave the lines sharp in the spectrum of Venus, which was photographed on the same night, likewise at a temperature of from — 3° to — 15°, while it produced wide and doubled lines only in the spectrum of the Nova."

With regard to the presence of iron lines in the spectrum, Herr Belopolsky says,—

"I must remark, on my side, that all my assertions relate only to the small part of the spectrum included between wave-lengths 458 and 420, and I can only repeat here, that in this region none of the iron lines were to be found that had been photographed in the iron spectrum with the same instrument."

The rest of the article is as follows:—

"The remark of Professor Vogel's, that the dark line cannot cover the edge of the bright Hγ line, might be construed as a reproach for my ignorance of Kirchhoff's law, if it were not that the mistake arose through a distinction, unknown on my part, (as I am not a perfect master of the German language), between the words 'verdecken' and 'überlagern.'"† . . .

"According to what Professor Vogel says, all bright lines should be diffuse on one side and sharply bounded on the other, whether they are accompanied by dark lines or not. This, however, is not seen on the photographs of Harvard College, where only those bright lines which have dark companions are sharply bounded on the side toward the violet."

"Finally, I must remark here, that the gaseous envelope of the Nova at its reappearance could be seen no better with the 15-inch than with the 30-inch refractor."

The beautiful photographs of Father Sidgreaves are next considered by Professor Vogel. In general, they accord with the results obtained at Potsdam. More than half of the 41 bright lines shown on these plates coincide with bright and frequently occurring lines of the solar chromosphere.

The following remarks are made on von Gothard's photographic comparison of the spectra of the Nova and various planetary nebulæ, (ASTRONOMY AND ASTRO-PHYSICS, January, 1893, and other journals):—

"To conclude, from the apparent agreement of the spectrum of the Nova with that of planetary nebulæ, that the spectra are identical, and further, that an object which is clearly enough shown by its spectrum to be a celestial body of stellar character whose surface has been strongly heated and then gradually cooled,

* This does not seem to follow, but the matter is unimportant.—Tr.

† Here some sentences are devoted to a comparison of passages by Professor Vogel and Herr Belopolsky in which these words are used,—words which in the translation have been rendered by "cover" and "overlap."

has changed overnight, so to speak, into a gaseous nebula I consider to be at least very rash. However well convinced I may be of the value of Von Gothard's photographs, especially as they form so beautiful a supplement to the observations made at the Lick Observatory, I cannot agree with Herr von Gothard's views, when he characterizes his results as a most interesting and important discovery, and the change which the spectrum of the star underwent during the summer months as unique in the history of astronomy. In contradiction of the last assertion particularly, I must recall the fact that the spectrum of Nova Cygni underwent a quite similar change. According to the observations at that time, when the powerful assistance afforded by photography was lacking, only a single line remained, which coincided with the brightest line of the nebular spectrum, λ 500.7 μμ within the limits of accuracy of the observations. Already at that time the view had been advanced that the Nova had changed into a nebula, and had been rejected by me."

The numerous and complete observations of Campbell at the Lick Observatory, are reviewed at length, the author dissenting however from Campbell's opinion that the spectrum of the Nova after its reappearance was nebular in character. Mr Campbell's defense of his position has already been given in this journal (Oct. 1893) and hence no further comment seems necessary here than perhaps the remark that it is difficult to see how a stronger case could be made. Undoubtedly the Nova, if it had been observed at the time of its reappearance without any knowledge of its previous history, would have been classed as a planetary nebula. It is not necessary to conclude from this that a compact heavenly body suddenly changed into a gaseous nebula, for such a conclusion is derived from hypotheses which have yet to stand the test of critical examination, and perhaps of comparison with observations yet to be made.

Professor Vogel's picture of what must take place when a compact heavenly body of large mass enters a cosmical cloud, according to the hypothesis of Seeliger, is evidently not in accordance with mechanical principles. To assume, as he does, that the particles of the cloud rush upon the body from all sides after it is in their midst, is to neglect the attraction of the body before it enters the cloud. But the particles describe hyperbolic orbits around the body, their motion toward it begins at an indefinite period before it reaches them, and when they are rendered luminous they are moving in not greatly different directions. It would seem that Professor Seeliger's hypothesis modified so as to give the compact body the unusual, but by no means impossible, velocity of 400 miles per second, in order to explain the absolute displacement of the dark lines in its spectrum, affords the most satisfactory explanation of the phenomena presented by the Nova that has yet been advanced.

A translation of Professor Seeliger's reply to these criticisms of his hypothesis will be given in our next number.

Sun Spots.—Dates of remarkable sun spots.
Jan. 21st, 1892, Visible with naked eye.
Feb 5th, 1892 " " "
Feb 19th, 1892, " " "
Jan. 21st 1893, Visible in opera glass.
Feb 5th, 1893 " " "
Feb 25th 1893, " " "
Aug 2d., cluster of spots plainly seen with naked eye.

Also Aug 19th and 30th Sept 3rd 4th, 6th and 28th. Oct. 12th to 19th, large spots were observed with my 4½ inch Clark telescope. Oct 24th and 25th spots visible with naked eye. Dec 7th and 8th spots plainly seen with field glass.

Brooklyn Village, Ohio, Dec 11th, 1893. MARTIN WINGEY.

PHENOMENA DURING THE YEAR 1894.

H. C. WILSON.

Thinking that a general preview of the astronomical phenomena, which ar to be expected during this year, would be of interest to our readers, we have prepared the following notes.

Eclipses.—There will be four eclipses, two of the Sun and two of the Moon, none of which will be of special interest. The first will be a *partial eclipse of the Moon* on March 21, beginning at $5^h\ 37^m$ A. M., and ending at $10^h\ 44^m$ A. M., central standard time. Only one-fourth of the Moon's diameter will be immersed in the Earth's shadow at the time of maximum eclipse.

The second will be an *annular eclipse of the Sun* beginning April 5 at $7^h\ 16^m$ P. M., and ending at $12^h\ 32^m$ A. M., April 6, central time. It will be visible as a partial eclipse in Asia, Alaska, and the eastern part of Europe. The path of the annular eclipse passes from the Indian Ocean across Hindostan, China, and Siberia, into Alaska.

The third will be a *partial eclipse of the Moon*, beginning Sept. 14 at $7^h\ 59^m$ P. M., and ending Sept. 15 at $1^h\ 04^m$ A. M. This will be visible throughout North and South America. The beginning will be visible in the western portions of Europe and Africa. Only 0.23 of the diameter of the Moon will be covered by the shadow at the middle of the eclipse.

The fourth will be a *total eclipse of the Sun*, beginning Sept. 28 at $9^h\ 1^m$ P. M., and ending Sept. 29 at $2^h\ 17^m$ A. M. central time. It will be visible mostly in inaccessible regions. The path of totality passes from Central Africa across the Indian Ocean to the south of Australia. As a partial eclipse it will be visible in the eastern part of Africa, Persia, Hindostan, the Indian Ocean, the southern part of Australia and one of the islands of New Zealand.

Transit of Mercury.—The planet Mercury will pass directly between the Earth and Sun on Nov. 10, so that for over five hours it will be seen projected as a round black spot upon the disc of the Sun. The transit will begin at $9^h\ 55^m$ A. M., and end at $3^h\ 12^m$ P. M. central time. The accompanying diagram, Fig. 1, will indicate the course which Mercury is to take across the solar disc. This transit, which will be the last to occur during this century, will be visible throughout North and South America, and in the western parts of Europe and Africa. Before November we will give the necessary data for computing the times of the contacts for different localities.

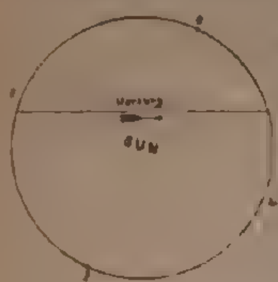


FIG. 1.

Occultations.—The usual number of occultations of stars by the Moon is to be expected. The lists of these for each month will be given one month in advance. We can, however, give only the data which apply to the occultations as seen from Washington, since these data are quite different for different localities, and the labor of calculating them for a sufficient number of points to make a general table even for the United States would be more than we can undertake. The Washington times will serve to call attention to the phenomena, but may be ex-

pected to be in error by many minutes, besides the difference in longitude for other places.

The Planets.—We have had the diagrams, Figs. 2 and 3, made in order to place before the eye of the reader the planets in their true places in their orbits, and relative positions with reference to the Earth and Sun. The circles represent the orbits of the planets, and their positions at the beginning and end of the year, and in some cases at the beginning of each month, are marked upon the circles. It was impracticable to draw them all to the same scale, because of the enormous dimensions of the orbits of Uranus and Neptune.

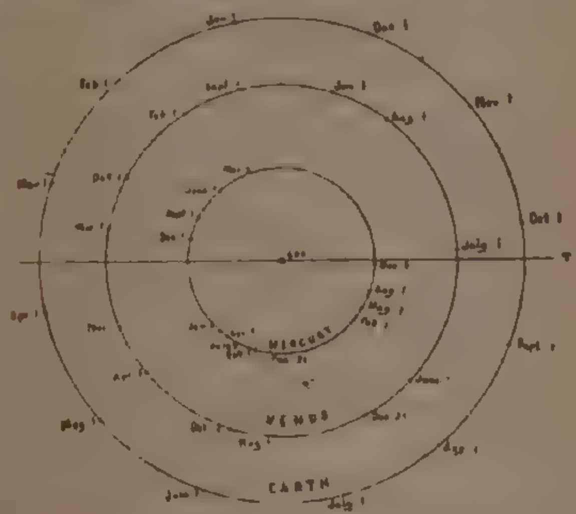


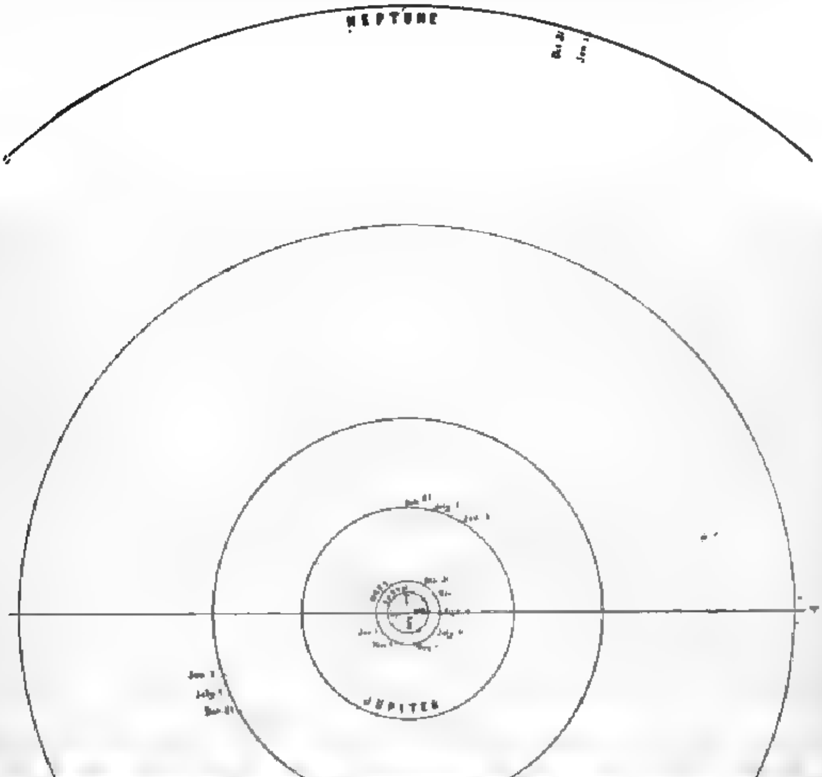
FIG. 2.—DIAGRAM SHOWING THE PLACES OF MERCURY, VENUS AND EARTH IN THEIR ORBITS DURING 1894.

At a glance one will see that Mercury makes a little more than four revolutions about the Sun, being seen from the Earth first on one side then on the other of the Sun. On Jan. 1 this planet as seen from the Earth, is to the right or west of the Sun. Toward the end of the month it will be behind the Sun, at superior conjunction. A few days before March 1 it will be at its greatest distance to the left or east of the Sun, and will therefore be visible in the evening just after sunset. About the middle of March Mercury will be in line between Earth and Sun, *i. e.*, at inferior conjunction, and invisible. A little study of the chart will show that we ought to expect to see Mercury as "evening star" in the latter part of February, the latter part of June and the middle of October, and as "morning star" about the middle of April, the first of August and the last of December.

In the same way we find that Venus will be "evening star" during January, but will pass between Earth and Sun in February, and after that will be "morning star," reaching her greatest distance to the right or west from the Sun in May. In November she will pass behind the Sun, becoming evening star again.

From Fig. 3 we see that Mars is just coming out from behind the Sun, and will not be in a very good position for three or four months yet, but that from July to the end of the year its position will be very favorable. Jupiter will be too close to the Sun for observation during May and June, and will be in best position in December. Saturn will be best seen in April and May, and will be invisible in

October. Uranus will be in best position in May and June, and Neptune in November and December. At the present time Mars, Saturn and Uranus are in that part of the sky which is visible in the morning, Jupiter and Neptune in the opposite region which is visible in the evening. Mars is rapidly leaving his companions behind, and at the end of the year will join Neptune and Jupiter.



pel II, the second comet of 1873, is due at perihelion April 20, and will be in good position for observation for several months after that time. It will be a telescopic comet. It was last seen in 1878, when it was observed for five months.

The second is Encke's periodic comet, which is not due at perihelion until Feb. 1895, but will be in best position for observation in December, 1894. This is also a telescopic comet, having a period of 3.3 years.

If the reader will refer to the plate of the Jupiter family of comets in the November number of ASTRONOMY AND ASTRO-PHYSICS he will see the orbits of these comets and their relation to that of the Earth.

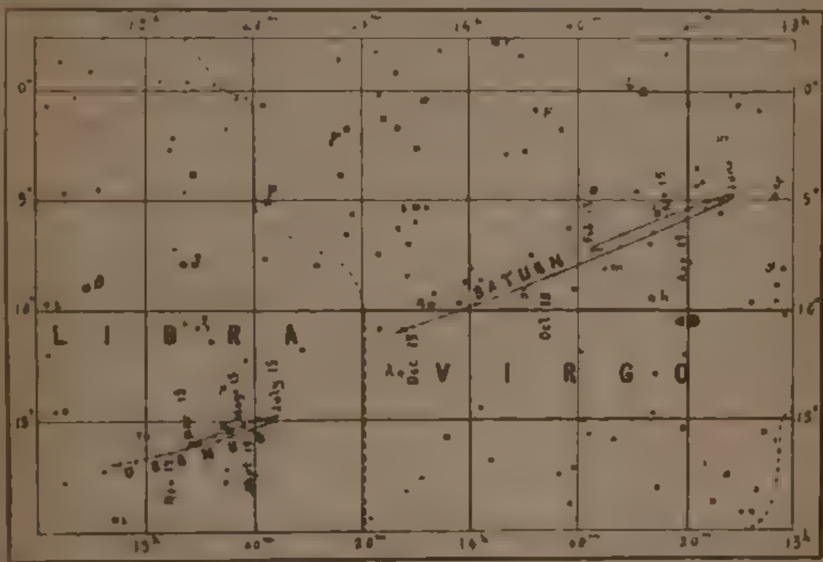


FIG. 4.—CHART SHOWING THE APPARENT PATHS OF SATURN AND URANUS AMONG THE STARS IN 1894.

The Phenomena of the Satellites.—The satellites of Mars are so small and faint that they are not likely to be seen by many amateurs. The most favorable time to make the attempt to see them will be in October when Mars is nearest the Earth.

The phenomena of Jupiter's four outer Satellites may be observed with a very moderate telescope. We will therefore give for each month the times of those phenomena which will be visible in the United States and the configuration of the satellites at the most convenient hour for observation on each night. The diagram, Fig. 5, shows the apparent courses of the satellites around the planet for this year. The diagram gives the appearance seen in an inverting telescope. The vertical scale is made three times that of the horizontal scale in order to clearly separate the lines. Unfortunately the arrows indicating the direction of motion have been omitted from the cut. It is easy, however, to remember that the motion in the upper half of each satellite orbit is toward the left, and in the lower half toward the right. It will be noticed that all the satellites except IV pass in front of the planet when going toward the left and behind it when moving toward the right. Satellite IV barely skirts the upper and lower edges of the planet. The time when a satellite enters upon the right edge of the disc of

Jupiter is designated in the table on page 78 as *Tr. In.* (transit ingress), that when it leaves the left edge *Tr. Eg.* (transit egress). The time when the satellite coming from the left goes behind the planet is designated *Oc. Dis.* (occultation disappearance), when it emerges on the right *Oc. Re.* (occultation reappearance). When the satellite enters the shadow of the planet the designation is *Ec. Dis.* (eclipse disappearance); when it emerges from the shadow, *Ec. Re.* (eclipse reappearance). The shadow of the planet as seen from the Earth is sometimes projected toward the right, sometimes toward the left, and sometimes directly behind the planet, according to the position of the Earth with reference to the line passing through the Sun and Jupiter, so that the last mentioned phenomena occur in different apparent positions with reference to the planet at different times. In February the shadow of Jupiter will be projected toward the right, so that the eclipses all occur after the occultations and on the right side of the planet.

FIG. 5.—DIAGRAM SHOWING THE APPARENT COURSES OF THE SATELLITES OF JUPITER AROUND THE PLANET IN 1894.

The four diagrams at the top of page 77, marked *Phases of the Eclipses, etc.*, show where the observer should look for the disappearance and reappearance of each satellite. Satellite I is so near the planet that it enters the shadow while behind the planet but reappears at the point marked $\underset{\bullet}{r}$. Satellite II disappears in the shadow at $\underset{\bullet}{r}$ very soon after emerging from occultation, and reappears at $\underset{\bullet}{r}$. The third satellite is so far out that both the disappearance and reappearance of eclipses occur quite a distance to the right of the planet, and IV does not enter the shadow at all. When the shadow of a satellite crosses the disc of the planet it is seen as a round black spot entering on the right and passing off the left edge. The beginning of this phenomenon is designated *Sh. In.* (transit of shadow ingress), and the end *Sh. Eg.* (transit of shadow egress).

The configuration of Jupiter's satellites will be indicated, as for February on page 77, for a given hour of each night, the light disc representing the planet and the dots the relative positions of the satellites. The numerals indicate the numbers of the satellites, and also the direction of their motions. The latter is always from the dot toward the numeral. A light disc at the left side of the page indicates that the satellite, whose numeral is attached, is projected upon the face of the planet; a black disc on the right, that the satellite is invisible by occultation or eclipse.

Five of the satellites of Saturn are usually visible with a telescope of moderate power. As they are best seen when at their greatest distances to the right or left of the planet (elongations), we will give each month the times of the eastern elongations. The western elongations will occur just half way between the eastern, and the positions of the satellites at other times can be interpolated with the aid of the diagram Fig. 6.

By this diagram the reader will see that the satellites of Saturn like those of Jupiter move toward the left in the upper half of their orbits and toward the right in the lower. Only the inner two transit across the face of the planet and are occulted. They are so small that they cannot be seen in transit, nor can

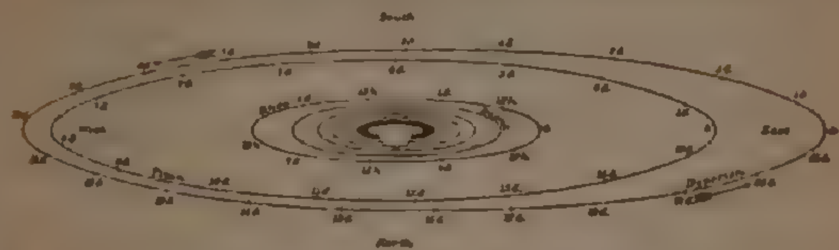


FIG. 6.—DIAGRAM SHOWING THE APPARENT COURSES OF THE SATELLITES OF SATURN AROUND THE PLANET IN 1894.

their shadows be seen. The positions of the satellites at intervals of one day, beginning with eastern elongation, are marked upon the diagram by the letters 1*d*, 2*d*, etc. The use of these will be readily seen from the following example. On Feb. 8 according to the table on page 78, Titan will be at eastern elongation at $12^h\ 9$ P. M. On the next day at the same time the satellite will be at the point marked 1*d*; at midnight of the 9th it will be midway between 1*d* and 2*d*, etc. The orbit of Iapetus is of the same form as the others but has a little more than twice the diameter of that of Hyperion.

The rings of Saturn will be in good position for observation this year. Their appearance is roughly indicated in the diagram.

The satellites of Uranus and Neptune are too faint to be seen except with large telescopes.

PLANET NOTES FOR FEBRUARY

Mercury will be 'evening star' during February. During the first half of the month he will be close to the Sun, but in the latter part will be visible to the naked eye for a short time after sunset. He will be at greatest elongation, east from the Sun 18°, on the evening of Feb. 25. His greatest brilliancy will be attained on the evening of Feb. 21. Mercury will be ten degrees due south from Venus at $9^h\ 41^m$ P. M. Feb. 8, central time.

Venus will be visible as evening planet for but a few days in February. On the 16th, at $3^h\ 04^m$ A. M., she will be at inferior conjunction, *i. e.*, between the Earth and Sun. Venus will be in conjunction with the crescent Moon, 11° north of the latter, at $3^h\ 03^m$ P. M. Feb. 6.

Mars will be visible in the southeast after 4^h A. M., but at too low an altitude for good observations in our latitude.

Jupiter will be at quadrature, 90° east from the Sun, Feb. 11, at $1^h\ 52^m$ A. M. He will be in excellent position for observation during the early part of the night. Jupiter will be in conjunction with the Moon, 4° 24' north of the latter, Feb. 13 at $3^h\ 16^m$ A. M.

Saturn may be observed after midnight. Look toward the southeast in the constellation Virgo, about 5° northeast from the star Spica. The rings of the planet are easily seen with quite a small telescope. They are now turned at an angle of 14° to the line of sight, so that with telescopes of moderate power the divisions may be seen. Saturn's apparent motion among the stars during February will be westward. He will be in conjunction with the Moon, 4° north, at $8^h\ 02^m$ P. M. Feb. 23.

Uranus rises about midnight, and is in position for observation from 3 to 6 A. M. He is in the constellation Libra, about 1° 45′ east and 26′ south of the star α. Uranus will be at quadrature, 90° west from the Sun, Feb. 3 at $7^h\ 04^m$ P. M. He will be stationary in right ascension Feb. 18, and after that will move slowly westward. He will be in conjunction with the Moon, 3° 36′ north, at $9^h\ 58^m$ A. M. Feb. 25.

Neptune will be at quadrature, 90° east from the Sun, Feb. 29 at $2^h\ 36^m$ A. M. He will be in good position for observation during February. He is almost stationary in Taurus, a little more than one-third of the way on a straight line from ι to ε Tauri. There is no star of equal brightness, *i. e.*, 8th magnitude, within a radius of 1°.

Planet Tables for February.

Date.	R. A.	Decl.	Rises.	Transits.	Sets.
1894.	h m	° ′	h m	h m	h m
			MERCURY.		
Feb. 5	21 41.7	−15 51	7 41 A. M.	12 38.1 P. M.	5 35 P. M.
15	22 49.2	− 8 18	7 36 "	1 06.1 "	6 36 "
25	23 40.4	− 0 37	7 17 "	1 17.9 "	7 19 "
			VENUS.		
Feb. 5	22 11.8	− 3 43	7 19 A. M.	1 08.3 P. M.	6 57 P. M.
15	21 49.5	− 4 12	6 20 "	12 06.7 "	5 54 "
25	21 28.2	− 6 00	5 26 "	11 06.2 A. M.	4 46 "
			MARS.		
Feb. 5	17 35.5	− 23 28	4 09 A. M.	8 32.6 A. M.	12 56 P. M.
15	18 05.8	− 23 43	4 01 "	8 23.5 "	12 46 "
25	18 36.2	− 23 37	3 52 "	8 14.6 "	12 37 "
			JUPITER.		
Feb. 5	3 20.0	+17 34	11 00 A. M.	6 15.6 P. M.	1 32 A. M.
15	3 23.4	+17 49	10 22 "	5 39.6 "	12 57 "
25	3 28.0	+18 08	9 46 "	5 04.9 "	12 24 "
			SATURN.		
Feb. 5	13 37.0	− 7 19	10 56 P. M.	4 30.8 A. M.	10 05 A. M.
15	13 36.5	− 7 13	10 16 "	3 51.0 "	9 26 "
25	13 35.4	− 7 04	9 35 "	3 10.5 "	8 46 "
			URANUS.		
Feb. 5	14 51.7	−16 02	12 51 A. M.	5 49.2 A. M.	11 47 A. M.

[redacted]

Jupiter's Satellites for February.

Phases of the Eclipses of the Satellites for an Inverting Telescope.

I. 

III. 

II. 

IV.  No Eclipse.

Configuration at 8h for an Inverting Telescope.

Day.	West		East.
1	3·	O	·1 2· ·4
2	.3 2· ·1	O	·4
3	·3 ·2	O	1· 4·
4	·1	O	·3 ·2 4·
5		O	1·2· 4· ·3
6	2.	O	4· 3. ·1●
7	4· 1·	O	3· ·2●
8	4· 3·	O	·1 2·
9	4· ·3 1·2·	O	
10	4· ·3 ·2	O	1·
11	·4 ·1	O	·2 ·3●
12	·4	O	1· 2· ·3
13	·4 2·	O	3· ·1●
14	·4 1··2	O	3·
15	3·	O	·4 ·1 2·
16	3· 1· 2·	O	·4
17	·3 ·2	O	·1 ·4
18	·1	O	·3 ·2 .4
19		O	1· 2· ·3 ·4
20	2· ·1	O	·3 4·
21	O 1· ·2	O	3· 4·
22	3·	O	·1 ·2 4·
23	O 2· 3· 1·	O	4·
24	·3 ·2 4·	O	·1
25	4· ·1 ·3	O	·2
26	4·	O	1· 2· ·3
27	4· 2· .1	O	3·
28	·4 ·2	O	1· 3·

Occultations Visible at Washington.

Date 1894	Star's Name.	Magnitude.	IMMERSION Washington M. T. h	m	Angle f'm N pt. °	EMERSION Washington M. T. h	m	Angle f'm N pt. °	Duration. h	m
Feb. 11	19 Arietis	6	6	49	76	8	02	226	1	13
12	ζ Arietis	5	11	13	97	12	07	237	0	54
16	c Geminorum	6	17	17	59	17	49	332	0	32
20	σ Leonis	4	9	55	152	10	58	277	1	03

Elongations of the Satellites of Saturn.

(The western elongations will be found approximately half way between the eastern and other positions may be easily interpolated.)

MIMAS.

		h		
Feb.	3	6.7	A. M.	W
	4	5.3	"	W
	5	3.9	"	W
	6	2.5	"	W
	7	1.1	"	W
	11	6 9	"	E
	12	5.5	"	E
	13	4.1	"	E
	14	2.8	"	E
	15	1.4	"	E
	15	midn.		E
	20	5.7	"	W
	21	4.3	"	W
	22	3.0	"	W
	23	1.6	"	W
	24	12.2	"	W
Mar.	1	4.6	"	E

ENCELADUS.

Feb.	2	2.4	P. M.	E
	3	11.3	"	E
	5	8.2	A. M	E
	6	5.1	P. M.	E
	8	1.9	A. M.	E
	9	10.8	"	E
	10	7.7	P. M.	E
	12	4.5	A. M.	E
	13	1.5	P. M.	E
	14	10.3	"	E
	16	7.2	A. M.	E
	17	4.1	P. M.	E
	19	1.0	A. M.	E

ENCELADUS CONT.

		h		
Feb.	20	9.9	A. M.	E
	21	6.7	P. M.	E
	23	3.6	A. M.	E
	24	12.5	P. M.	E
	25	9.4	"	E
	27	6.3	A. M.	E
	28	3.1	P. M.	E

TETHYS.

Feb.	2	4.4	A. M.	E
	4	1.7	"	E
	5	11.0	P. M.	E
	7	8.3	"	E
	9	5.6	"	E
	11	2.9	"	E
	13	12.2	"	E
	15	9.5	A. M.	E
	17	6.8	"	E
	19	4.1	"	E
	21	1.4	"	E
	22	10.7	P. M.	E
	24	8.0	"	E
	26	5.3	"	E
	28	2.6	"	E

DIONE.

Feb.	1	3.1	P. M.	E
	4	8.8	A. M.	E
	7	2.4	"	E
	9	8.1	P. M.	E
	12	1.8	"	E
	15	7.4	A. M.	E

DIONE CONT.

		h		
Feb.	18	1.1	A. M.	E
	20	6.8	P. M.	E
	23	12.5	"	E
	26	6.1	A. M.	E
	28	11.8	P. M.	E

RHEA.

Feb.	3	7.7	A. M.	E
	7	8.0	P. M.	E
	12	8.4	A. M.	E
	16	8.8	P. M.	E
	21	9.2	A. M.	E
	25	9.6	P. M.	E

TITAN.

Feb.	4	6.0	P. M.	S
	8	12.9	"	E
	12	10.5	A. M.	I
	16	2.0	P. M.	W
	20	4 7	"	S
	24	11.2	A. M.	E
	28	9.5	"	I

HYPERION.

Feb.	2	6 6	P. M.	I
	8	3.2	A. M.	W
	12	12.8	P. M.	S
	17	4.7	"	E
	24	12.9	A. M.	I
Mar.	1	9.2	"	W

IAPETUS.

Feb.	28	1.0	A. M	W
Mar.	19	9.9	P. M.	S

Phenomena of Jupiter's Satellites.

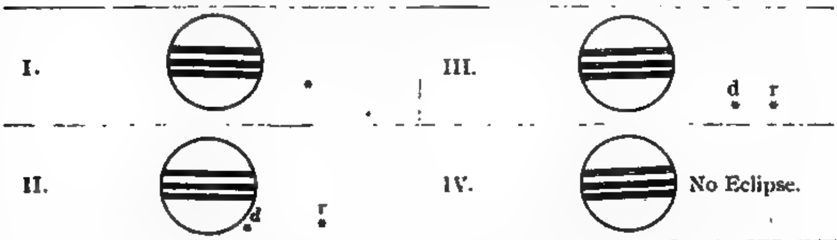
Central Time.

		h m						h m			
Feb.	4	5 06	P. M.	III	Ec. Re.	Feb.	14	8 37	P. M.	I	Sh. Eg

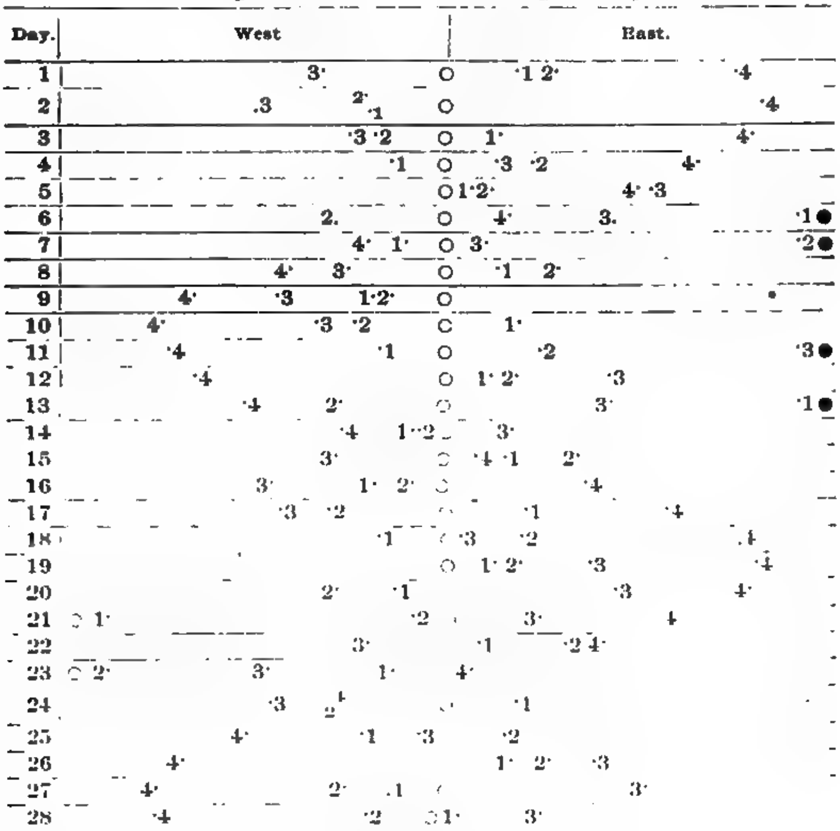
[redacted]

Jupiter's Satellites for February.

Phases of the Eclipses of the Satellites for an Inverting Telescope.



Configuration at 8^h for an Inverting Telescope.



Occultations Visible at Washington.

Date 1894	Star's Name.	Magnitude.	IMMERSION Washington M. T.		IMMERSION Angle f'm N pt	EMERSION Washington M. T		EMERSION Angle f'm N pt.	Duration.	
			h	m		h	m		h	m
Feb. 11	19 Arietis	6	6	49	76	8	02	226	1	13
12	ζ Arietis	5	11	13	97	12	07	237	0	54
16	c Geminorum	6	17	17	59	17	49	332	0	32
20	σ Leonis	4	9	55	152	10	58	277	1	03

Elongations of the Satellites of Saturn.

(The western elongations will be found approximately half way between the eastern and other positions may be easily interpolated.)

MIMAS.

	h		
Feb. 3	6.7	A. M.	W
4	5.3	"	W
5	3.9	"	W
6	2.5	"	W
7	1.1	"	W
11	6 9	"	E
12	5.5	"	E
13	4.1	"	E
14	2.8	"	E
15	1.4	"	E
15	midn.		E
20	5.7	"	W
21	4.3	"	W
22	3.0	"	W
23	1.6	"	W
24	12.2	"	W
Mar. 1	4.6	"	E

ENCELADUS.

	h		
Feb. 2	2.4	P. M.	E
3	11.3	"	E
5	8.2	A. M	E
6	5.1	P. M.	E
8	1.9	A. M.	E
9	10.8	"	E
10	7.7	P. M.	E
12	4.5	A. M.	E
13	1.5	P. M.	E
14	10.3	"	E
16	7.2	A. M.	E
17	4.1	P. M.	E
19	1.0	A. M.	E

ENCELADUS CONT.

	h		
Feb. 20	9 9	A. M.	E
21	6.7	P. M.	E
23	3.6	A. M.	E
24	12.5	P. M.	E
25	9.4	"	E
27	6.3	A. M.	E
28	3.1	P. M.	E

TETHYS.

	h		
Feb. 2	4.4	A. M.	E
4	1.7	"	E
5	11.0	P. M.	E
7	8.3	"	E
9	5.6	"	E
11	2.9	"	E
13	12.2	"	E
15	9.5	A. M.	E
17	6.8	"	E
19	4.1	"	E
21	1.4	"	E
22	10.7	P. M.	E
24	8.0	"	E
26	5.3	"	E
28	2.6	"	E

DIONE.

	h		
Feb. 1	3.1	P. M.	E
4	8.8	A. M.	E
7	2.4	"	E
9	8.1	P. M.	E
12	1.8	"	E
15	7.4	A. M.	E

DIONE CONT.

	h		
Feb. 18	1.1	A. M.	E
20	6.8	P. M.	E
23	12.5	"	E
26	6.1	A. M.	E
28	11.8	P. M.	E

RHEA.

	h		
Feb. 3	7.7	A. M.	E
7	8.0	P. M.	E
12	8.4	A. M.	E
16	8.8	P. M.	E
21	9.2	A. M.	E
25	9.6	P. M.	E

TITAN.

	h		
Feb. 4	6.0	P. M.	S
8	12.9	"	E
12	10.5	A. M.	I
16	2.0	P. M.	W
20	4 7	"	S
24	11.2	A. M.	E
28	9.5	"	I

HYPERION.

	h		
Feb. 2	6 6	P. M.	I
8	3.2	A. M.	W
12	12.8	P. M.	S
17	4.7	"	E
24	12.9	A. M.	I
Mar. 1	9 2	"	W

IAPETUS.

	h		
Feb. 28	1.0	A. M	W
Mar. 19	9.9	P. M.	S

Phenomena of Jupiter's Satellites.

Central Time.

	h m					h m			
Feb. 4	5 06	P. M.	III	Ec. Re.	Feb. 14	8 37	P. M	I	Sh. Eg.

Approximate Times when the Great Red Spot will pass the Central Meridian of Jupiter.

		h m			h m			h m
Feb.	2	1 20 A. M.	Feb.	11	11 39 P. M.	Feb.	21	9 58 P. M.
		9 12 P. M.		12	7 31 "		22	5 50 "
	4	10 51 "		14	1 18 A. M.		23	11 38 "
	5	6 42 "		14	9 10 P. M.		24	7 29 "
	7	12 30 A. M.		16	10 49 "		26	1 16 A. M.
		8 21 P. M.		17	6 40 "			9 08 P. M.
	9	2 09 A. M.		19	2 28 A. M.		28	2 56 A. M.
		10 00 P. M.			8 19 P. M.			10 47 P. M.
	10	5 52 "		21	2 06 A. M.			

Minima of Variable Stars of the Algol Type.

[Given to the nearest hour in Central Standard Time.]

U CEPHEI.

R. A. .... $0^h\ 52^m\ 32^s$
Decl. .... $+81°\ 17'$
Period .... $2d\ 11^h 50^m$

Feb.	1	9 A. M.
	6	8 "
	11	8 "
	16	8 "
	21	7 "
	26	7 "

ALGOL.

R. A. .... $3^h\ 1^m\ 1^s$
Decl. .... $+42°\ 32'$
Period .... $2d\ 20^h 49^m$

Feb.	11	6 A. M.
	14	2 "
	16	11 P. M.
	19	8 "

R CANIS MAJORIS.

R. A. .... $7^h\ 14^m\ 30^s$
Decl. .... $-16°\ 11'$
Period .... $1d\ 3^h\ 16^m$

Feb.	[illegible]	7 P. M.
	4	10 "
	[illegible]	2 A. M.
	7	5 "
	11	6 P. M.
	12	9 "
	13	midn.
	15	4 A. M.
	20	8 P. M.
	21	11 "
	23	2 A. M.
	24	6 A. M.
	28	7 P. M.

S CANCRI.

R. A. .... $8^h\ 37^m\ 39^s$
Decl. .... $+19°\ 26'$
Period .... $9d\ 11^h\ 38^m$

Feb.	6	3 A. M.
	25	2 "

S ANTLIÆ.

R. A. .... $9^h\ 27^m\ 30^s$
Decl. .... $-28°\ 09'$
Period .... $0d\ 7^h\ 47^m$

Feb.	1	8 P. M.
	2	4 A. M.
	2	7 P. M.
	3	3 A. M.
	3	7 P. M.
	4	2 A. M.
	5	2 "
	6	1 "
	6	midn.
	7	"
	8	11 P. M.
Feb.	9	10 P. M.
	10	10 "
	11	9 "
	12	8 "
	13	8 "
	14	3 A. M.
	14	7 P. M.
	15	3 A. M.
	16	2 "
	17	2 "
	18	1 "
	18	midn.
	19	midn.
	20	11 P. M.
	21	10 "

S ANTLIÆ CONT.

Feb.	22	10 P. M.
	23	9 "
	24	8 "
	25	8 "
	26	3 A. M.
	26	7 P. M.
	27	3 A. M.
	28	2 "

δ LIBRÆ.

R. A. .... $14^h\ 55^m\ 06^s$
Decl. .... $-\ 8°\ 05'$
Period .... $2d 07^h 51^m$

Feb.	8	7 A. M.
	15	6 "
	22	6 "

U CORONÆ.

R. A .... $15^h\ 13^m\ 43^s$
Decl. .... $+32°\ 03'$
Period .... $3d\ 10^h\ 51^m$

Feb.	7	9 P. M.
	14	7 "
	18	6 A. M.
	25	4 "

U OPHIUCHI.

R. A. .... $17^h\ 10^m\ 56^s$
Decl. .... $+1°\ 20'$
Period .... $0d\ 20^h\ 08^m$

Feb.	3	6 A. M.
	4	2 "
	8	6 "
	9	3 "
	14	3 "
	19	4 "
	24	5 "
	25	1 "

New Asteroid 1893 AO.—This was discovered by Wolf at Heidelberg, on a photographic plate taken Nov. 6. Its position at $9^h\ 18^m$ Heidelberg M. T. was R. A. $2^h\ 19^m$; Decl. $+\ 11°\ 22'$. Daily motion $-\ 0.6^m$ in R. A. and $-\ 2'$ in Decl. Magnitude 13.

Ephemeris of Comet *c* 1893 (Brooks).—From Professor Porter's last elements. I have computed the following ephemeris of Comet Brooks.

G. M. T.	App. R. A.			App. Dec.		Log *r*	Log Δ
1894	h	m	s	°	′		
Jan. 1.5	20	7	34	+75	[illegible]	0.2894	0.1864
2.5	20	23	7	74	56		
3.5	20	37	53	74	39		
4.5	20	51	57	74	19		
5.5	21	5	4	73	58	0.3009	0.2054
6.5	21	17	18	73	32		
7.5	[illegible]	28	53	73	7		
8.5	21	39	50	72	41		
9.5	21	50	2	72	14	0.3123	0.2255
10.5	21	59	20	71	46		
11.5	22	8	11	71	17		
12.5	22	16	34	70	49		
13.5	22	24	24	70	20	0.3234	0.2462
14.5	22	31	34	60	50		
15.5	22	38	24	69	21		
16.5	22	44	55	68	53		
17.5	22	51	[illegible]	68	24	0.3340	0.2672
18.5	22	56	46	67	56		
19.5	23	2	14	67	27		
20.5	23	7	28	66	59		
21.5	23	12	26	66	32	0.3444	0.2881
22.5	23	17	3	66	5		
23.5	23	21	30	65	39		
24.5	23	25	47	65	13		
25.5	23	29	54	64	48	0.3545	0.3088
26.5	23	33	50	64	23		
27.5	23	37	36	63	59		
28.5	23	41	13	63	35		
29.5	23	44	39	63	12	0.3642	0.3290
30.5	23	47	55	62	49		
31.5	23	51	1	+62	27		

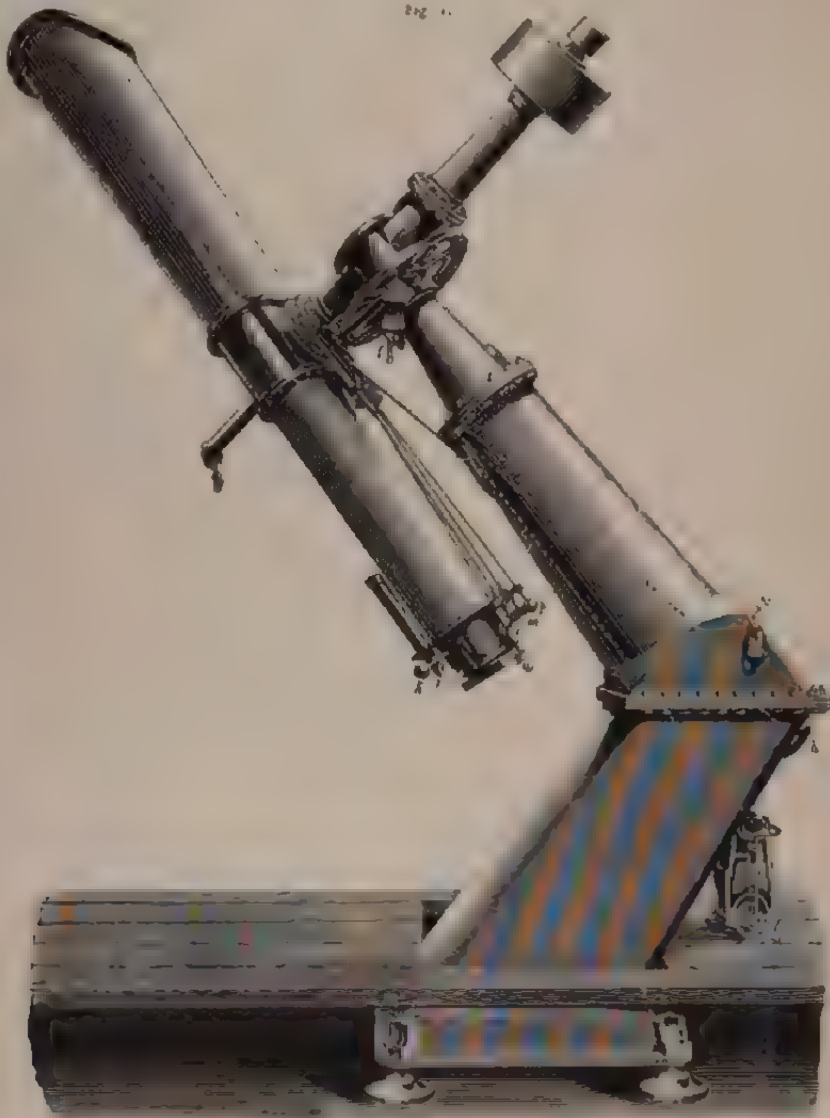
The theoretical brightness of the comet is slowly diminishing.

O. C. WENDELL.

Harvard College Observatory, Dec. 16, 1893.

[redacted]

PLATE V.



THE PHOTOGRAPHIC TELESCOPE OF THE POTSDAM OBSERVATORY

varied and hearty commendations, received from leading scientists at home and abroad, during the last two years, the publisher has believed that this periodical was fairly entitled to rank with the first of its kind. That it has grown too technical and heavy for popular readers, students, and many amateurs, has been evident for more than a year. This was the reason why it seemed necessary to bring out another simpler publication of the same kind. It remains now to be seen whether or not there is really support enough for so expensive a publication as this. The coming year will doubtless answer this query.

Honors for Professors Hall and Barnard.—The December number of *L' Astronomie* conveys the information that at a recent meeting of the Astronomical Society of France, M. Tisserand, president, and director of the Paris Observatory, announced that the Academy of Sciences had decided to confer the Arago gold medal upon Professor Hall for his discovery of the Satellites of Mars, and upon Professor Barnard for his discovery of the fifth satellite of Jupiter, each medal to be worth 1,000 francs. The Arago medal has been conferred only once before, upon the illustrous Leverrier in recognition of his discovery of Neptune, and is an honor that comes to very few astronomers. The discoveries of the satellites of Mars and of the fifth satellite of Jupiter are justly reckoned among the greatest observational achievements of this century, and we are gratified that the labors of such eminent astronomers as Professors Hall and Barnard are so fully appreciated in the highest scientific circles abroad. No higher tribute than the Arago medal could have been paid to American science, and the honor thus conferred upon Prof. Hall and the great astronomer at the Lick Observatory will be fully appreciated by all American astronomers.

The Photographic Telescope of the Potsdam Observatory.—The photographic telescope of the Potsdam Observatory is an ideal one so far as convenience and utility are concerned. The mounting may be said to be a cross between the German and the English forms. It has the good qualities of both these, and the bad qualities of neither. The tube is mounted on a polar axis that has no northern support. This pier, which is of iron and tapering, is bent at a certain point to an angle equal to the latitude. This bent portion is the polar axis upon which the tube and declination axis are mounted. The telescope is therefore free to move under the polar axis even when pointed to the north pole. It is thus possible to point the instrument to any part of the heavens and then revolve it through 360° without touching the pier or meeting with any obstruction to the view. An uninterrupted exposure can therefore be had of any object throughout its diurnal path.

The optical part of the telescope consists of two objectives mounted in a double tube. These glasses are 13 (French) and 9 (French) inches in diameter. The 9-inch is a visual, and is used as a guiding telescope for the 13-inch which is corrected for photography. Both objectives are of the same focus, viz. 3.4 meters. They were made by Steinheil and were mounted by Repsold.

It might be suggested that this form of mounting would be subject to more or less flexure as there is no support for the northern end of the polar axis where the weight of the tube falls. Dr. Scheiner, however, states that it is the most stable instrument they have at Potsdam, and that it has not shown any displacement greater than 15″ in two years.

That the method is safe for instruments not exceeding 13 inches is shown by the fine photographs of clusters (especially M 5, M 11, and M 13) that have been made with it.

Every photograph made with this instrument has a *reseau* impressed upon it, so that accurate measures of the star positions may be made. This *reseau* is impressed on the plate before it is exposed to the stars. In securing an impression of the *reseau* the greatest care must be exercised to avoid errors of parallax, for it cannot be placed in contact with the sensitive plate. The method of doing this at Potsdam is as follows: A small electric lamp is placed in the focus of the 13-inch. Over the object glass is placed a tightly-fitting cap. This cap contains a plate holder with the sensitive plate in it. In a similar holder, between the object glass and the plate, is the *reseau* which consists of a plane glass plate on which an opaque film of silver has been deposited. Upon this the reticle has been ruled leaving lines of clear glass. The small electric light being in the focus of the telescope, its rays will leave the object-glass parallel and thus impress on the plate an image of the *reseau* without any errors of parallax.

Orbit of the Companion of Sirius.—In Gould's Astronomical Journal for Feb. 4th, 1891, under the title of "Orbit of the Companion of Sirius" I described a graphical method for finding the "best" orbit of a double star from "scattering" observations. This method seems to me to be in one respect superior to that described by Professor See, in his paper read before the congress of ASTRONOMY AND ASTRO-PHYSICS at Chicago and published in the last number of this journal, because after doing all that he describes, it takes an additional step in the direction of securing an orbit to give a still better general average of all the observed positions.

For brevity I will only here refer directly to the illustration of the orbit of γ Virginis accompanying Professor See's paper. I am quite sure that if he had gone one step farther, and following the method above referred to had connected each platted observed position, by an arrow, with the place of the companion in its assumed orbit at the corresponding date, so that it could be seen at a glance whether the companion, moving according to the law of equal areas, was anywhere systematically either ahead of or behind its observed positions, a still better orbit would have been suggested. CHARLES P. HOWARD

The editor has kindly submitted to me the above note of Mr. Howard, who raises a point not fully developed in my paper, but it is not one which has escaped my attention in actual work on Double Star Orbits. Mr. Howard seems to assume that I have not sufficiently tested the equality of the areas, and suggests a method for doing this which would be good except for the additional complication thus introduced into the diagram.

In determining orbits I have always measured up the areas by the method of small triangles used by Professor Burnham, which is in all respects satisfactory and has the advantage of leaving the diagram free from unnecessary complications.

It must not, however, be assumed that no systematic deviation should appear in the observed areas, for systematic errors in angle are often conspicuous, and hence the apparent ellipse should not satisfy these erroneous angles.

The final test of an orbit must be derived from a comparison of the computed with the observed angles and distances, but in this comparison, as in the graphical work, care must be exercised to exclude worthless observations.

T. J. J. SEE

The British Photographs of the Recent Total Eclipse of the Sun.—The British Eclipse Expeditions for photographing the total solar eclipse of April 16, 1893,

have proved very successful, and a series of beautiful negatives of the corona were secured at the Brazilian and African stations.

The two stations were supplied with identical apparatus.

1st. A 4-inch photographic lens 60 inches focus.

2nd. A 4-inch lens of 60 inches focus with a meniscus enlarging lens of 8 inches negative focus, the total focal length being 5 feet, 6 inches. These instruments were the work of Dallmeyer. The latter instrument gave an image of the Sun 1½ inch in diameter or an enlargement of about three times.

In Africa 9 negatives were secured altogether, four with the enlarging lens and five with the other instrument. These negatives were made by Sergeant J. Kearney, R. E.

In Brazil 12 negatives were obtained, six with the enlarging apparatus and six without enlargement. They were made by Mr. Taylor.

The plates used were the Cadet and the developer Pyro-Ammonia.

The exposure times were 20^s — 150^s — 50^s — 5^s — 2^s — 15^s, in the order of exposure.

The best negatives were obtained with the shorter exposures. This was a necessary result, due to the fact that the corona is not photographed on a black sky; it must be discriminated from a partially luminous background, and this can best be done with reasonably short exposures, as was pointed out and insisted upon by Mr. Burnham in the preparation for the eclipse of Jan. 1, 1889, (see also p. 73, Lick Observatory Eclipse Report, Jan. 1, 1889).

The negatives from Brazil and Africa, show a splendid amount of detail and extension. The enlarged images are specially fine and show that henceforth large pictures of the corona may be secured with small instrumental equipment which can easily be transported to the most inaccessible places. Indeed Mr. Taylor is confident that a very much larger image could have been successfully secured with the same instrument and he would now prefer an enlargement of eight times instead of three as he thinks the results would have been equally successful on the greater scale. By all means the method of direct enlargement in the telescope should be used hereafter in all expeditions.

The careful development of these English eclipse negatives is specially to be commended, as not only the extensions are secured but also the details near the Moon are carefully preserved in the development. Some of the plates suffered somewhat from the warm, moist climate to which they were subjected.

The corona of 1893 as shown on these negatives is singularly different from that of 1889, Jan. 1st, or 1889, Dec. 22. There is an absence of the beautiful polar fans, and the great equatorial wings of which the coronas of those dates were characteristic. The corona of 1893 more nearly resembles that of 1871 in the distribution of the coronal streams in all latitudes.

One of the most important things to be settled by observations of this eclipse was the question whether rapid changes occur in the form of the corona. The photographs of 1889, Jan. 1, and 1889, Dec. 22, showed that great changes certainly took place in the interval of one year, but no successful photographs had ever been made of the same eclipse with sufficient interval in absolute time to show if these changes were great enough and rapid enough to be shown in a few hours' time. There was certainly no change in a few minutes. This question would perhaps have been settled at the eclipse of 1889, Dec. 22, had the observations not failed in Africa on account of cloudy weather.

In the present eclipse all the parties sent to secure photographs of the corona were very successful, and the stations Chile, Brazil and Africa were far enough

apart in point of actual time of totality to settle this question pretty definitely—at least so far as any considerable change was concerned—and the question has in all probability been settled definitely and in the negative, so far as to change in a few hours.

The negatives of the Brazilian and African expeditions are in the hands of Mr. W. H. Wesley, the secretary of the Royal Astronomical Society, for comparison.

Mr. Wesley's previous experience in such work coupled with his great artistic skill, makes him eminently qualified to conduct such an investigation. Very fortunately, through the courtesy of Professor W. H. Pickering, Mr. Wesley has also the opportunity to compare with these the original negatives of the Harvard party in Chile, which gives a still greater time interval.

Though it will require more time than Mr. Wesley has yet been able to give to the subject to settle definitely the question of change in these photographs, his comparisons have already been of the greatest interest as they prove that no great change can have taken place.

From the extreme interest attached to this very important question, it will perhaps be an excusable act to quote from a recent letter of Mr. Wesley's in answer to a question as to whether his investigations showed any change among the photographs. From a serious illness in his family, Mr. Wesley had not been able to give as much time to the work as he had wished.

"So far as I have gone," he writes, "I am by no means sure if there has been *any* change in the corona. If there is any it is *extremely* slight."

At first there seemed to be some changes but as all the photographs did not verify this they were supposed to be photographic effects.

Mr. Wesley says further:—

"The elimination of *apparent* differences (of a photographic nature) will, I see, be a very difficult task and until I have made much more careful study of the Brazil and African photographs, I am not in a position to speak positively with regard to *real* changes in the corona. They can, however, only be very slight."

The more detailed investigation of these valuable photographs by Mr. Wesley will be watched for with the greatest interest.

Though no certain changes have so far been shown in the corona, considerable changes, as would be expected, were noticeable in the prominences in the negatives from the different stations.

The last negative of Mr. Taylor's list shows "Bailey's Beads" very beautifully where the Sun is just emerging.

The Motion of 61 Cygni.—The *Sitzungsberichte* of the Berlin Academy of Oct. 26, contains the announcement by Dr. J. Wilsing of an observed variation of short period in the distance of the components of 61 Cygni. A series of photographic plates for the determination of the parallax of this star was begun at Potsdam in the autumn of 1890. In the course of the reduction of these measures it was found that certain discordances existed between the parallaxes derived from different comparison stars. These discordances could not be accounted for on the ground of errors of observations, nor did the measures of the comparison stars themselves show evidence of a difference between their own respective parallaxes. Dr. Wilsing was therefore led to suspect the presence of one or more unknown bodies in the system, and to investigate by observation the effect upon the distance between the two visible components. Great care was taken to eliminate from the results all sources of error known to affect photographic observations, and the reductions were carried out with all necessary precision. The series of observations extended from 1890, October, to 1893 September. The following table contains the results suitably grouped in means. They have all been corrected to 1891.0 for proper motion by assuming that the continuous yearly increase of the distance is 0″.10.

Mean Date.		Observed Distance. ″	Prob. Error. ″	Reduced Distance. ″	Diff. from Mean.	No. of Plates.	Impressions.
1890	Oct. 18	20.97	± 0.042	20.99	+ 0,041	2	4
	Nov. 5	20.89	.060	20.91	− .039	1	2
	Dec. 17	20.95	.060	20.95	+ .001	1	2
1891	Feb. 4	20.99	.060	20.98	+ .031	1	2
	May 13	20.95	.024	20.91	− .039	6	12
	June 14	20.82	.032	20.77	− .179	4	8
	Aug. 25	21.08	.017	21.02	+ .071	12	24
	Sept. 17	21.05	.017	20.98	+ .031	12	24
	Oct. 13	21.14	.019	21.06	+ .111	10	19
	Nov. 11	21.17	.022	21.08	+ .131	8	16
	Dec. 17	21.20	.021	21.10	+ .151	9	17
1892	Jan. 15	21.14	.023	21.04	+ .091	2	14
	May 16	21.08	.022	20.94	− .009	5	15
	June 15	21.11	.026	20.96	+ .011	4	11
1893	Jan. 13	21.14	.014	20.94	− .009	6	40
	Mch. 24	21.01	.019	20.79	− .159	3	19
	Apr. 15	21.01	.017	20.78	− .169	3	24
	May 14	21.10	.015	20.86	− .089	4	34
	June 11	21.14	.016	20.90	− .049	4	32
	July 18	21.22	.017	20.97	+ .021	3	24
	Aug. 15	21.22	.017	20.96	+ .011	3	25
	Sept. 8	21.25	.020	20.98	+ .031	2	18

A comparison of the fifth column of the above table with the third shows that the systematic differences are too large in comparison with their probable errors to be taken as the result of errors of observation. Dr. Wilsing gives also a graphical representation of the observations by means of a curve. He concludes finally that the distance of the two visible components of 61 Cygni is subject to a fluctuation as great as 0″.3, and having a period not far from 22 months. Systems of this kind, he thinks, offer a connecting link between the spectroscopic and visual binary systems. H. J.

Observatory at Manila, Philippine Islands.—It may be of interest to you and your readers to learn that the telescope which has been building for our Observatory at Manila has been completed and is now on its way to the Philippine Islands. A cut is now being made of this instrument which will be forwarded to you, as I believe it will be of interest to your readers, as the latitude of our Observatory is only 14° 22′ north.

The objective of this telescope is by Merz of Munich and is of the same size as the one in Strassburg and the one Schiaparelli is using at Milan, having an aperture of a little more than 18 Paris inches, being nearly 20 inches English.

The instrument is very rigidly built, although mere weight was not the object sought after. The telescope tube weighs about one ton and about 5,000 pounds are being moved when the instrument is turned in R. A. It can be set in Declination and R. A. from the floor by means of two handwheels and finding circles, the hour circle being driven by a sidereal clock in order to be able to set directly to right ascensions.

The force necessary to move the telescope by means of the hand wheels is about 4 pounds on a radius of 14 inches. The motions are also communicated to the eye-end and it takes only a force of about 2 ounces to clamp and move the telescope either in R. A. or Declination. The fine hour circle can be read from the floor and the declination circle from the eye-end of the telescope. This eye-end is so arranged that the spectrograph and photographic apparatus can readily be attached; its construction is clearly shown in the cut.

The driving clock runs for over four hours with one winding and is provided with electric control. The illumination is by means of incandescent lamps and in addition, there is a self-adjusting oil lamp.

In design, execution and especially cost it compares most favorably with large telescopes of recent manufacture and it was finished in the short contract time of 10 months.

During the time it was mounted it was examined by the astronomers of the Naval Observatory, by those of Georgetown College and the Catholic University as well as by many scientists, all of whom expressed their admiration for the instrument.

THE EQUATORIAL TELESCOPE FOR THE MANILA OBSERVATORY,
PHILLIPPINE ISLANDS.

It was designed and built, as well as the other instruments for the Manila Observatory, already mentioned in a former number of this journal, by Geo. N. Saegmuller, Washington D. C.

I hope soon to be able to give you more news of this instrument.

Georgetown College, Georgetown, D. C., JOSÉ ALGUÉ, S. J.

Nov. 25, 1893.

Auxiliary Quantities for Computing Precession.—The following tables taken from *Astronomische Nachrichten*, No. 3197, will be found very useful in reducing star places from the various epochs of the star catalogues in common use to 1894 and 1895. The table was prepared by Dr. H. Kreutz of Kiel, Germany.

Auxiliary Quantities for Computing Precession According to Struve for Several Common Epochs:

	$t = 1894.0$				$t = 1895.0$		
t_0	$m''(t-t_0)$	$\lg[n'(t-t_0)]$	$\lg[n''(t-t_0)]$	t_0	$m''(t-t_0)$	$\lg[n'(t-t_0)]$	$\lg[n''(t-t_0)]$
	m s				m s		
1790	+ 5 19.448	2.143209	3.310300	1790	+ 5 22.251	2.147304	3.313455
1800	+ 4 48.741	2.099215	3.275350	1800	+ 4 51.813	2.103800	3.279981
1810	+ 4 18.012	2.050437	3.226528	1810	+ 4 21.105	2.055376	3.231007
1825	+ 3 31.964	1.964093	3.141084	1825	+ 3 35.057	1.971241	3.147332
1830	+ 3 16.608	1.932319	3.108410	1830	+ 3 19.681	1.939051	3.115142
1835	+ 3 1.251	1.896986	3.073077	1835	+ 3 4.324	1.904284	3.080375
1836	+ 2 58.180	1.889561	3.065652	1836	+ 3 1.252	1.896985	3.073076
1840	+ 2 45.893	1.858524	3.034615	1840	+ 2 48.966	1.866492	3.042583
1842	+ 2 39.750	1.842131	3.018222	1842	+ 2 42.822	1.850403	3.026494
1845	+ 2 30.535	1.816321	2.992412	1845	+ 2 33.608	1.825094	3.001185
1850	+ 2 15.170	1.76957	2.94506	1850	+ 2 18.240	1.77933	2.95542
1855	+ 1 59.817	1.71718	2.89327	1855	+ 2 2.890	1.72518	2.90427
1860	+ 1 44.458	1.65753	2.83308	1860	+ 1 47.530	1.67018	2.84627
1864	+ 1 32.170	1.60523	2.77732	1864	+ 1 35.242	1.61717	2.79356
1865	+ 1 29.098	1.58851	2.76460	1865	+ 1 32.170	1.60323	2.77932
1870	+ 1 13.737	1.50631	2.68240	1870	+ 1 16.810	1.52404	2.70013
1872	+ 1 7.593	1.46852	2.64461	1872	+ 1 10.665	1.48753	2.66312
1875	+ 0 58.379	1.40485	2.58094	1875	+ 1 1.449	1.42713	2.60322
1880	+ 0 43.015	1.27222	2.44831	1880	+ 0 46.087	1.30218	2.47827
1885	+ 0 27.653	1.08[illegible]	2.25[illegible]	1885	+ 0 30.725	1.12609	2.30218
1890	+ 0 12.290	0.72814	1.90423	1890	+ 0 15.363	0.82505	2.00114
1895	0 3.073	0.126058n	1.302170n	1895		—	—
1900	− 0 18.436	0.904230n	2.080320n	1900	− 0 15.363	0.825040n	2.001130n

m and n are Struve's constants for the epoch $\frac{1}{2}(t + t_0)$.

If α' and δ' represent the approximate place of the star for the epoch $\frac{1}{2}(t + t_0)$, we have for the formulæ to be used

$$\alpha = \alpha_0 + [m'' (t - t_0)] + [n' (t - t_0)] \sin \alpha' tg \delta'$$
$$\delta = \delta_0 + [n'' (t - t_0)] \cos \alpha'$$

The Chicago Academy of Sciences—Section of Mathematics and Astronomy. Dec. 5th.—Professor G. W. Hough, president in the chair, after routine business had been transacted, the chair introduced Professor Albert A. Michelson of the University of Chicago, who read the paper of the evening on his "Determination of the Length of the Standard Meter in Terms of the Wave Length of the Light of Cadmium," recently made in France under the auspices of the International Committee of Weights and Measures.

Professor Michelson gave a résumé of the efforts hitherto made to find an absolute or natural standard of length, such as $\frac{1}{10000000}$ part of a quadrant of the

Earth's Meridian, and the length of a pendulum vibrating in seconds, and pointing out the practical difficulties which had rendered these efforts futile. He then discussed the history of the standard which Physicists had sought to base upon the wave length of light as the unit, and proceeded to give an account of his own researches by means of the refractometer. This delicate instrument supplies a means of finding the number of light waves in a given small unit of length. The speaker said that experiments on the spectra of different elements had shown that the red line of cadmium was one of the sharpest and best fitted for supplying the desired light, and hence the cadmium line had been adopted in the experiments on the length of the meter. Professor Michelson then proceeded to give a full exposition of the theory of the Refractometer and to show its extreme accuracy. He gave an explanation of the intermediate standards employed in ascertaining the length of the meter, and spoke of the great precautions taken to assure the highest degree of precision. He said his work would soon be published by the International Bureau in Paris, but as it had not yet appeared, he would communicate the preliminary results of two complete and independent sets of measures, which gave

$$1553163\lambda$$
$$1553164\lambda$$

for the length of the standard meter. These results were received by members of the Academy with great enthusiasm, and the distinguished physicist was warmly congratulated on his splendid achievement. On motion of the recorder a vote of thanks was unanimously tendered the speaker for his interesting paper.

In the discussion which followed Judge Ewell, Professor Hough, Dr. Crew, Professor Burnham, Dr. Loves, and several other gentlemen took part. In conclusion, Professor Michelson exhibited a Refractometer and gave the members of the Academy an opportunity to observe some interesting interference phenomena.

T. J. J. SEE, Recorder.

New York Academy of Sciences. Section of Astronomy and Physics. Minutes of the meeting December 18, 1893.—The meeting was called to order at 8.15 P. M., with Professor Rees in the chair. The minutes of the previous meeting were read and approved.

Mr. Harold Jacoby presented the following report on the meeting of the National Academy of Sciences.

The National Academy of Sciences met in the Capitol at Albany, November 7-9. The papers presented included one by Dr. S. C. Chandler, entitled "Additional Researches on the Motion of the Earth's Pole." Dr. Chandler finds that the most recent observations obtainable (some still unpublished) confirm the doubly periodic law deduced by him. He showed that the two separate motions of the pole both take place from west to east. Dr. Chandler's paper was discussed by Professors Hall, Newcomb and Boss. They all expressed themselves as now favoring the truth of Dr. Chandler's law of variations. Professor C. S. Hastings read a paper on "A new form of telescopic objective, as applied to the twelve inch equatorial of the Dudley Observatory." The principal characteristics of this instrument are: first, that one of the "ghosts" is made to coincide with the focal plane, thus rendering it harmless; and second, that the transformation from a visual to a photographic telescope is accomplished by substituting a second glass for one of the lenses of the visual combination, instead of adding a third lens. Professor Asaph Hall read a short paper on "Double Stars." Professor Charles L. Doolittle (introduced by Professor Boss) communicated a paper, "Latitude Determinations at the Sayre Observatory," but was unable to be present in person.

During the afternoon of November 8, the members of the Academy visited the new Dudley Observatory by invitation. The completed Observatory was opened for inspection, and an address was made by Professor Simon Newcomb.

Mr. Harold Jacoby also read:

"Some recent papers on the reduction of astronomical photographs." This paper will be contributed to the Astronomical Journal.

Professor Rees made some remarks on the photographic chart of the heavens.

Mr. Post exhibited a number of plates of the Pleiades and β Cygni which he had made at his Observatory at Bayport, L. I. He intended to measure these plates, in order to compare their accuracy with that attained by Rutherfurd and other astronomers.

HAROLD JACOBY,
Secretary of Section.

Astronomy and Astro-Physics.

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General Astronomy.

ON THE PHYSICAL CONSTITUTION OF THE PLANET JUPITER.*

G. W. HOUGH.

The planet Jupiter was one of the first objects to which the telescope of Galileo was directed, and the satellites of the planet were among the earliest discoveries made with that instrument. In 1630, the telescope had been constructed of sufficient power to show the great equatorial belt, and previous to the beginning of the 18th century, the principal phenomena seen on the surface of Jupiter had been observed and the time of rotation and the position of the axis of the planet ascertained. Notwithstanding, however, the great mass of facts which have been collected from observations extending over a period of two hundred and fifty years, yet, up to the present time no theory of the physical condition of the surface has been advanced which has met with universal acceptance.

It is not our purpose to describe in minute detail Jovian phenomena, but simply to call attention to a few points which have a direct bearing on the topic under consideration.

In order that the subject may be more clearly understood, it will be well to state briefly the salient features presented to the eye of the observer. The disk of Jupiter appears as an ellipse, having axes in the ratio of 14 to 15; the longer axis lying in the direction of the planet's equator. The equatorial diameter is about 89,000 miles, and 1″ of arc, seen from the Earth at mean distance, represents 2,300 miles. The mean density of the planet is 1.37 times that of water, and hence the surface density is probably less than that of any known liquid. As the equator of the planet is inclined only three degrees to the orbit, the effect of the Sun will be nearly constant through the Jovian year, and phenomena due to meteorological conditions should have great permanency. During the revolution of the planet in its orbit, the

* Paper read before the Congress of Astronomy and Astro-Physics held in Chicago, August, 1893.

equator as seen from the Earth, is displaced about four degrees, or 1″ of arc. The objects, therefore, seen near the center of the disk, may apparently be shifted in latitude plus or minus 1″ of arc, while objects in higher latitude will be displaced a less amount from the same cause. As the observer is looking on the surface of a sphere, objects will only be seen in normal proportions when on the middle of the disk. At the edge of the disk they will be infinitely small. The rotation of the planet, therefore, will cause all objects to change their size and shape as they are brought under the eye of the observer. Since the apparent rotation of the surface is most rapid at the equator and zero at the poles, objects passing across the disk will appear to move with different linear velocities, so that spots or markings, lying in different latitudes, may apparently change their relative positions under the eye of the astronomer; a fact which is sometimes lost sight of by modern observers.

There are two principal periods of rotation, determined by the observation of spots and markings, which in round numbers are $9^h\ 56^m$ and $9^h\ 50^m$.

If the longer period is assumed as the approximate time of the rotation of the planet, the proper motion of the objects conforming to the shorter period is about 250 miles per hour at the equator, or a complete revolution in 45 days. Neither of the periods given are absolutely fixed, but the observed motion of markings approximately conforms to the one or the other. The rotation period appears to be independent of the latitude of the object observed.

The most conspicuous marking on the disk is the great equatorial belt, which has been visible since the earliest observations with the telescope. The rotation period from observations of the belt, is $9^h\ 56^m$. The belt changes in size and position, expanding or contracting in width. On either side of the equator there are other belts which are not so distinct, but all are arranged approximately parallel to the equator of the planet. Dark spots or markings are frequently seen on these belts. These spots usually have a drift in longitude relative to each other. The spots are sometimes seen without material change in size or shape during two or more oppositions of the planet.

In latitude 8″ to 12″ south, oval white spots have been observed by myself at every opposition since 1879. Similar objects have been delineated by earlier observers. These spots are usually from 1″ to 1.5″ of arc in diameter; they have motion among themselves in longitude and possibly in latitude.

Now, although we only see objects in two dimensions, it is reasonable to suppose from their shape that they have a depth commensurate with their surface dimensions, in which case, these oval spots may extend downwards towards the center of the planet at least 3,000 miles. Since these oval spots are free to move with reference to each other, it indicates that the medium in which they are located, has a depth at least equal to the diameter of the object.

The most conspicuous isolated mass of dark matter, is the great red spot, south of the equator, which has excited so much interest since 1878. It appears probable that this object was observed by Cassini in 1665, and also by modern observers previous to 1878. Between 1665 and 1713 the ancient spot reappeared and vanished nine times and at no period was it visible for more than three years. The spot is elliptical in outline, having a length of about 30,000 miles and a breadth of 8,000 miles.

The determination of the period of rotation of the planet, in 1879 and subsequent years, from its observation, indicates that the spot is not stationary but has a slow drift in longitude. There has also been a slight shifting in latitude. Now, if the depth of the spot is assumed equal to its breadth, it would indicate that the medium in which it floats extends downwards at least 8,000 miles. The great change in the color and visibility of the spot during the past fourteen years, would be explained by its greater or less submergence beneath the surface.

Granting, therefore, the reasonable assumption that the oval, nearly round detached objects, are not simply superficial in their dimensions, we must conclude that the medium in which they are located has a depth measured by thousands of miles.

The satellites of Jupiter also offer phenomena which have a direct bearing on the subject. The satellites at times cross all parts of the disk in transit. Usually the satellite disappears at some distance from the limb after ingress and reappears at a similar distance before egress. From this fact it is concluded that the center of the disk of Jupiter has the same reflecting power as the satellite, and hence has no inherent light of its own.

Last year it occurred to me to ascertain definitely at what distance from the limb of the planet the satellite would disappear when projected on the disk in transit. From numerous micrometrical measures, I ascertained that a satellite could be followed with the 18½-inch refractor to a distance of 10″ of arc from the limb. When the transit, however, occurred within 10″ of the pole, (3 and 4 sat.) the satellite could be seen during the entire

transit across the disk. Now, if the diminution of light at the limbs of the planet is due to atmospheric absorption, it would seem to indicate an enormous atmosphere, one of about 20,000 miles in depth. But from the well defined outlines of the limbs of the planet, most astronomers have concluded that the true atmosphere can have no great depth as compared with the diameter of the planet.

From what has already been said regarding the probable magnitude of objects and their freedom *inter se*, it seems to me that all the phenomena observed can be best accounted for by assuming that the planet is yet in a gaseous condition.

Recapitulation:

The arguments leading to the conclusion that the planet is gaseous, are two:

1st The probable volume of the spots both white and dark, seen on the disk and their freedom of motion *inter se*.

2d. The gradual fading of the light of the satellite when projected on the disk in transit.

LIGHT-WAVES AND THEIR APPLICATION TO METROLOGY.

A. A. MICHELSON

Every accurate measurement of a physical quantity depends ultimately upon a measurement of length or of angle; and it will readily be admitted that no effort should be spared to make it possible to attain the utmost limit of precision in these fundamental quantities. At present, lengths are measured by the microscope, and angles by the telescope; and the extraordinary degree of accuracy already attained by the use of these instruments depends entirely on the properties of their optical parts in their relation to light-waves; so that, in fact, light-waves are now the most convenient and universally employed means we possess for making accurate measurements. It can readily be shown that this high degree of accuracy is especially due to the extreme minuteness of these waves.

Thus it is well known that the image of a luminous point consists of a series of concentric colored rings surrounding a bright central disc which is smaller the smaller the ratio of the wave-length of the light to the diameter of the objective employed. In fact, it can be shown that the radius of the bright central disc

* From *Nature*, Nov. 16, 1893.

contains as many wave-lengths as the distance of the image from the objective contains the diameter of the objective. Thus in a telescope twenty diameters long, the diameterof the bright disc is forty wave-lengths or 0.02 mm. If the image be magnified by increasing its distance from the objective, or otherwise, these diffraction rings are magnified in the same proportion; so that nothing is gained thereby in *distinctness*, beyond the point where the rings are just large enough to be visible. But, were it not for the inevitable loss of light, it would be advantageous for *measurements of position* to increase the magnification much further.

This can be accomplished by an extremely useful instrument which has been misnamed the "interferential refractometer" It will be interesting to note that notwithstanding the apparent difference in form, this apparatus, when used as a measuring instrument, differs in no essential particular from the microscope or the telescope, or (what is perhaps a trifle unexpected) the spectroscope; and it is possible to change any one of these instruments into the other by unimportant modifications.

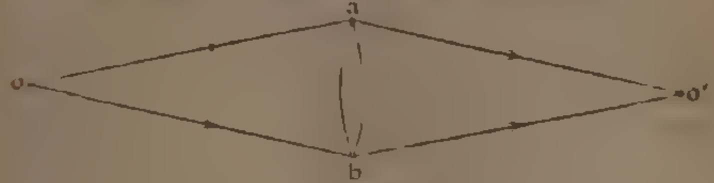


Fig. 1

Thus, let o, Fig. 1, be a source of light, a b a lens which forms an image of o at o'. The operation of the lens, when used to distinguish minute objects, depends upon the accuracy with which all its parts contribute to make the elementary waves reach the focus in the same phase of vibration, but to determine the position of o with respect to a b, this is not at all necessary; and in fact, if we disregard the possible inconvenience due to the dissimilarity between the phenomenon observed and the object whose position is to be measured, it would be as well to entirely annul the central portions of the lens, leaving only an external annular ring, or better still, only two small portions at opposite ends of a diameter

This involves no sacrifice of accuracy, but on the contrary a very considerable gain; for it is now possible to increase the size of the interference fringes up to any desired limit without diminishing the intensity of the light, the result being the same as could be obtained with a perfect microscope of unlimited magnifying power with a source of unlimited intensity.

For this purpose the two small portions to which the lens is reduced are replaced by plane mirrors or prisms, whose office is simply to bring the two interfering pencils into coincidence. Further, the pencils, instead of starting from a point or a line, may be separated by a plane transparent surface; and a second similar surface may be used to reunite the pencils after reflection. Thus the telescope or microscope will have been converted into a refractometer. The exact nature of the analogy will be apparent by a comparison of Figs. 1 and 2.

Fig. 2

It may be assumed that under the most favorable circumstances the utmost attainable limit of accuracy of a setting of the cross-hair of a microscope on a fine ruled line is about $\frac{1}{8}$ of a micron. Now it is usually admitted that the middle point of an interference fringe, if it be sufficiently broad and clear, can be determined within about $\frac{1}{20}$ of the width of a fringe. In the refractometer this would mean only $\frac{1}{40}$ of a light-wave, or about 0.01μ from which it would follow that the refractometer is about five times as accurate as the microscope. But a number of trials with the form of refractometer shown in Fig 8 gave as the mean error of a series of ten observations:

Fr	Fr	Fr
Morley 0.0056	Nicholson 0.0059	X 0.0110

The third observer had no previous practice in this kind of measurement

It is evident from these results that $\frac{1}{20}$ of a fringe is too large an estimate of the average error of a setting, and that it is, in fact, less than 0.01 of a fringe, corresponding to an error in distance of about 0.003μ.

For angular measurement the microscope is replaced by the telescope.

Fig. 3 represents a disposition sometimes adopted for observing minute angular displacements of the mirror *d c*, the light starts from *o*, is reflected by the plane parallel glass plate *p* to the objective *a b* of a telescope, whence the now parallel rays proceed to the mirror *c d*. Thence they retrace their path to the plate *p*,

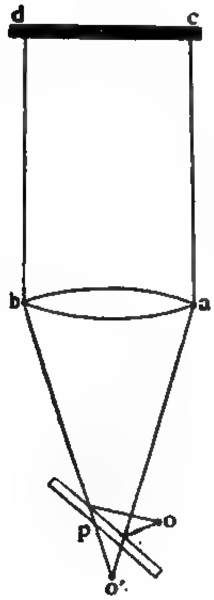


FIG. 3.

FIG. 4.

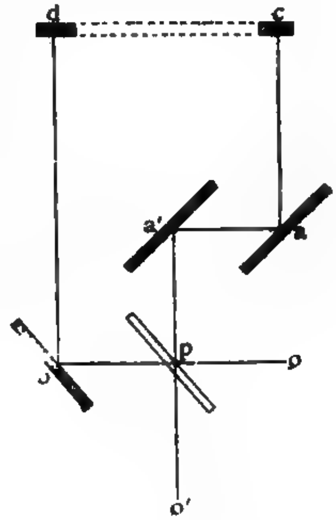


FIG. 5.

through which they are transmitted, forming an image of the source at o', which is viewed through the eyepiece.

Fig. 4 is the exact analogue in the form of a refractometer; and Fig. 5, though slightly different in aspect, is still essentially the same instrument. The path of the rays is *o p a c a p o'* for one of the pencils, and *o p b d b p o'* for the other.

From considerations quite analogous to those employed in the former case, it can be shown that the limit of accuracy attainable in the estimations of angles involves an error of about one-fifth of the angle subtended by a light wave at a distance equal to the diameter of the objective. This is halved by the fact that the angular motion of the beam is twice that of the mirror; so that with a telescope of 10 cm. aperture the limit of accuracy may be estimated at $\frac{1}{2000000}$, or say 0.1″. But taking 0.01 fr. as the smallest perceptible displacement of the mirrors *cd*, the corresponding angle of rotation of the line *cd* (10 cm. long) would be only $\frac{1}{20000000}$, or say 0.01″.*

* In the use of the revolving mirror as in galvanometers, gravity and torsion balances, etc., the accuracy can be increased by enlarging the surface of the mir-

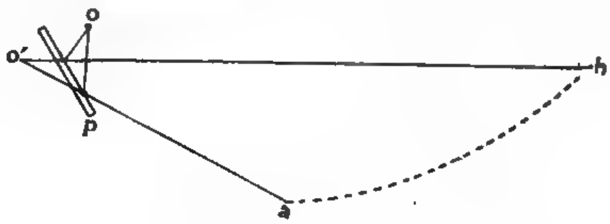


Fig. 6.

It is not at first evident that there is any relation between the refractometer and the spectroscope. A comparison of Fig. 6 and Fig. 7 shows, however, that there is a strict analogy. Fig. 6 represents a disposition sometimes adopted to observe the spectrum by means of a concave grating, and Fig. 7, with unimportant modifications, is the arrangement actually employed in the analysis of radiations by means of their "visibility curves," as will be explained below.

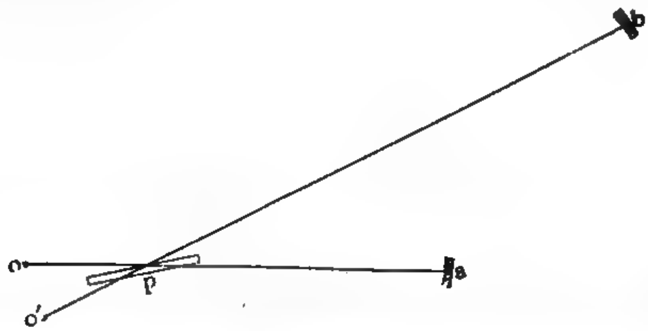


Fig. 7.

The name "interferential refractometer" seems rather inappropriate to an instrument which has so many important applications beside the measurement of indices of refraction; but as it has been sanctioned by long usage it will be retained.

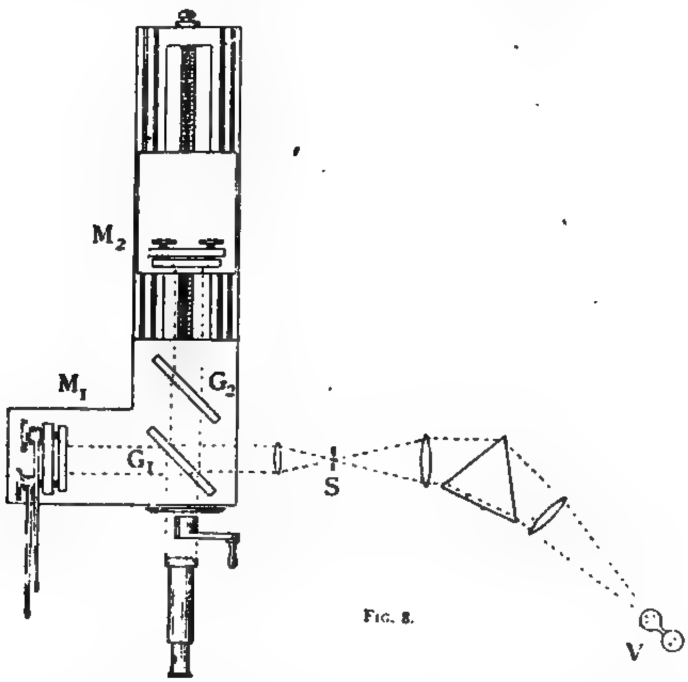


Fig. 8.

Among the many forms of the apparatus which have been rendered classic by the works of Arago, Fresnel, Fizeau, Jamin and Mascart, and which are so admirably adapted to the work for which they were designed, there are none which are not open to serious objections when applied to the solution of such problems as the measurement of lengths and angles, for the analysis of the constitution of the light of spectral lines, and especially for the determination of wave-lengths in absolute measure. For these, the form of instrument shown in Fig. 8 has many important advantages, among which the following may be mentioned:—It is simple in construction, and is easily adjusted; it may be used with a broad luminous surface as source of light; the pencils may be separated as far as desired; its range of difference of path between the interfering pencils is unlimited; and when properly adjusted the position of the interference fringes is perfectly definite, so that there is no uncertainty on account of parallax, and no

difficulty in counting the number of fringes passing a given point. Finally, it may be added, that this is probably the only form of instrument which permits the use of white light (and consequently of the identification of the fringes) in the determination of the position or inclination of a surface without risk of disturbance due to contact or close approximation.

As shown in Fig. 8, the refractometer consists essentially of a plane parallel plate of optical glass G_1 and two plane mirrors M_1 M_2. The beam of light to be examined falls on the plate G_1 at an angle, usually 45°, part being reflected and part transmitted.* The reflected portion is returned by the mirror M_1, and passes back through the inclined plate. The transmitted portion is returned by the mirror M_2, and is reflected by the inclined plate, and from this point it coincides with the other beam, so that the two are in condition to produce interference fringes.†

A little consideration will show that this arrangement is in all respects equivalent to an air-film or plate between two plane surfaces. If the virtual distance between these surfaces is small, white light may be employed, and interference fringes may be observed similar in all respects to those between two plates of glass pressed nearly into contact.‡

If, however, the distance exceeds a few wave-lengths, monochromatic light must be employed. In this case the fringes are in general invisible, unless they be viewed through a small aperture. If, however, the two surfaces are very accurately parallel, the fringes are always distinct, and it follows from the symmetry of the conditions that they are concentric rings. Their diameters increase as the square root of the order of the ring.

These rings are not formed at the surface of the mirrors (as is the case when the distance between them is small), but are per-

* The front surface of the plate G_1 is lightly coated with silver. The light which leaves the refractometer is a maximum where the thickness of the silver film is such that the intensities of the transmitted and reflected portions are equal. The silvering has another important advantage in diminishing the relative intensity of the light reflected from the other surface, and for this reason the thickness of the film may be advantageously increased, which permits also a more uniform surface. The ultimate ratio of intensities of the two pencils is not affected, for what is lost by transmission on entering the plate is made up by reflection on leaving it.

† One of the beams has to pass twice through the thickness of the glass plate G_1, and in order to equalize the two paths, a similar plate G_2 is introduced in the path of the other beam.

‡ If the plate G_1 be not silvered, the colors follow the same order as those of Newton's rings, but if the silvering be sufficiently heavy, the colors are complementary. this, if the plates G_1 and G_2 are exactly equal and parallel. Otherwise the excess of path in glass of one of the pencils disturbs the order of colors by the effect of achromatism due to the dispersion of the glass, as was first pointed out by Cornu.

fectly distinct when the eye or the observing telescope is focussed for parallel rays.

In the preceding comparison between the refractometer and the telescope, microscope, or spectroscope, the "accuracy" has been increased at the expense of "definition." When, however, the object viewed is beyond the "limit of resolution" of the instrument, its form and distribution of light can no longer be inferred from that of the image. Thus, if the object be a disc, a triangle, or a double star, the appearance in the telescope is the same. Similarly in the spectroscope, a source of great complexity cannot be distinguished from one which produces a single spectral line. So that for such objects, even in the ordinary sense of the word "definition," the more familiar optical instruments cannot claim any advantage over the refractometer; but if by "definition" is meant not the actual resemblance of the image to the object, but the accuracy with which the form or the distribution of light in a minute source may be inferred, then it can be shown that all the advantage rests with the refractometer

As an illustration of such an application of interference methods, let us consider the celebrated experiment of Fizeau, in which Newton's rings are observed with a sodium flame as source. The light, consisting of two separate systems of radiations differing by about one-thousandth in wave-length, each system produces its own series of interference fringes. When the surfaces are nearly in contact, the difference of path is very nearly the same for both systems, and the fringes coincide, and the clearness is a maximum. When, however, the difference af path reaches about 500 waves for one of the systems, it is a half wave more for the other, and the maxima of intensity of the one coincide with the minima of the other; hence at this point the fringes are faintest. But when the difference of path of the first system is about 1000 waves, it is a whole wave more for the second, and the fringes coinciding, there is again a maximum of distinctness M. Fizeau has counted 52 such periods, corresponding roughly to a difference of path of 50,000 waves

Suppose, now, that this double line were so close that it could not be resolved by the spectroscope; then from the evidence furnished by the variations in distinctness of the interference fringes as the difference of path increases, the duplicity of the line could be readily detected. But besides this, it can be shown that the relative intensities of the components, their distance apart, and even the distribution of intensities within the component lines can be inferred.

Thus it has been shown (*Philosophical Magazine* for September, 1892) that among some twenty radiations which were examined (though all give simple lines in the spectrum) the great majority of them are shown to be highly complex. Thus, the red hydrogen line is a double whose components have the intensity ratio 7 : 10, and whose distance is about a fiftieth of the interval between the sodium lines. Each component of the yellow sodium lines is itself a double whose components are in the ratio 7 : 10, and whose distance is about one-hundredth of that between the principal components. Thallium gives a double line whose components are in the ratio 1 : 2, at a distance of about a fiftieth of that of the sodium lines, while each component has a small companion whose intensity is about a fifth of that of the principal lines, at a distance of about one three-hundredth of that of the sodium lines.

The green mercury line is made up of a group of five or six lines, the strongest of which is itself double (or perhaps triple) the distance of the components, being less than a five-hundredth part of that between the sodium lines.

These distances, small as they are, can be measured within about a twentieth part, so that by this means it is possible to detect a change of wave-length corresponding to the ten-thousandth part of that between the two sodium lines.

The red line of cadmium is the simplest of all the radiations thus far examined, consisting of a single narrow line whose intensity falls off symmetrically according to an exponential law, its width (at the points where its intensity is reduced to half its maximum value) being only 0.002 (D_1-D_2). The green and the blue cadmium lines are also comparatively simple, and all three of these lines give interference fringes clearly visible at a difference of path of 100 mm., and under appropriate conditions they all satisfy the requisites for a definite and inalterable standard of length.

The most important of these conditions is that the radiating vapor be so rare that the molecules may vibrate freely; in other words, that the time occupied in the collisions between the molecules be so short relatively to that of the free path, that its influence in disturbing the free vibration may be neglected. Experience shows that in general this limit corresponds to a pressure of one or two thousandths of an atmosphere.

It may be noted that at atmospheric pressure—even when the radiating substance is introduced in quantity barely sufficient to color a Bunsen flame—the greatest difference of path attainable

is only one or two centimetres, whereas with mercury vapor in a vacuum tube intereference fringes have been observed with a difference of path of 47 centimetres, or about 850,000 waves.

In order to make any practical use of these minute quantities for standards of length, it is necessary to employ an intermediate standard, such as that shown in Fig. 9 consisting of a bronze bar carrying two plane-parallel glasses, silvered in front, the distance between which can be compared on the one hand with the fundamental standard in actual use—the metre or the yard—and on the other with the length of a light-wave.

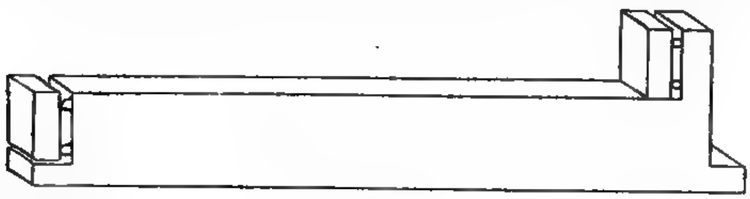


Fig. 9.

The former process is accomplished by moving the standard (whose length it is convenient to take at 10 centimetres) ten times through its own length, the coincidence and the parallelism of the surfaces being controlled at every step by the interference fringes in white light formed between these surfaces and that of the *reference plane* (the virtual image of the mirror MM in G_1, Fig. 8.) The position of a fiducial mark on this standard is compared by means of two micrometer microscopes with the lines defining the standard metre at the first and last steps.

In the second process the only difficulty encountered is due to the very great disproportion between the length of a wave and that of the 10 centimetre standard, and the consequent difficulty in keeping the correct count of the very large number of waves which pass as the reference plane is moved from one surface to the other.

This problem has been solved in the following manner. Nine standards were constructed similar in all respects to that of ten centimeters, save that each succeeding one was half as long as the preceding. The last of the series is thus approximately 0.39 mm. long, corresponding to a difference of path of 0.78 mm. The number of waves in this distance in red cadmium light is 1212 plus a fraction, which is corrected by direct observation of the difference of phase of the circular fringes on the upper and lower (front and rear) surfaces of the standard. This verification is also made with the green and blue radiations.

It is important to note that the measurement of these fractions alone is sufficient to fix the whole number, even if there be an uncertainty of several waves. Thus the relative wave-length of the three radiations being known, the number of green and of blue waves corresponding to the observed number of red waves can be readily calculated, as is shown in the following table:—

	Number of Waves	
Wave-length	Observed	Calculated
μ		
0.64389	1212.34	1212.34
0.50863	1534.76	1534.76
0.48000	1626.16	1626.13

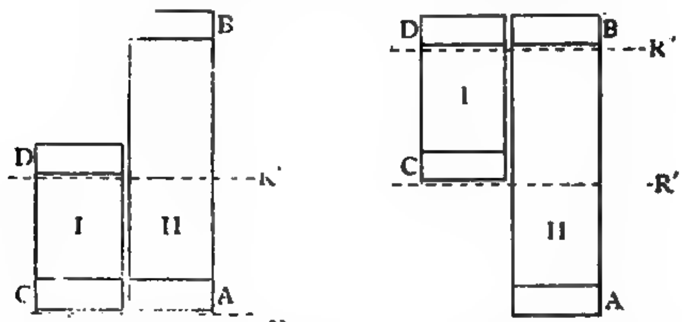


Fig. 11.

If the whole number assumed as the basis of this calculation were in error by one or more waves there would be no correspondence between the observed and calculated fractions. The length of this standard and the succeeding one are now compar-

[redacted]

face (c Fig. 11) once more coincides with the reference plane R′, and its inclination is again adjusted by the interferences fringes.

Fourth Stage.—The reference plane is finally moved back till it coincides with D, the upper surface of I, and its inclination is again adjusted. If now the standard II is just twice as long as I, the fringes will appear simultaneously on *both* upper surfaces D and E.

The adjustment of the length of the standard is usually made to within a few waves, and the outstanding difference is measured by a compensating device.

This is furnished by the rotation of the compensating plate G_1, Fig. 8. The plate is held in a metal frame which is supported at one end by a short thick rod firmly fixed to the bed. At the other end a delicate spiral spring is attached; the tension of the spring *twists* the rod through a minute angle and thus alters the thickness of glass traversed by one of the interfering pencils. The other end of the spring is attached to a flexible cord passing over a pulley which is connected with a graduated circle. The angular motion is thus reduced about 100,000 times, and yet the proportionality is preserved.

Suppose the outstanding difference is ϵ a fraction of a wave-length known to within one or two tenths, then

$$\text{II} = 2\text{I} + \epsilon$$

and consequently the number of red waves should be

$$2 \times 1212.34 + \epsilon.$$

This fraction is corrected by direct observation, as in the case of standard I, and the same control is furnished by the concordance of the results for the three colors; so that an error in the whole number of waves is well-nigh impossible.

The process of comparison and correction is repeated in the same way with the other standards, until we finally arrive at the whole number of waves and approximate fraction in the 10 centimeter standard. Up to this point the question of temperature and pressure is of minor importance for the comparisons and corrections are made while both standards are under the same conditions; and being all made of the same material, it is sufficient to know that the temperature is the same for both. In the measurement of the fractions on the 10 centimetre standard, however, it is necessary to know the temperature and pressure with all possible accuracy, and it is also important that the comparison of this standard with the metre should be made, as nearly as

may be, under the same conditions as that of the determination of the standard in light-waves.

The author having been honored by an invitation from the International Bureau of Weights and Measures to undertake a series of experiments upon the lines here briefly indicated, the necessary apparatus was constructed in America, and shortly afterwards installed in the Bureau International des Poids et Measures at Sèvres.

Two complete and entirely independent determinations were made. These have not yet been completely reduced, but an approximate calculation gives for the number of waves of red light in one metre of air at 15° C and 76 mm.

First series.	......	......	......	1553163.6
Second series.	......	......	......	1553164.6

The difference from the mean is half a wave, or about one fourth of a micron.*

From these results it follows we have at hand a means of comparing the fundamental standard of length with a natural unit—the length of a light-wave—with about the same order of accuracy as is at present possible in the comparison of two metre bars.

This unit depends only on the properties of the vibrating atoms of the radiating substance, and of the luminiferous ether; and is probably one of the least changeable quantities in the material universe.

If therefore, the metre and all its copies were lost or destroyed, they could be replaced by new ones, which would not differ from the originals more than do these among themselves. While such

[redacted]

WEST INDIAN HURRICANES AND SOLAR MAGNETIC INFLUENCE.

H. A. HAZEN.

In the January number of this journal there was a paper by Professor F. H. Bigelow on the relation between solar magnetism and terrestrial temperature, and in the *American Meteorological Journal* for January on the subject at the head of this article. It may be of interest to readers of this journal if I make a few comments on the latter paper which is destined to attract world-wide attention. If the views therein set forth can be maintained and are accepted they will form an extraordinary departure from scientific methods thus far adopted by meteorologists and will mark an era in that science. So important is this discussion and so far reaching in its results that it should be put forth only after the most careful research and accurate substantiation of all the facts which enter it. It is safe to say that a score of meteorologists have been working in the same or similar lines, I mean in the effort at determining definite recurring weather conditions, and with very meagre and disappointing results thus far.

Professor Bigelow, in choosing these hurricanes as the basis for the investigation, has been particularly fortunate for the reason that these storms are quite well defined in their characteristics, occur in a rather circumscribed region, in a well defined period of the year, and can be easily and accurately collated. The only serious difficulty that can arise will be in determining the exact date of their origin, since it is well known that their beginning is in a disturbed condition often lasting for several days. We can avoid error in this regard by taking the day when violent or increasing winds first manifest themselves. It is very essential, however, in all meteorological inquiries of this sort, that we have a very large number of cases to work upon, and I think every one will be painfully impressed with the meagerness of the data upon which such a remarkable conclusion or law was based. I find only forty cases of hurricanes occurring during the years studied and these must be distributed over a period of about 27 days.

On examining the curves showing this magnetic influence upon these storms we see a seeming wonderful similarity amounting to well nigh an exact accordance between their crests and hollows. It would seem as though we would be abundantly justified in concluding that this influence is so marked we ought to find it at every recurrence of the dates in question, that is,

whenever the 6th, 17th, 22d and 26th days of the period came around (these are the more prominent coinciding dates selected by Professor Bigelow) we would expect to find a storm or at least a disturbance in this region. In the 19 years covered by this discussion, these were on the average, 14 recurrences of these dates or of this supposed influence, during each year, or 266 in all. The total number of effects or storms as given by the table, however, is only 28 on these dates, that is, out of a possible 266 cases there are only 28 successes, or 9.5 per cent which favor the influence idea. At the outset this would seem absolutely fatal to the supposition that there is any physical connection between these phenomena. We seem to have nothing more here then a mere coincidence in the few cases given. This position is all the stronger the closer the agreement in the few cases we have.

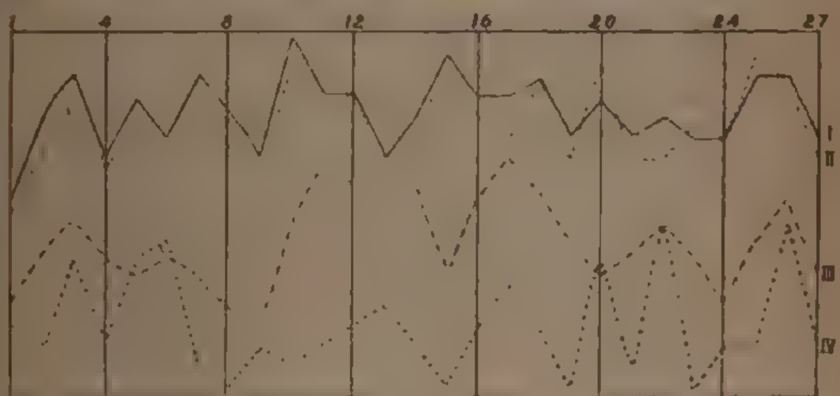
The marked discrepancy at the 20th day in the supposed law of coincidence, is most serious and cannot be lightly passed over. If it is found impossible to explain the occurrence of such a large number of hurricanes on an off day, it will throw a cloud upon the whole research, and this cloud will be all the denser the closer the agreement in the remaining cases. It will be voted that I have laid down general principles only which should be followed in studies of this kind. It seems to me there is a most serious defect in the fundamental magnetic influence curve. Is it possible to conceive of such well defined and such frequent changes in the Sun's magnetism or magnetic influence? If the Sun's magnetism undergoes such changes once in about four days and if the amount of this change has any marked value from the crest to the hollow, we certainly could not find any definite terrestrial effect following from it, for the terrestrial effects would be so blended as to mask entirely the original influence.

On examining more closely the material used, we are much surprised to find that 42 out of the 80 cases are of north north Atlantic storms, or storms above 45° north latitude, and which have no relation whatever to the tropical hurricanes we are supposed to be studying. It would give rise to great confusion in meterorology if one should undertake to study such definite phenomena as these tropical disturbances and at the same time mingle with them storms of a decidedly different aspect. The north north Atlantic storms cover a vast region; their place of origin is frequently far inland and impossible to ascertain; their aspects are often very difficult to determine; in every way they are unsuited to connect with the other storms and cannot be employed properly. I do not mean to say that these storms can-

not be used in proving a solar magnetic influence at the earth, for it is possible they may be so used, but the principles upon which they may be studied, and the rules to be adopted in collating them are so different from those for the others that they must be taken by themselves, or else it must be shown that the origin of the two classes of storms is under similar conditions and that there is a reasonable certainty of a strict comparison between the date, place and manner of origin, etc., of the two.

In the course of an investigation upon the storms of the western Gulf for the forecast division I have had occasion to prepare a list of the 144 tropical storms that have been observed in the waters about the West Indies and the Gulf of Mexico. In order to get some idea of their intensity I have at the same time placed a figure upon a scale of 1 to 3 against each storm, that is, 1 indicates a relatively weak, and 3 a relatively severe storm. This is not a scale of violence alone but one taking in the general extent of the storm and other characteristics showing intensity. The accompanying table gives these storms. The horizontal row beginning with (D) gives the date, and that with (I) the intensity.

I have distributed these storms by the days of solar rotation or magnetic influence adopted by Prof. Bigelow and the summary of these is here given, also the curves showing



I. Hurricanes by number of cases.
II. Same by weight or intensity.
III. Curve of solar magnetic influence.
IV. Hurricane curve found by Professor Bigelow.

These curves show facts as follows:

1st. There is a marked correspondence between the two top curves, and this was to be expected because they are of identical

Storms in the neighborhood of the West Indies during August, September and October from 1874 to 1893.

	August.				September.					October.								
1874, D.					2	8	75											
I.					3	1	3											
1875, D.	1	6	13		9	24				13								
I.	1	1	1		1	1				2								
1876, D.					12					19								
I.					2					3								
1877, D.	2				14	17	21			16	24							
I.	1				3	2	3			1	1							
1878, D.	12	24			1	12	24	29		9	13	18						
I.	2	1			3	1	2	1		1	1	3						
1879, D.	13	16	20	30	8	12				3	10	25						
I.	1	3	1	2	1	1				1	2	2						
1880, D.	5	12	15	24	7					1	5	7	20	27				
I.	2	2	2	3	1					1	2	1	2	2				
1881, D.	1	16	22	27	9	14												
I.	1	1	3	2	1	1												
1882, D.	29				2	22				6								
I.	1				3	1				3								
1883, D.	15	23			4					8	22							
I.	3	2			3					1	2							
1884, D.					3	10				7	11	21						
I.					2	2				2	2	1						
1885, D.	8	23	30		15	18	24	26		10								
I.	2	2	1		1	1	2	2		2								
1886, D.	6	13	15	19	22	24				1	7	22						
I.	1	2	3	3	1	1				2	3	2						
1887, D.	5	15	19	30	1	11	15	24		6	8	9	11	14	16	22	28	29
I.	1	3	3	2	1	2	1	1		1	1	2	2	1	3	1	2	2
1888, D.	16	31			7	24				10	24							
I.	2	3			1	1				1	1							
1889, D.	8	20	25		1	4	13			1	4							
I.	1	1	1		3	1	3			2	2							
1890, D.	11	21	27		[illegible]					2	22							

phenomena. A larger number of storms would have brought about a slightly closer agreement between the seeming waves in these curves.

2. Any one at all familiar with work of this kind will at once see the utter hopelessness of comparing either of these curves, I and II, with that for magnetic influence III

3d. The make up of the last curve IV. from such diverse records and from so few storms would have led us to expect very slight agreement with I or II, but one could have hardly anticipated such marked contradictions It should be noted that I and II are rigidly constructed from the data gathered from the original records and collated too with absolutely no reference to a magnetic or any other period.

Some one may think that I and II would have been materially different if every slight disturbance in the West Indies had been collated This is not true, however, for the reason that any farther disturbances, if there had been such, though a careful search did not show them, would have been distributed uniformly along the 26 68 days and would have simply given a horizontal rise in the curve all along the line and would not have changed the phases of the curve, or rather, its crests and hollows, in magnitude or position in the least This is shown quite clearly by the marked correspondence between I and II.

It should be remarked, in closing, that I believe fully in a strong influence from the Sun's electricity or magnetism upon our weather and that too directly and not through its heat influence. I have been working at this problem for thirteen years and have obtained a few interesting results. I have also studied such curves as these by the thousand and have found the investigation practically hopeless without there is first an elimination of a variety of conditions or effects which serve to mask or cloud the correspondence between phenomena. Unless we can show how or why a physical connection can or may exist between diverse conditions, we are standing on most dangerous ground and are practically on precisely the same footing as the "planetarians," "lunarians," "cyclists," "sunspotists," etc

Jan 15, 1894.

Dr. A Auwers has published, in *Astr. Nach* No. 3195–96, the results of an exhaustive piece of work in the way of comparison of star-catalogues He has compared the fundamental catalogue used in forming the 'Astronomische Gesellschaft' catalogues with forty-seven different others. The result occupies many pages of figures, and it is difficult to comment. The paper should be studied by those interested in star-catalogues.—*Observatory*, Jan., 1894

FREE PUBLIC OBSERVATORIES.

W. W. PAYNE

A movement of interest is noticeable of late that has for its object the founding of what may be called free public Observatories for instruction and entertainment in cities and large towns. The idea is by no means, a new one in this or foreign countries; for to a very limited extent it has prevailed, in localities where astronomy and its kindred branches have been cultivated, in one form or another, for many years. Very naturally thoughtful people, young or old, educated or uneducated, desire to gain some knowledge of the heavens whose wonders and awe-inspiring sublimity are spread out to human eye everywhere almost every day. Is it a wonder that Astronomical Observatories should unformly have the custom of setting apart one or more evenings each week to meet this common want in the people's mind? Is it a wonder that men of means who have some idea of the value of science in general should give largely to build and endow Astronomical Observatories and chemical and physical laboratories, when they know from common observation what factors such institutions are in the advancement of knowledge and the elevation of common thought and common life?

But our purpose now is not so much to call attention to the beneficent influence of science as a means of popular culture or elevating entertainment, as it is to notice the strong and pronounced phase of this influence in asking that measures be adopted speedily whereby it may be more effective and more generally useful All agree that more should be done for the masses of our intense American life. But thinking men or philanthropists have not put themselves to the task, as yet, very severely, of working out the methods by which these worthy ends may be accomplished. It is true that College extension work and Chautauqua Circles have grown prodigiously within the last five years, and the demands are increasing. Books and apparatus have been adapted to this new order of things, and the change for the better in public sentiment in regard to the value of popular education is everywhere apparent. The walls of college caste even are yielding before this powerful wave and their cloister-like exclusiveness will soon deservedly be a thing of the past. Ruts in science, literature, politics or anything else mean stagnation and that is the sure symbol of death to any cause. Last of all it is

sad to admit it, but it is nevertheless generally true that astronomers have been ill-disposed, averse to, and coldly critical towards anything looking to a popular study of astronomy, unless such instruction be very sacredly guarded by appropriate technical language which involves the essentials of the higher mathematics that specialists only know how to use. This so-called learning is a delusion and a snare. For a scholar to say that he can not express himself concerning anything he knows in plain English so as to be understood by any one possessing common intelligence is a confession he ought to be ashamed of. The difficulty felt is not in the facts of astronomy or the nature of the science, as exacting as it is, but rather in the scholar himself. If he had a little more patience and less shameless pride he would be able to do much more for the cause of popular advancement in useful elementary astronomy. We would not now feel like speaking quite so plainly if we had not seen unmistakable evidences of this thing within the last few months. On the other hand very strong appeals have come from persons interested in the study of popular astronomy recently and simultaneously in the great cities of Boston and New York, saying that the time has come when there should be built and equipped and endowed in each of these places free public Observatories for the purposes of instruction and elevating entertainment. In the February issue of *Popular Astronomy* and other scientific publications as well, will be found articles and notes indicating the sources and extent of the information referred to. It may here be added that the movement has been sufficiently prominent to claim the attention of the Boston Scientific Society at one of its late regular meetings, at which a paper on the theme was read by one of its prominent members which was followed by a general discussion showing general favor for, and hearty sympathy with, the idea of establishing a free public Astronomical Observatory for the city of Boston. It was argued that such an institution should be founded, it should be endowed, it should be wisely managed, and one of its objects should be the entertainment of the public. A scholarly astronomer, an authority in Europe and America in the most intricate and difficult questions of practical and theoretical astronomy, now advising that a free public Astronomical Observatory should be built for the entertainment of the public as one object! Why, think of it, are the scholars coming into line? Unmistakeably they are. And when they do move unitedly nothing probably less than a confusion of their mother tongue will stop them. Unquestionably this new impulse

is one setting in the right direction, and there is evidence that it will grow in strength because of the character of the people that are behind it. That it is wise and right and timely seems equally clear. The Boston beginning is a noble one and we hope it will gather strength and enthusiasm rapidly. The scheme in New York is less developed so far as known, but it is believed that New York will not be behind Boston in any worthy public enterprise of this kind. New York may possibly take the lead.

Death of Dr. Rudolf Wolf.—Astronomy has lost an earnest devotee by the death of Dr. Wolf, Director of the Zurich Observatory, who died on December 6 last, at the age of 77 years. Dr. Wolf was born in 1816 near Zurich, and his early studies were under the direction of Horner, Encke, and Poggendorf. In 1847 he was appointed Director of the Observatory at Berne, where he commenced his studies and observations of sun-spots, which studies led him to announce the connection between sun-spots and the Earth's magnetism. In 1855 he was appointed to the new Observatory at Zurich, which position he held at the time of his death. Dr. Wolf will probably be chiefly remembered for his work on sun-spots, in which he has been engaged for almost the last fifty years. His "Sun-spot Numbers," published annually, which give the state of the solar energy for each month in the year, as indicated by the total area of the spots on the Sun for each month in the year, have been of the greatest service to many investigators in this branch of science. As a member of the Federal Geodetic Commission, of which he was formerly President, Dr. Wolf showed an extensive knowledge of Geodesy, and contributed 'A History of Geodetic Measures in Switzerland' to its literature. Among his other works are a 'History of Astronomy,' which is a most valuable work of its kind, and a 'Treatise on Astronomy,' which is his latest work. Dr. Hirsch, Secretary of the Geodetic Association, thus writes of him:—

"During his long life, wholly devoted to science and filled with sustained and fruitful work, our friend preserved to the last days of his short illness the serene tranquility which marked his lovable character. His fine figure will remain engraven in the memory of his friends and colleagues as the very type of an indefatigable and powerful worker in the great field of science."—*Observatory*, Jan., 1894.

We regret also to have to record the death of Dr. Adolphe Steinheil, chief of the optical firm of Steinheil & Sons, Munich, who died November 4, 1893, aged 61. Also of Friedrich Gustav von Bülow, who was the founder of the Observatory at Bothkamp, from which MM. Vogel and Lohse issued the three volumes of the Bothkamp observations, 1870–74.—*Observatory*, Jan., 1894.

THE SOLAR FACULÆ.*

GEORGE E. HALE.

The almost simultaneous discovery of the doubly reversed H and K lines in the spectrum of the faculæ, made by M. Deslandres and myself in 1891, was not altogether unexpected. Several years earlier, Professor Young had seen this bright pair of lines in the spectrum of spots, but their position at the extreme limit of the visible spectrum made satisfactory observation impossible. Professor Young found, however, that the bright lines were not confined to the spot itself, but extended on to the disc for a considerable distance. With the application of photography the difficulties of visual observation disappeared, and the bright lines were found, not only in the neighborhood of spots, but also in extensive regions irregularly distributed over the solar disc. A dark central line of double reversal, which had escaped the eye of the observer, was also clearly registered upon the photographic plate. With a sufficient dispersion it is occasionally found that the doubly-reversed lines extend entirely across the Sun. They are not uniformly bright, but have alternate maxima and minima of intensity. In the minima the lines sometimes seem to disappear completely, but it appears probable that with sufficient dispersion they could be photographed at any point on the disc. H and K are almost identical in appearance, except that the latter is always the brighter cf the two.

The question has been raised as to whether the bright lines with dark centres are true double reversals, or simply close double lines. A moment's consideration of the facts of the case ought to leave no room for doubt on this point. If a photograph of the spectrum is taken with the slit lying across the Sun's limb, it is found that the two bright lines of the double reversal unite into a single bright line at the edge of the disc, and this single bright line exactly coincides in position with the central dark line of the double reversal. This is shown in the photograph from which the cut was made, but the reproduction fails to bring it out.

The investigations of Cornu and other physicists, on the reversal of the lines of metallic vapors in the electric arc, offer most striking analogies to the phenomena just described. Through

* Communicated by the author.

the kindness of M. Cornu I have recently had the pleasure of examining some of his photographs of reversed lines. The investigations were confined to the ultra-violet, and the calcium lines which correspond with H and K were not specially studied. The reversals of other lines are similar, however, and will serve our present purpose equally well. An ultra-violet line of aluminium on one of the photographs reproduces the solar H and K reversals so perfectly that it might readily be mistaken for one of them were there any doubt as to its mode of production

In M Cornu's experiments an image of the arc was formed on the slit of the spectroscope. A portion of the metal, or one of its salts, was introduced into a cup-shaped cavity in the lower carbon, and vaporized by the passage of the current. Under these conditions the central portion of the arc, where the line of sight passed through the cool exterior vapor to the intensely heated vapor and the glowing carbon poles, showed the aluminium line reversed—two narrow bright lines enclosing a narrow dark line. At the edge of the arc, however, where the line of sight passed through only the cooler vapor of the exterior, the two bright lines united into a single bright line, corresponding in position with the dark line seen in the first case.

We thus arrive at a basis for the interpretation of the H and K reversals in the Sun. In the arc we have a reversed line produced by the absorption of the cooler vapor of the exterior. The chromosphere would seem to play the part of the absorbing vapor in the Sun. At the base of the chromosphere, or below it, is the hotter vapor corresponding with that at the centre of the arc. Here is the seat of the brilliant radiation of calcium, which produces the two bright components of the doubly-reversed H and K lines. The upper part of the chromosphere, on the other hand, acts as an absorbing screen, and produces the central dark line of the reversal. A photograph of the solar disc secured by means of the spectro-heliograph (using the K line), should therefore show not the entire chromosphere, but only its lower and hotter parts.

The distinction is an important one when it is remembered that photographs taken with the spectro-heliograph show the entire surface of the Sun to be mottled over with small, irregularly shaped, bright regions, which seem to form a nearly unbroken reticulation (ASTRONOMY AND ASTRO-PHYSICS, May, 1893, p. 450). This is not to be mistaken for the well-known "granulation" or the "réseau photosphérique" of M. Janssen. Neither does it seem at all probable that the brighter regions are merely elevations in the chromosphere, for if the upper part of the chromo-

sphere acts as an absorbing medium, and produces the dark central line of the H and K reversals, an increase in the depth of the chromosphere would certainly not diminish the absorption. It seems likely that the reticulation represents a true facular network, for the small faculæ seen without the spectroscope near the Sun's limb, and well described by Secchi (*Le Soleil*, German edition), are probably parts of the same reticulation. Probably no sharp distinction can be made between the base of the chromosphere and the upper surface of the underlying faculæ. The latter seem to be in intimate connection with the interior of the Sun, and one might therefore expect to find the H and K lines very bright in them.

In this connection it should be remarked that the H and K lines over spots are frequently somewhat narrower and less brilliant than on the disc, and the central dark line is often absent. I have explained this as probably due to the fact that we are here dealing with the radiation of the chromosphere overlying the *cooler* region of the spot. (ASTRONOMY AND ASTRO-PHYSICS, 1892, p. 815.)

In a recent paper on the "Physical Constitution of the Sun" (ASTRONOMY AND ASTRO-PHYSICS, 1893, p. 832), Father Sidgreaves has expressed his belief that faculæ are prominences seen in projection on the solar disc, and M. Deslandres has advocated a similar hypothesis in the December number of *Knowledge* (see also *Comptes rendus*, Nov. 27, 1893). As my own position in regard to the subject is evidently not fully apprehended by M. Deslandres, I shall endeavor in what follows to state it as clearly as possible.

In a note dated January 18th, 1892 (ASTRONOMY AND ASTRO-PHYSICS, February, 1892, p. 159), I wrote as follows in regard to the regions on the Sun's surface in which I had found the H and K lines to be doubly reversed: "On January 12th, 1892, it was found possible to photograph the *forms* of some of these reversed regions, using a moving slit apparatus just completed for our large diffraction spectroscope by Brashear. The K line in the fourth order spectrum was employed, as is customary in the case of prominences. The reversed regions are of great extent, and in appearance closely resemble faculæ. Several explanations may be suggested to account for them. They may be:—

"1. Ordinary prominences projected on the disc.

"2. Prominences in which H and K are bright, while the hydrogen lines are absent.

"3. Faculæ.

"4. Phenomena of a new class, similar to faculæ, but showing only H and K bright, and not obtained in eye observations or ordinary photographs because of the brilliant background upon which they are projected."

Subsequently I found, by comparing photographs of faculæ near the Sun's limb, made at the focus of a telescope in the ordinary manner, with photographs of the reversed regions made with the spectro-heliograph, that there was a very close agreement in form. For this reason I adopted the provisional name "faculæ" in subsequent references to the bright regions shown on spectro-heliograms, reserving an exhaustive discussion of the phenomena until sufficient material had been collected for that purpose. Part of this material, in the form of about three thousand spectro-heliograms and several hundred ordinary photographs of the Sun, had already been collected and partially reduced. It is intended that one of the first volumes of publications to be issued by the Yerkes Observatory shall be devoted to a discussion of these results. At the present time, and at a distance from my photographic and other records, I can discuss the subject only in a provisional way.

No one can doubt that prominences, and particularly eruptive prominences, are closely related to faculæ. To this point I have already called attention in the following words: ". . . In a great many photographs taken with the spectro-heliograph, faculæ are shown projecting above the Sun's limb. And the intimate relationship between faculæ and eruptive prominences is not less evident, especially in composite photographs showing faculæ and prominences on the same plate. When we consider that *eruptive* prominences probably rise from faculæ, it is not at all surprising that such prominences sometimes show a continuous spectrum in addition to their bright lines. For a violent eruption would naturally carry up with the prominence some "dust-like" matter from the facula, which would give a continuous spectrum." (Astronomy and Astro-Physics, November, 1892, p. 815.)

The projection of faculæ at the limb, while very frequently shown in short exposure photographs* of the Sun's disc, is rarely much greater than the average depth of the chromosphere. If faculæ are prominences seen in projection, or if they are always covered by prominences, as Father Sidgreaves and M. Deslandres hold, these projections should be much higher—*i. e.*, we should

* Obtained with the spectro-heliograph and also by direct exposure at the focus of a telescope.

always find a prominence above one of these projecting faculæ. As a matter of fact, the long exposure spectro-heliograms of the chromosphere rarely show prominences at such points; when prominences are present they are almost invariably eruptive, and of small extent at the base. But the projecting faculæ give the reversed H and K lines, even when no prominence is present. M. Deslandres states, however: "Les facules sont, par définition, les plages brillantes de la surface solaire, plages qui, à l'intensité générale près, donnent les mêmes raies noires que les parties voisines, et correspondent aux parties élevées, aux montagnes de la photosphère. Elles sont distinctes des flammes de calcium audessus d'elles." (*Knowledge*, December, 1893, p. 230).

It is probably true that the faculæ are ordinarily quite different from the prominences which sometimes cover them, but I cannot see that any evidence, other than a "definition," is offered to prove the absence of the bright H and K lines from their spectra. These lines may have their origin in hot calcium vapor distributed through the mass of the facula, or confined to its outer portion,* but I by no means consider it proved that the spectra of faculæ do not contain the bright H and K lines.

Leaving for a moment the question of faculæ, let us next consider whether prominences projected on the solar disc should be rendered visible by the spectro-heliograph. The reasoning which has already led us to the conclusion that the nearly continuous reversals of the H and K lines on the disc originate at the base of the chromosphere would seem to apply with greater force to the base of prominences, for here the H and K lines are apparently brighter than in the surrounding chromosphere. And, in fact, I have previously shown that certain outbursts on the solar disc, which, there is every reason to believe, are true eruptive prominences, have been photographed at the Kenwood Observatory with the spectro-heliograph (see ASTRONOMY AND ASTRO-PHYSICS, 1892, p. 611; *ibid*, p. 920, Plate xlvi., photographs of the eruptive prominence of July 15, 1892; *ibid*, 1893, p. 454). Such brilliant outbursts are quite exceptional, only seven having been found in over two thousand spectro-heliograms.

While it is thus certain that some prominences can be detected on the Sun's disc, it is equally certain that others cannot. Since

* It may even be that the lines originate in the chromosphere overlying the faculæ. In this case, the increased brightness of the lines, as compared with their brightness in other parts of the chromosphere, would have to be accounted for. Any such brightening, if due to the faculæ, would in all probability be confined to those parts of the chromosphere immediately overlying the faculæ, so that the forms of the latter would still be obtained in spectro-heliograms.

January, 1892, bright regions in which the H and K lines are reversed have been found only in the sunspot zones. Not having access to our records, I cannot give the northern and southern boundaries of this facular zone with accuracy, but I do not think we have photographed a single bright calcium region more than 70° north or south of the equator. During the same period our photographs have shown great numbers of prominences of higher latitude than 70°, and prominences have not been uncommon in the near vicinity of the poles. Thus there are bright prominences which give no indication of their presence when projected on the disc, for by no flight of the imagination could the small and evenly distributed meshes of the facular reticulation be supposed to represent the bases of such large and brilliant prominences. In the face of this difficulty, I prefer to wait for further evidence before adopting the conclusion that quiescent prominences in the sunspot zones can be photographed when projected on the disc.

M. Deslandres has suggested that the reversals of calcium on the solar disc be called "*flammes faculaires*, nom qui est en accord avec les faits, et évite toute ambiguité." (*Knowledge*, December, 1893, p. 231; *Comptes rendus*, Nov. 27, 1893). I regret that, for the following reasons, I cannot consistently adopt this name:—

1. The use of the identical term "flame," commonly employed to describe one variety of chemical combination, is objectionable, because we do not know that the solar and terrestrial phenomena referred to are in any way similar.

2. I have offered evidence to show that many faculæ are not covered by prominences, but themselves give the H and K lines.

3. Even if it could be shown that all faculæ are covered by prominences, it would seem unnecessary to replace the well-known term "prominence" by a less satisfactory synonym.

For the present, if one does not wish to commit himself by speaking of faculæ and prominences on the disc, the general term "calcium reversals" may perhaps be used, though it is not altogether free from objection.

As to the electric origin of the bright H and K lines in the Sun, (Deslandres, *loc. cit.*) it seems to me that the merely negative evidence at our disposal is not a safe foundation on which to build an argument. It is true that the hydrogen spectrum has not hitherto been obtained in the laboratory by the simple effect of heat, but it does not follow that this gas would not give a spectrum of bright lines when subjected to solar conditions. The H

and K lines of calcium had not been obtained artificially without electrical means until I succeeded in photographing their feeble radiations in certain flames (see ASTRONOMY AND ASTRO-PHYSICS, 1893, p. 452). At solar temperatures these radiations may be greatly strengthened, and the gradual shift toward the violet of the maximum of intensity in the calcium spectrum noticed with increased temperatures renders such a strengthening probable.

While it seems quite possible, and even probable, that electricity plays some part in solar phenomena, the evidence upon which to base any very positive statements appears to be lacking.

Before passing on to the discussion of certain practical questions, important in connection with future investigations of the Sun, let us sum up some of the conclusions to which we have been led.

The faculæ are the elevated regions of the photosphere. They form an irregular reticulation over the entire surface of the Sun,* and in the sunspot zones appear as irregular bright regions of varying extent. The spectrum of the faculæ is similar to the general spectrum of the Sun, but is somewhat brighter, and contains the doubly-reversed calcium lines H and K.† The hot calcium vapor from which these lines emanate may be diffused throughout the mass of a facula, or confined to its upper surface. In the latter case, the base of the chromosphere and the upper surface of the facula would practically coincide. The white-hot particles giving a continuous spectrum would ordinarily be found in the facula proper, and (more sparsely scattered) in the lower region of the chromosphere. *Eruptive* prominences are closely related to faculæ, and probably rise from them. It thus occasionally happens that a violent eruption carries some of the white-hot particles to a considerable distance above the photosphere. In such a case the prominence gives a continuous spectrum in addition to its bright lines. While some faculæ are covered by prominences, others do not appear to be so covered. Certain exceptionally bright eruptive prominences have been photographed in projection on the solar disc. Ordinary prominences in the region of the sun's poles are not shown in spectro-heliograms. Sunspots, even in their central parts, seem to be covered by the chromosphere (or by overhanging prominences). The chromosphere (or prominences) is frequently so bright as to completely hide small spots in spectro-heliograms.

* At least during the maximum period of sunspots.

† The less refrangible hydrogen lines may also be present, but under ordinary conditions they are too faint to be recognized.

As M. Deslandres has discussed the instruments and methods employed in my photographic investigations of the Sun, I may perhaps be allowed to express an opinion as to the most advantageous manner of continuing these researches.

The spectro-heliograph used in the greater part of my photographic work has a pair of slits arranged to move in the focal planes of the collimator and observing telescope of a large diffraction spectroscope, attached to a 12-inch equatorial refractor.* It has proved itself a thoroughly practical instrument, and from two to twenty or more photographs of the forms of prominences and calcium reversals have been made with it on every clear day (with few exceptions) since January, 1892. Neither my assistant, Mr. Ellerman, nor myself have experienced any difficulty in using a second slit 0.005 inch wide, and this width could be decreased were it considered desirable. By increasing the width of the second slit and making a series of photographs of the K line at various points on the solar disc with the slit stationary, the character of the double reversals can be studied. This method of successsive sections, which I first employed in 1891, seems to me quite as convenient as that used by M. Deslandres. As the slits always move together there are no troublesome adjustments to be made by hand.

But after studying and experimenting with a great variety of instruments, I came to the conclusion early in the year 1893 that a spectroscope with collimator and observing telescope parallel (or nearly so) to each other, and slits fixed in the axis of each, the whole instrument being arranged to move on wheels at right angles to the axis of the large telescope, would possess important advantages over all other forms of spectro-heliograph. An [redacted]

The question raised by M. Deslandres, in regard to the best dispersion to employ, is a most interesting and important one. On the one hand, a feeble dispersion would seem to offer important advantages on account of the narrowness of the K line and the greater brightness of the image. On the other hand, it must not be forgotten that the fainter details of the reversals may be lost if the dispersion is insufficient. M. Deslandres recognizes this fact when he states that the exceedingly faint H and K reversals in the general spectrum of the Sun are best obtained with a "spectroscope puissant" (*Knowledge*, December, 1893, p. 231); and again, when he remarks that for the study of the details of the reversals on the solar disc "une grande dispersion est nécessaire" (*Ibid.*, p. 232). This advantage of high dispersion is due to the fact that the width of the bright lines is not proportional to the dispersion, at least up to a certain limit. If by doubling the dispersion the lines were doubled in width, there would evidently be no change in their brightness as compared with the solar spectrum in which they lie. But, within certain limits, the brightness of the lines with respect to the solar spectrum increases with the dispersion. It is evident, therefore, that we must not employ too feeble a dispersion. I have found the fourth order spectrum of a Rowland grating (14,438 lines to the inch) very suitable, though I should have preferred prisms had circumstances permitted their use. The separation of the H and K lines at the focus of the observing telescope, where the photographic plate is placed, is nine millimètres. M. Deslandres uses a single prism, giving a separation of H and K of two millimetres, and magnifies the image of the second slit—and, consequently, the width of the K line—three diameters. The resulting separation of H and K on his photographic plate would thus be six millimetres, and the width of the K line two thirds the width of the K line in my instrument, if the line were supposed to have a width proportional to the dispersion. As has already been pointed out, the width of a line is not proportional to the dispersion, and it is probable that there is no great difference in the effective width of the line in the two instruments. Thus, any question in regard to displacement due to motion in the line of sight, width of the second slit, etc., would apply almost equally to both cases, but the greater dispersion of the grating would increase the brightness of the K line as compared with the solar spectrum, and fainter and more delicate calcium reversals should be obtained by its means.

It would probably be advisable to have a set of three prisms

for a spectro-heliograph, so that one, two, or three might be used as occasion required. The method of forming a magnified image of the second slit on the photographic plate, which we owe to Dr. C. Braun, formerly Director of the Haynald Observatory at Kalocsa, is in some respects a valuable one. Its principal defect—the widening of the K line and the second slit—can be avoided by enlarging the solar image before it enters the spectro-heliograph.

The large solar spectroscope which is to be used with the Yerkes telescope will be specially arranged for the study of the H and K reversals on the solar disc. Among the attachments to be employed for this purpose will be a pair of long slits, arranged to move in the focal planes of the collimator and observing telescope. The method of photographing the K reversal in successive sections of the disc will thus be similar to that hitherto employed with the Kenwood Observatory spectro-heliograph, but the exposures will be made automatically by an electrical device controlled by an astronomical clock. As I have already remarked, the spectro-heliograph for the Yerkes telescope will be arranged on another plan.

BERLIN, Dec. 19, 1893.

ON TWO GREAT PROTUBERANCES.*

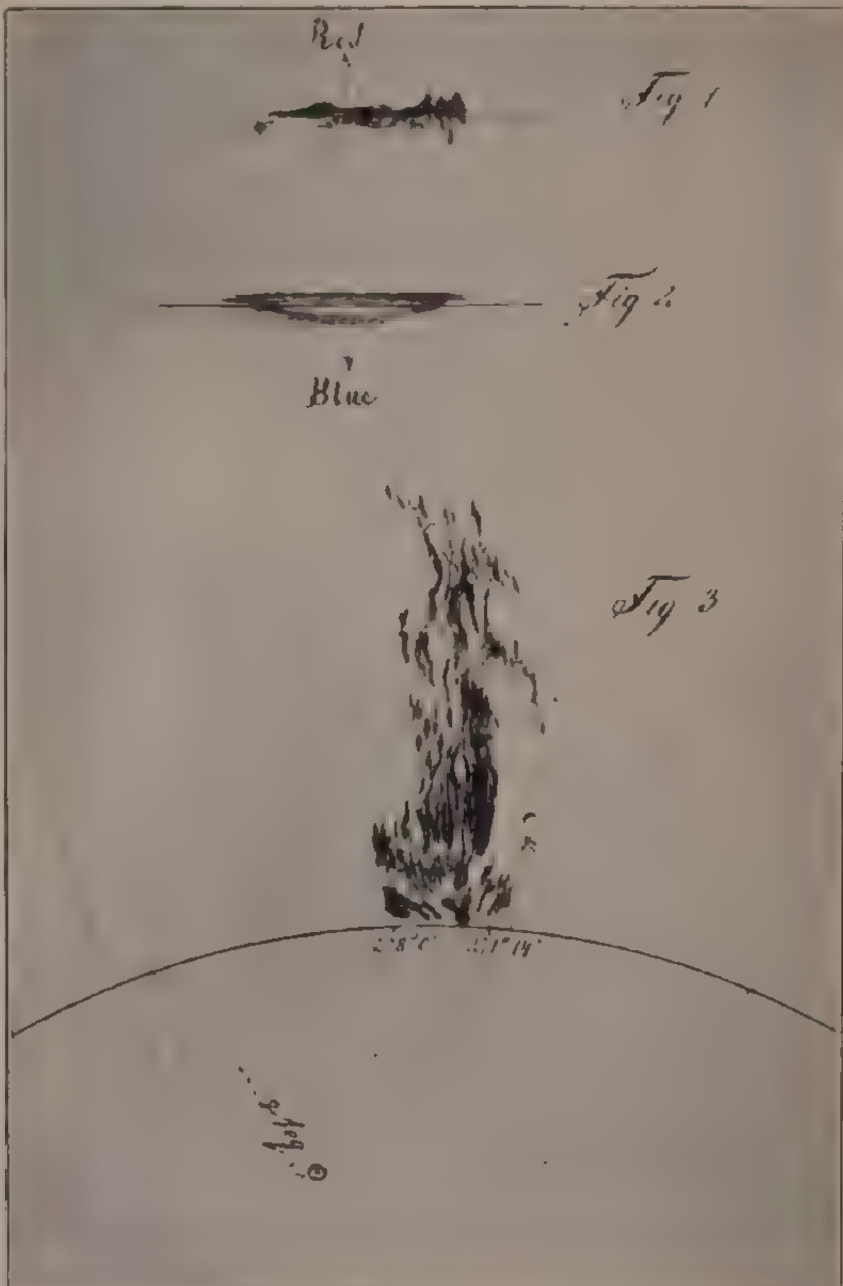
J. FENYI, S. J.

On the 19th and 20th of September, 1893, I had an opportunity of observing two protuberances which, on account of their enormous size and motion and their occurrence within the same twenty-four hours, are of great interest in the theory of these phenomena.

The first one was found on Sept. 19, 2 P. M., Greenwich mean time, as a bright protuberance on the west limb of the Sun, in position angle 271° 14′ to 278° 0′, *i. e.*, in −17° 0′ to −23° 26′ heliographic latitude. The entire mass already betrayed a large motion in the line of sight. At the lower part, at 278°, the spectral light spread out toward the red; but the entire remaining mass showed a much more rapid motion toward us, by a displacement in the direction of the blue end of the spectrum (Fig. 1). At 2^h 12^m I measured this displacement with a filar microm-

* Translated from the original communicated by the author. German geographical miles have been reduced to English miles.

PLATE VI.



PROMINENCE OBSERVED AT KALOCSA 2^h P. M. SEPT. 19, 1893.

eter, and found that the velocity was 297 kilometres per second. After completing a drawing (reproduced in Fig. 3) in which the lower parts are carefully drawn, but the upper somewhat hastily filled in, in order to represent the structure of the image, I began to measure the height of the protuberance by observing its passage across the slit. At the first transit I obtained a height of 368″.5, and not in conformity with the drawing, I found an interruption in the image, extending from the height of 175″ to 294″. Nine transits were observed, in which the seconds of time were counted continuously and the instants of passage noted. By this method not only were the intervals between transits exactly obtained, but also the absolute moments when the summit of the protuberance entered the slit, and the intervals of time between the separate measures of the investigation. The following table gives a view of the details of the rise of this protuberance. The times are given in mean time of Greenwich to the nearest tenth of a second. In computing the motions the figures noted in the observing book were used, and the necessary corrections were afterwards applied.

PROTUBERANCE OF SEPT. 19, 1893.

Greenwich Mean Time.	Duration of Transit.	Height of the Protuberance in seconds of arc.	Velocity of ascent in kilometers per second.	Computed Acceleration in meters per sec.
h m s	s	″		per sec.
2 21 5.4	24.7	368.5		
21 51.1	(23.6)	352.8	114.6	
22 37.1	25.6	383.2		+ 151
			126.1	
23 35.5	26.3	393.5		+ 1613
			214.0	
24 24.6	27.3	408.2		+ 3666
			406.0	
25 18.9	29.4	439.0		− 1204
			330.6	
26 15.1	31.2	465.7		− 3980
			103.9	
27 15.7	31.8	474.5		+ 2235
			240.1	
28 23.2	33.3	498.0		

In the computation 1″ at the limb of the Sun was taken to be 724 kilometres.

The protuberance therefore rose 129″.5 in 7^m $17^s.8$. From this we get for the mean velocity of ascent 212 kilometres or 132 English miles per second. The greatest observed height reached 8′ 18″, or 0.520 of the Sun's radius—in absolute measure 224,000 miles.

Before I measured the height, I endeavored to make a faithful

sketch of the form of the prominence, but in spite of all my care the drawing had little value, on account of the rapid change of the object; however, I had an opportunity during the work to observe the structure and the changes of form in some detail. Throughout the whole course of its appearance the entire object consisted simply of very bright luminous bands or strips scattered one after another in ragged forms, and apparently lying nearly at right angles to the limb of the Sun (Fig. 3). They were strikingly bright even in the highest parts of the prominence. The form as a whole was also like a band or stripe, which had no pronounced inclination, but stood erect nearly in the direction of the Sun's radius. The whole enormous structure had a large motion toward us. In the observations of transits already mentioned, the summit of the protuberance, when it came into view, was outside the image of the slit, and the displacement continued as far as the chromosphere. The amount of the displacement, which was subject to numerous fluctuations, was about the same as when I measured it in the beginning with the micrometer, and therefore represented a velocity toward us of about 300 kilometers per second. At $2^h\ 30^m$ nothing more was to be seen of the whole gigantic structure than a small protuberance, whose height, according to an estimate, was about 30″. The eruptive metallic line λ 6677 was seen bright in the chromosphere from 286° to 275°. A group of faculæ was going over the limb of the Sun.

Now it is very remarkable that although these eruptive phenomena were displayed with most unusual magnificence, so that even in this year of heightened solar activity they were then without a parallel, on the very next day there should occur a still mightier eruption at a place nearly opposite to that of the first. This time the whole occurrence, from beginning to end, was displayed before my eyes. It was a few minutes before 9^h on the 20th of September when I saw a singular bright form in position angle 115° 36′ – 112° 32′, which was produced by displaced spectral light, and was therefore outside the image of the slit (Fig. 2). Its unusually diffuse aspect, and especially the circumstance that there was no protuberance standing on that place, induced me to regard the appearance more attentively, and to measure its position. While I was doing this, and preparing to measure the considerable displacement towards the red, a very strong and unusually brilliant arch arose, which likewise showed a considerable displacement toward the red end of the spectrum. According to two measurements with the filar micrometer the displace-

ment corresponded exactly with a velocity of 255 kilometres per second.

In the mean time the circumstances had entirely changed. In the few minutes during which I was occupied in the manner already referred to, a mass of prominence-forms had arisen on the part of the limb between 102° and 118°, and in the middle, at 107° 16′ to 112° 0′, towered the brilliant prominence which is the subject of the following measurements. The rapidity of its development allowed no time to make a drawing, and a rough sketch which was made in the greatest haste is without value. It will perhaps be sufficient to remark, in this connection, that this protuberance was strikingly similar to the one already described, not only in its outline, but particularly in its band-like structure. The direction of the whole eruptive stream was likewise sensibly coincident with the Sun's radius. I now—at $9^h\ 7^m$—hastened to measure the height of the already enormous mass by observing its passage across the slit, in the manner described above. During the eight transits, each of which lasted from 37 to 60 seconds, I had time to include several conspicuous intermediate points in the measurements. The first transit gave a height of 8′ 6″ or 352,000 kilometres. As this height was attained in about 12 minutes, the mean velocity must have been 488 kilometres per second, a result which is in tolerable accordance with subsequent exact measurements. The following table shows the progress of events during the eight transits.

PROTUBERANCE OF SEPT. 20, 1893.

Greenwich Mean Time h m s	Duration of Transit s	Height of the Protuberance in seconds of arc	Velocity of ascent in kilometers per second	Computed Acceleration in meters per sec per sec.
9 8 13.4	36.8	486.0		
			286	
9 3.5	38.3	505.5		+ 2904
			437	
9 56.8	40.7	538.1		− 255
			423	
10 51.7	43.2	570.7		− 1320
			347	
11 50.9	45.4	599.4		− 2800
			171	
12 56.2	46.6	615.0		+ 4740
			498	
14 7.2	50.3	664.5		− 3280
			262	
15 18.3	52.3	690.6		

From this we see that the protuberance finally reached the enormous height of 11′ 30″.6, or 0.722 of the Sun's radius,—in absolute measure 500,000 kilometers. In an interval of $7^m\ 4^s.9$

the protuberance rose 204″.6, from which we obtain an undoubtedly correct mean velocity of 214 miles per second. The maximum observed velociey was 309 miles.

This protuberance, like the first, showed throughout its entire height, (*i. e.*, throughout its passage across the slit), a displacement toward the red, which, according to two measurements, corresponded to a motion of from 150 to 159 miles in a direction away from the Earth.

After $9^h\ 15^m$ the sky became cloudy; but the dissolution of the prominence had already set in. The last two transits were already somewhat uncertain on account of the faintness of its highest parts.

The occurrence in the same 24 hours of two such vast and infrequent outbreaks in the gaseous envelope of the Sun is very surprising. In the course of my observations of the Sun's limb, which are made daily whenever possible, I have met with no similar occurrence in the whole of the present year (1893). In this period of increased solar activity protuberances of 70″ height are to be found every day, and those of 120″ are not infrequent. The greatest heights observed in this year, exclusive of those now under consideration, were 260″ on March 28, 215″ on June 29, and 294″ on Sept. 23. When therefore as in this case, heights of 498″ and 690″ are reached, and enormous motions are exhibited, by prominences which are separated in time by an interval of only 19 hours, we are led to suppose that these two extraordinary apparitions have some real though perhaps remote relationship. This view has support in the circumstance that the prominences were situated on nearly diametrically opposite parts of the Sun, and were similar in form, structure, and progress of development. Both appeared to consist simply of bright bands or strips, and seemed to shoot upwards fiery streams of matter which were not sensibly inclined to the vertical, (leaving out of consideration the motion in the line of sight), and which did not spread out at a great height; while eruptions observed in other years presented quite different forms and cources of development. Some emphasis might also be laid on the fact that both were very bright up to enormous heights. In the sketch of the first prominence it was noted that the brilliancy was extraordinary half way to the summit. On observing even the last transits on the 20th of September, the image was bright at a height of from 351″ to 637″ — only the upper part was faint. Metallic vapors, which were indicated in the base of the first protuberance by the presence of the eruptive

line λ 6677, were not noticed in the base of the second. At that place on the surface of the sun no luminous form could be recognized which could be considered as related to the eruption.

When we regard these appearances in their entirety and in detail, and seek to understand their nature, we may well find ourselves in doubt whether we can regard them as produced simply by the mechanical motion of matter; we feel driven to assume an apparent motion, having its origin in the rapid propagation of a physical or chemical process. The simplest representation of the appearances which have been described is offered in the theory of H. Brester, which regards the prominences as nothing more than the luminous condition of that part of the Sun's gaseous envelope where the dissociated elements of hydrogen are cooled down to the point of recombination. We must confess that not only do the enormous velocities observed in these prominences find a welcome explanation in this theory, but that the details of the development and the banded structure are brought into better accord. The cloud-like, tattered structure of protuberances in general, and especially the often-observed process of dissolution, which is almost perfectly like that of our own clouds, would be exactly what we should expect as a consequence of such local explosions or luminous outbreaks in the Sun's gaseous envelope. The theory has naturally many difficulties to encounter which I cannot enter into here. I may, however, be allowed to point out one argument that demonstrates the impossibility of regarding as successful any of the attempts yet made to explain the great motions of the prominences as the propagation of a luminous condition; and that is the undoubted fact of the displacement of spectral lines, which requires the assumption of motions as great as those which are directly observed in the rise of prominences. I regard it as impossible that a displacement of the spectral lines can be produced by the mere advance of a physical or chemical process without a progressive motion of the light emitting molecules which constitute the mass. By accepting such an explanation as this we should leave the firm ground of experience without having any support whatever from theory.

Before we can assume an explanation of this kind, it must be shown that in such progressive explosions the molecules have at least a temporary motion in the direction of propagation, corresponding in amount to the enormous velocities found by observation. Moreover, the direct proof that a displacement of the spectral lines takes place in explosions, in a manner determined by the direction of propagation, is not without the province of experi-

ment. If the explosion of our known gaseous combinations takes place at the rate of only a few kilometers per second, the velocity of hundreds of kilometers in tne Sun may well be ascribed to the greater heat there or to the nature of the dissociated atoms; a displacement, even though a minute one, must therefore be demonstrable by experiment in our laboratories. The infrequent strongly disturbed prominences could then be regarded as explained. It would then be the turn of the common, quiescent prominences, which all day long may be seen floating high above the limb of the Sun, without undergoing any important changes.

Since the assumption of electrical forces only embarrasses explanation in a province which is still obscure, we must confess that the nature of the solar protuberances remains an open question.

ON A CERTAIN LAW IN THE SPECTRA OF SOME OF THE ELEMENTS.*

C. RUNGE.

The list of standard wave-lengths lately published by Rowland in ASTRONOMY AND ASTRO-PHYSICS includes several of those doublets and triplets of lines, whose width and distribution has been studied by Prof. Kayser and myself. It is of interest to inquire whether Rowland's most exact measurements confirm the law that the doublets or triplets of the same series give the same difference of oscillation-frequencies. In the following table I have collected all the instances and I only regret that they are not [redacted]

	λ	1/λ	Difference.	Weight.	Greatest possible error.	Deviation from mean.
Sodium	6160.970	16231210				
			17246	14	52	19
	6154.431	16248456				
	5896.154	16960208				
			17196	12	58	31
	5890.182	16977404				
	5688.434	17579531				
			17239	10	62	12
	5682 861	17596770				
		Mean:	17227			
Magnesium	5183.792	19290897				
			40728	7	74	11
	5172.871	19331625				
			20089	7	74	11
	5167.501	19321714				
	3838.430	26052318				
			40678	2	136	[illegible]
	3832.446	26092996				
			20089	2	[illegible]	39
	3829.505	26113085				
		Mean:	40717			
			20078			
Calcium	6162.383	16227489				
			105900	14	53	[illegible]
	6122.428	16333389				
			52288	14	54	[illegible]
	6102.891	16385677				
	3973.835	25164608				
			105913	[illegible]	127	11
	3957.180	25270521				
			52128	[illegible]	128	132
	3949.034	25322649				
		Mean:	105902			
			52260			
Aluminium	3961.680	25241817				
			112092	25	128	11
	3944.165	25353909				
	3092.962	32331467				
			112133	9	209	30
	3082.272	32443600				
		Mean:	112103			

The deviation from the mean value is in each case considerably smaller than the greatest possible error except for the difference between the second and third line of the second Calcium triplet.

But even here the deviation might be made smaller than the greatest possible error by giving the second triplet a greater weight. The weight 5.5 for instance instead of 3 makes the mean 52243 and the deviations from the mean value 45 and 115 both within the limits.

There is one more triplet of Calcium lines in Rowland's list, but it does not belong to the same series as the two given above. As Kayser and I have shown, the first two lines of the triplets that belong to the same series are a little nearer to one another. But the second and third lines give the same difference of oscillation frequencies in both series. Rowland has

λ	$1/\lambda$	Difference
4435.852	22543584	52176
4425.609	22595760	

The deviation of the difference from the mean value 52243 is 67, while the greatest possible error is 102.

Rowland gives three more Aluminium doublets. They belong to the same series as the second one of the two given above. They ought properly to be called triplets, for the less refrangible line is double, the weaker component giving with the most refrangible line the constant difference of oscillation-frequencies. Unfortunately Rowland's list does not include the weaker component, except in the case mentioned above.

From the accordance of Rowland's determinations with the law of the constant difference of oscillation-frequencies, one may draw two conclusions; first, that this law is in all probability absolutely correct, and secondly, that Rowland has not overrated the exactness of his measurements.

ON THE MOTION OF ζ HERCULIS IN THE LINE OF SIGHT.*

A. BELOPOLSKY.

I desire to call the attention of spectroscopists to the star Herculis, (R. A. = $16^h\ 37^m$, Decl. = $+31^\circ\ 47'$; 1895), as it seems to have a large motion in the line of sight. It is well known that this star is double; the principal component is of the third magnitude, and the companion, whose mean distance is about 1″

* Translated from *A. N.* 3184. Velocities in Herr Belopolsky's article are given in German geographical miles, usually to the nearest tenth. They are given here in kilometers to the nearest unit. As 1 geographical mile = 7.42 kilometres, the precision expressed is nearly the same.—*Tr.*

has a magnitude of 6.5. The time of perihelion passage is to be expected toward the end of the present century.

It is impossible to decide how far the spectrum of the principal star is influenced by that of the companion, but it can safely be assumed that with the slit-width which was used, and with an exposure of one hour, no trace of the spectrum of the companion would be produced on the plate. The deviations which occur in the measurements might otherwise, perhaps, be explained by this circumstance.

The spectrum was obtained with the larger spectrograph (two compound prisms) of the Pulkowa Observatory, in connection with the 30-inch refractor. Iron and hydrogen were used for the comparison spectra.

The sky was not in general very transparent during the observations, and on only one day, (May 18), was a photograph obtained on which the lines were sharp and traceable far into the violet, while on the remaining days, (May 22, June 2, 3, 4, 14 and 16) the photographs were taken through thin cirrus clouds, which strongly absorbed the violet part of the spectrum

The slit was placed in the focus for the $H\gamma$ rays, and the camera objective was adjusted by experiments on the $H\gamma$ line.

It is an extremely difficult matter to keep a star on the slit with the great refractor, as the right ascension and declination screws are arranged only for rather coarse motions, while the pressure of the finger on the eye-end of the tube suffices to make the spectrum in the camera disappear. It was only after several mechanical devices had been added that we were able to successfully photograph the spectra of stars down to the fourth magnitude.

The spectrum of ζ Herculis belongs to Vogel's class II, and hence it was possible to obtain fairly accurate measures of the motion in the line of sight.

The measurements were carried out according to the Potsdam method, but the results are not final, since the value of a revolution of the micrometer has not yet been determined for different temperatures. The investigations have not yet been finished. The spectrograph was not attached to the 30-inch refractor until the beginning of April in the present year. On this account the computations have also not been made as rigorously as at Potsdam.

The following values of a revolution of the micrometer have been used:

At λ 430.8 $\mu\mu$	1 rev.	corresponds to	198	kilometres.
434.1	"	"	217	"
438.4	"	"	232	"
440.5	"	"	237	"
441.5	"	"	238	"

Each of the spectrograms (except that of May 18) was measured in two ways: (1), the displacement of the most suitable lines, referred to the Hγ line, was measured according to the method of Vogel; (2), the displacement of the star lines, with reference to the artificial iron lines was measured directly.

I must remark that the Geissler tube was made luminous, (at a distance of 35 centimetres in front of the slit), for only a short time (3 to 5 minutes) in the middle of the exposure, while the spectrum of iron was photographed either at the beginning or at the end.

The results of the measurements are now given, as follows: The displacement is represented by D.

1893, May 18, 11^h 15^m Pulkowa Mean Time.

First Method,	D = − 0.309 rev.	first series.
	D = − 0.307 "	second series, on another day.
Direct displacement	D = − 0.316 "	first series.
of Hγ line,	D = − 0.323 "	second series.

The resulting velocity is:

First method	— 66.6 kilometrs.
Direct displacement of the Hγ line	— 69 "

May 22, 11^h 0^m Pulkowa Mean Time.

First method	D = − 0.356 rev.	first series.
	D = − 0.360 "	
Direct displacement of the Hγ line	D = − 0.393 "	

[redacted]

June 2, 11^h 40^m Pulkowa Mean Time.

First method, D = — 0.328 rev.

Direct displacement of the Hγ line		D = — 0.341 rev.
" " " " iron line	λ 430.8	D = — 0.302 "
" " " " " "	438.4	D = — 0.325 "
" " " " " "	440.5	D = — 0.304 "
" " " " " "	441.5	D = — 0.300 "

The resulting velocities are:

by displacement of the Hγ line		— 73 kilometers.
" " Iron line	λ 430.8,	— 60 "
" " " "	438.4,	— 75 "
" " " "	440.5,	— 72 "
" " " "	441.5,	— 71 "
	Mean,	— 70 "

First method, — 71 kilometres.

June 3, 11^h 5^m Pulkowa Mean Time.

First method, D = — 0.289 rev.

Displacement of iron line	λ 438.4,	D = — 0.290 "
" " " "	440.5,	D = — 0.288 "
" " " "	441.5,	D = — 0.279 "

The resulting velocities are:

by displacement of the iron line	λ 438.4,	— 67 kilometres.
" " " "	440.5,	— 68 "
" " " "	441.5,	— 66 "
	Mean,	— 67 "

First method, — 62 kilometres.

June 4, 11^h 6^m Pulkowa Mean Time.

First method, D = — 0.276 rev.
Displacement of the Hγ line, D = — 0.275 "

Displacement of the iron lines:

	λ 438.4	λ 440.5	λ 441.5
First series,	0.325 rev.	0.320 rev.	0.266 rev.
Second "	0.296 "	0.315 "	0.281 "
Third "	0.301 "	0.268 "	—
Fourth "	0.323 "	—	—
Mean	0.314 rev.	0.301 rev.	0.274 rev.

The resulting velocities are:

by displacement of the Hγ line		— 59 kilometers.
" " " iron "	λ 438.4,	— 73 "
" " " " "	440.5,	— 71 "
" " " " "	441.5,	— 65 "
	Mean	— 67 "

First method, — 60 kilometeres.

June 14, $11^h\ 4^m$ Pulkowa Mean Time.

First method	D = — 0.255 rev.
Displacement of iron line λ 432.6,	D = — 0.271 "
" " " 438.4,	D = — 0.254 "
" " " 440.5,	D = — 0.279 "
" " " 441.5,	D = — 0.248 "

The resulting velocities are:

by displacement of the iron line λ 432.6,	— 56	kilometres.
438.4,	— 59	"
440.5,	— 66	"
441.5,	— 59	"
Mean	— 60	"

First method, — 56 kilometres.

June 16, $11^h\ 10^m$ Pulkowa Mean Time.

First method,	D = — 0.263 rev.
Displacement of the Hγ line,	D = — 0.265 "

Displacement of the iron lines:

	λ 432.6	λ 438.4	λ 440.5	λ 441.5
First series,	0.311 rev.	0.310 rev.	0.285 rev.	0.301 rev.
Second series,	0.299 "	0.283 "	0.278 "	0.295 "

The resulting velocities are:

by displacement of the Hγ line,	— 57	kilometres.
" " " iron line λ 432.6,	— 63	"
438.4,	— 69	"
440.5,	— 67	"
441.5,	— 71	"
Mean,	— 65	"

First method, — 57 kilometres.

[redacted]

DISPLACEMENT OF THE $H\gamma$ AND IRON LINES.

1893.	Observed motion.	Reduction to Sun.	Motion relative to Sun.
May 18	— 69 km.	+ 1 km.	— 68 km.
22	— 88	— 1	— 89
June 2	— 70	— 4	— 74
3	— 68	— 4	— 72
4	— 68	— 4	— 72
14	— 60	— 6	— 66
16	— 65	— 7	— 72
			Mean, — 73 km.

The mean of the two methods for each day gives:

1893.	Motion relative to Sun.
May 18	— 68 km.
22	— 84
June 2	— 75
3	— 67
4	— 66
14	— 64
16	— 69
	Mean, — 70 km.

Although the deviation of each day from the mean are greater than might be expected from a consideration of the Potsdam probable error, we must not at once draw conclusions from this circumstance, for there seems to be a constant difference between the two methods used, which very probably depends upon the aspect of the iron lines in measuring. There is always a tendency to estimate the displacements greater than they really are. I did not succeed, however, by independent measurements and by studying the arrangement of the precipitated grains of silver, in reducing the differences of the direct displacements, particularly in the case of May 22, for which day the two methods give considerable deviations from the mean in the same direction.

For comparison I give here the motion of α Boötis, determined by the displacement of iron lines. It will be seen there is no material difference between my determinations and earlier ones, a fact which indicates the spectrograph was correctly adjusted.

α BOÖTIS. 1893, MAY 6, PULKOWA.

Iron line λ 429.9,	+ 2.4 kilometers.
432.6,	+ 4.2
438.4,	+ 7.4
440.5,	+ 3.3
441.5,	+ 4.7
Mean	+ 4.4 kilometres.

Reduction to Sun, — 10.1 km.
Motion relative to Sun, — 5.7 " = — 3.6 English miles.

The determinations of Vogel, Scheiner, and Keeler give;

v	-4.4	English miles.	(6 determ.	First method).	
s	-5.2	"	"	(6 "	" ").
k	-4.3	"	"	(3 "	Displacement of the D lines).

ON THE NEW STAR IN AURIGA*

H. C. VOGEL

Seeliger† has advanced a hypothesis, founded on the most important observations of the Nova, which regards the occurrence from several new points of view, and is in many respects worthy of careful attention. He believes that his considerations enable him to avoid the difficulties which are encountered in explaining the great relative velocities, or on the other hand in the assumption of very great masses, on the hypothesis of the existence of two compact heavenly bodies.

In recent times it has become a generally accepted view, largely through the instrumentality of celestial photography, that the interstellar spaces are filled with more or less extensive aggregations of very sparsely distributed matter. There is consequently no improbability in the entrance of a heavenly body into such a cloud, and certainly a much greater probability than that two compact bodies should pass at a very short distance. "But as a body enters such a cosmical cloud, its surface will begin to be heated no matter what the constitution of the sparsely distributed material may be. In consequence of the superficial heating, vaporized products will form around the body, which will in part become detached from it and quickly assume the velocity of the neighboring parts of the cloud."

If observed spectroscopically, the star thus rendered incandescent would show two superposed spectra; one continuous, and in consequence of the layer of glowing vapor in front of the body crossed by dark absorption lines, the other from the nature of the occurrence, consisting of bright lines. Both spectra would be displaced in proportion to the relative motion in the line of sight. On the whole the resulting appearance would very closely resemble that which was actually presented by the Nova.

"Of very great importance, however, is the investigation of the question whether we can in this way arrive at a plausible explanation of the great relative velocity indicated by the two spectra.

* Continued from January number, page 57.
† A. N. 3118. ASTRONOMY AND ASTRO-PHYSICS, Dec. 1892.

On the approach of the body, the cloud would evidently be lengthened in the direction of approach. This lengthening, and likewise the relative velocity of the individual cloud-particles with respect to the body, would grow with the increasing proximity of the latter. Without some definite provision in regard to the structure of the cloud, it is difficult to give any detailed representation of the phenomenon that will ensue, and we must content ourselves with considering some special case which will allow of closer investigation. If we assume, for example, that the separate particles of the cloud are in general influenced only by the attraction of the body, they will describe hyperbolas around the latter with its center as focus. Their greatest relative velocity will diminish rapidly with their distance from the body so that the neighborhood of the latter will be filled with particles having very different velocities. It is easy to see that no extravagant assumption is required to obtain very great velocities for the particles which pass close to the surface of the body,—velocities such as have been proved to exist in the case of Nova Aurigæ, and this even when the initial velocity of the particles is very small. It also follows from what has been shown above, that the spectral lines of particles moving away from the body with such different velocities must be greatly widened; moreover, not only is not the slightest difficulty encountered in explaining the different brightness of various parts of these lines, but the existence of such maxima of brightness follows as a necessary consequence. This point does not seem to me to be unimportant since it cannot be deduced from the hypothesis of the close passage of two compact masses, but leads to the very improbable assumption that there are several moving bodies of this kind.

"As long as the body moves within the cosmical cloud, the appearances just described will be continually reproduced, and it follows that the characteristic features of the spectrum, apart from minor changes determined by all the circumstances of the case, must as a whole remain unchanged for a considerable time.

It is also not surprising that during this time the brightness of the star should undergo little variation, but that it should fall off pretty rapidly after the emergence of the body from the cloud. This also agrees well with the observed light curve of the Nova."

Seeliger assumes that the star entered the cosmical cloud in the beginning of December, and left it not long before the beginning of March. He seeks to decide the question, how the great relative velocity could persist for so long a time, by comparing the motion of the star against the resistance of the cloud with that

of a meteor in the upper regions of our atmosphere, and arrives at the conclusion that the retardation would not necessarily be perceptible.

That in spite of this small retardation, enough kinetic energy is transformed into heat to cause the superficial incandescence of the star, is made the subject of a mathematical investigation by Seeliger, with the result that "we can assume that the density of the cosmical medium is very small compared with that of the extremely tenuous air in which the meteor is brought to incandescence, and still obtain the necessary quantity of heat. It is worthy of remark that all the numerical values can be varied within very wide limits without danger of contradiction."

In the second apparition of the Nova, Seeliger finds a confirmation of his views, for it is inherently probable that the supposed nebulous or dust-like clouds are more numerous in certain regions of space than in others, and it is also permissible to make various assumptions as to the distribution of density in the matter comprising them.

At the first glance there is something remarkably captivating about this hypothesis, but on a closer comparison with the observations, to which I will confine myself, doubts of no inconsiderable moment arise whether it really appears adequate to a complete explanation of Nova Aurigæ. However the case may be, I fully concur with the opinion of Seeliger that the hypothesis, which deals with entirely possible conditions, is to be regarded as a valid explanation of the appearance of certain new stars.

Under the supposition that the body moving through the cloud, and the cloud itself, have no unusually great velocities in comparison with the mean velocity of stars in space (for the main object of the hypothesis is to avoid the assumption of such velocities) and that the body entering the cloud is moving toward us, the particles of the cloud, shortly before the entrance of the body, would, by virtue of its attraction, move toward and past it. They would carry with them particles of its atmosphere, and, by their passage close to its surface, acquire a more or less great velocity in a direction opposite to the Earth. The spectrum of the body, whose surface on entering the cloud is thus raised to incandescence, would probably be continuous and show absorption lines, and on it would be superposed the bright line spectrum of the detached particles of the body's atmosphere. The centers of these lines would at first be relatively displaced, the bright lines toward the red, since with Seeliger we have to assume that the

particles of the cloud would, in general, move away from us. But when the body is once in the cloud, particles rush upon it from all sides, those grazing its surface would acquire motions making all possible angles with the line of sight. The bright lines would consequently appear very greatly broadened, but a displacement of the centers of the bright and dark lines could no longer be supposed to exist, since the motions of body and cloud would not be appreciable in comparison with the enormous velocities which would be imparted to the separate parts of the cloud by the attraction of the body. At the exit of the body we should expect appearances similar to those at its entrance, but the bright lines would be displaced toward the blue.

Now the body, at the time of the first spectroscopic observations, was already near the middle of the cloud, and its exit, according to Seeliger, occurred in the beginning of March. During this interval, however, the spectrum remained essentially unchanged, and the observatians have therefore not corresponded with the appearances which were to be expected.

It should further not remain unmentioned that Seeliger has fixed his attention on only the relative motion, determined by the displacement of the bright with reference to the dark lines, and has overlooked the great velocity of over 400 miles per second with which the star having absorption lines in its spectrum is actually moving in space. But this fact must not be left out of consideration in any hypothesis regarding the nature of Nova Aurigæ.

If, now, a body should move toward us with such a velocity, through a cosmical cloud, the conditions would be materially different and the resulting spectrum would perhaps approximate more closely to the observed one, inasmuch as it may be assumed that the bright lines would be constantly displaced toward the red, even when the body was in the middle of the cloud. At the same time the bright and dark lines would overlap as far as their centres and therefore would not appear to be separated by their full breadth; for a great number of the particles of the cosmical cloud which produce the bright lines would have the same velocity as the penetrating body, as with such great velocities vortex motions would be inevitable. In this connection there remains unexplained, why the smallest motion relative to the body with the absorption spectrum is not zero, but something like 360 miles per second, how under the assumed conditions any other appearances than a broadening of the bright lines could be produced, how therefore, with uniform distribution of matter, or of the sep-

arate particles in the cosmical cloud, so prominent maxima of intensity could be formed in these bright lines, and why these maxima should have been subject to only very slight changes in their relative positions.

In the second apparition of the Nova, when practically only an emission spectrum was produced, the essential data for testing the hypothesis are wanting; but we should naturally expect to find appearances like those which accompanied the first outbreak, and above all, if we assume with Seeliger that the compact body entered an outlying portion of the supposed nebula, a strong heating of its surface.

The opinion that the Nova could be explained by the encounter of a heavenly body with several bodies, forced itself upon me immediately after the first observations, and I have since been more and more confirmed in this view by the further observations which have been made. It is true that the question, whether there was not too small a probability in such an encounter of heavenly bodies, at first gave rise to doubts, but they soon disappeared on considering that, according to the Kant-Laplace hypothesis, it is difficult to conceive of a large celestial body unaccompanied by attendants. It is really surprising that in all the hypotheses accounting for new stars, the assumption of these simple conditions should have been left out of consideration.

If we assume that a body, having a mass of the same order as that of the sun, should suddenly pass close to a system like our own, whose central star had lost its light by gradual cooling, then enormous disturbances would be produced, and the collision of individual members of the system and the consequent production of luminous outbursts, would be inevitable.

Let us now suppose that the body which gave the continuous spectrum with absorption bands in the composite spectrum of the Nova, and which, as is well known, was moving in space with a velocity of about 400 miles per second, came close to a system moving with no more than the ordinary velocity, in a direction with regard to which no special assumptions need be made.

By the passage close to one of the larger or several of the smaller bodies of the system, perhaps also by direct collision with smaller bodies, the star entering the system would suddenly be brought to a high state of incandescence. At the time of the spectroscopic observations the body was in a part of the supposed system which was somewhat thickly filled with little bodies, and these, by their close passage and by partial collisions,

maintained the brilliant incandescence of the surface and atmosphere of the penetrating body, which the extension of the continuous spectrum into the violet shows that they must have possessed. In this way some of the bodies themselves acquired an excessively high temperature and a more or less high velocity, giving rise to the spectrum with bright lines, and thus producing the same effect as the particles of the cosmical cloud in Seeliger's hypothesis. There is however this importane difference, that the motion of the little bodies was regulated by the central star, they moved in an actual stream against the penetrating body, and therefore could not have moved toward it from all directions.

It is now explicable that the bright lines should have appeared broadened, displaced, and diffuse; nor does it any longer appear strange that the least displacement of the bright lines (one of the edges) did not coincide in position with the middle of the dark lines, but represented a small motion in space which was perhaps not greatly different from that of the supposed stellar system.

The atmospheres of the central body and larger bodies of the system would also be greatly heated by inevitable disturbances of the surface-levels and consequent eruptions, and if these causes were not sufficient to produce a higher temperature in the surfaces of the bodies than in their atmospheres, (which however we might expect in the case of a body heated by the fall of smaller bodies from without), they would nevertheless give rise to a spectrum in which bright lines would predominate. In this then we have a simple explanation of the intensity-maxima in the bright hydrogen lines, which indicate a small motion in space, and which at first had the greatest intensity.

We also find a satisfactory explanation, based on assumptions whose probability has ample support in a system subject to so many disturbances, of other observed phenomena, such as the second maximum of brightness which were seen so long in the hydrogen lines, and the temporarily appearing third maximum in the same lines, and even, assuming that they are not to be regarded as reversals, of the fine bright lines which appeared in them. It should be remarked, by the way, that from this point of view they are not explained by any of the hypotheses previously mentioned.

In continuation I may still specially add that the anomalies observed in the measurements of the D lines,—namely, that the displacement of these lines in the star spectrum referred to the artificial lines was found to be less than that of the hydrogen lines, (Huggins, Becker),—as well as similar observations of the

finer chromosphere lines (Campbell) are at once intelligible, for the spectra of the different bodies need not necessarily exhibit the same lines. And I may still further mention that the second outburst of the Nova in the autumn of 1892 may be ascribed to the meeting of the flying body with some distant member of the stellar system through which it was supposed to be passing. The most reliable proof of the correctness of the views here set forth would, in fact, be secured, if it could be shown with greater certainty that there have been such changes in the wave-lengths of the bright lines in the spectrum now visible as are indicated by the observations of Campbell, for the assumption of an orbital motion would then be allowable.

I will not however lose myself further in details, for my purpose was in the main only to show that the meeting of a body moving at random through space and an ordered system of bodies has not too small a probability, for no objection can be raised to the assumption of a planetary system in connection with a fixed star and also to show that in the assumption of such a system traversed by a body moving with the abnormal velocity of from 400 to 460 miles per second and in which the body might remain for weeks or months, as it would require, for example, five months to pass through our own system), the principal phenomena observed in Nova Aurigæ find a natural, unstrained explanation.

ON THE NEW STAR IN AURIGA.*

H. SEELIGER.

The phenomena presented by this remarkable star have not yet come to an end. It is for this reason that I have hitherto refrained from adding anything further to my first communication on the subject in A. N. 3118.† The opinions which I there announced were necessarily founded on assumptions of the greatest possible generality, since it was not permissible in the discussion to anticipate the results of observations which at that time were still incomplete. The reappearance of the Nova in August, 1892, occurred, it may be remarked, nearly at the time when my paper was published. It seems to me that the time for taking up the investigation again has not even yet come, but that it should

* Translated from A. N. 3187.

† See also ASTRONOMY AND ASTRO-PHYSICS, December, 1892.

wait for further developments. Herein also are contained the reasons why I have not entered into a discussion of remarks which have been made here and there on the nature of the occurrence, and it is not my intention to discuss them now. Nevertheless, certain recently published remarks which Herr H. C. Vogel* has appended to a collection of observed appearances of the Nova, seem to make it advisible that I should enter into a brief consideration of the subject, for Herr Vogel's paper contains views which I cannot recognize as correct, and the propagation of which I cannot regard as useful to science.

I had assumed that the outburst of the new star was caused by its contact with a cloud of thinly scattered matter In regard to the physical constitution of the material, purposely nothing was said, in order not to anticipate special appearances which might possibly be required to satisfy the observations. When therefore my views are characterized from the one side as a modification of the meteoric hypothesis, the agreement with the real facts is as small as in the interpretation that the cosmical cloud could not be dust-like in nature. A clear view of the motion of such a cloud relatively to the star is obtained by applying the elementary principles of mechanics. "On the approach of the body the cloud would evidently be lengthened in the direction of approach. This lengthening, and likewise the relative velocity of the individual cloud-particles, would grow with the increasing proximity of the latter."†

The motion of the separate particles will evidently depend upon the physical structure which is assigned to the cloud. It will be different with a dust-like constitution from what it would be with a fluid or gaseous one; but in any case the phenomena produced will be essentially those demanded by the hypothesis. Thus, what will take place is as follows: (1) In consequence of the approach of the cloud and the body due to attraction, a stream of matter appearing to come from a definite direction, will pour upon the body, no matter whether the relative velocity of the two is large or small when they are very far apart. (2.) Certain conditions of motion will be developed in the more or less continuous stream as soon as the body enters the cloud, and in the main will remain unchanged until it passes out, or at least as long as the separate parts of the cloud preserve the same constitution. If the matter is assumed to be dust-like in character, the

* Ueber den neuen Stern im Fuhrmann, Abhandlung der Berliner Akademie vom Jahre 1892. Translation in ASTRONOMY AND ASTRO-PHYSICS, Dec., 1892, and Jan., 1893.

† From Professor Seeliger's first paper.—*Tr.*

attraction of the body on the separate particles will be the principal agent. The separate particles will then describe hyperbolas whose asymptotes are all parallel and whose focus is in the body The particles will approach the body from the direction of the asymptotes Since a simple application of Kepler's laws will determine all the circumstances of motion in this case, it seems superfluous to consider them more closely here, only the following remarks may still be added. The hyperbolas described in the relative motion will have a considerable curvature close to the body if the initial velocity is appreciably smaller than the maximum velocity. Then after passing the body the particles will move along the hyperbolas, in directions which, under certain circumstances, may form acute angles with the original directions. The magnitude of these angles will depend upon the initial velocity, (with given maximum velocity): within certain imits, therefore, it might be assumed. But in the case of a continuous stream the circumstances would not assume this simple form, for the reason that particles describing congruent, but differently lying hyperbolas, would, on account of the prevailing symmetry, come in contact behind the body. If the observer should be situated not very far from the direction of the asymptotes,—and it is easy to see that very wide limits are here allowable,—these collisions would not be directly noticeable. If the medium has a fluid or gaseous structure, this point either does not come into consideration at all, or it takes an entirely different form. The circumstances would then in general resemble those of flying projectiles, which have become widely known by photographic researches, and particularly by the beautiful experiments of E. Mach.

The conditions last mentioned require, under some circumstances, a closer investigation. Strange to say, however, Herr Vogel has denied the correctness of the deductions first arrived at, although they are incontestable, and has arrived at the following conclusions, which are irreconcilable with the teachings of mechanics. He says (pp. 56 and 57):

"Under the supposition that the body moving through the cloud, and the cloud itself, have no unusually great velocities in comparison with the mean velocity of stars in space (for the main object of the hypothesis is to avoid the assumption of such velocities) and that the body entering the cloud is moving toward us, the particles of the cloud, shortly before the entrance of the body, would, by virtue of its attraction, move toward and past it. They would carry with them particles of its atmosphere,

and, by their passage close to its surface, acquire a more or less great velocity in a direction opposite to the Earth. The spectrum of the body, whose surface on entering the cloud is thus raised to incandescence, would probably be continuous and show absorption lines, and on it would be superposed the bright line spectrum of the detached particles of the body's atmosphere. The centers of these lines would at first be relatively displaced, the bright lines toward the red, since with Seeliger we have to assume that the particles of the cloud would, in general, move away from us. But when the body is once in the cloud, particles rush upon it from all sides, and those grazing its surface would acquire motions making all possible angles with the line of sight. The bright lines would consequently appear very greatly broadened, but a displacement of the centres of the bright and dark lines could no longer be supposed to exist, since the motions of body and cloud would not be appreciable in comparison with the enormous velocities which would be imparted to the separate parts of the cloud by the attraction of the body. At the exit of the body we should expect appearances similar to those at its entrance, but the bright lines would be displaced toward the blue."

All that Herr Vogel says here about the directions of motion really applies only to the accelerations. *The two conceptions acceleration and velocity are thus confounded*, and consequently the principal objection of Herr Vogel to my assumptions must be regarded as entirely removed.

As for the further remarks which relate to my hypothesis, and which Herr Vogel probably regards as of minor importance, I shall at present touch upon only one more point, reserving the rest for a future occasion. Herr Vogel says, "It should further not remain unmentioned that Seeliger has fixed his attention only on the relative motion which is indicated by the displacement of the bright as compared with the dark lines, and has overlooked the great velocity of over 400 miles per second with which the star whose spectrum contained absorption lines was actually moving through space. But this circumstance must not be left out of consideration in any hypothesis on the nature of Nova Aurigæ." After these words, everyone would expect that Herr Vogel, in the hypothesis of his own which is given further on, would take into account this point, which he evidently considers to be an important one. This is not done, however; indeed, no attempt is made to explain the great velocity, nor, I may be permitted to mention, can it be made successfully in the case he supposes. By establishing this relation of the two cases, I am freed from every necessity of going further into the subject.

With regard to the hypothesis advanced by Herr Vogel as an explanation of the new star, I shall content myself with quoting the paragraphs which contain its principal features, and adding to them a few brief comments. This will be sufficient, I think, to allow a judgment to be formed as to the validity or probability of these and similar views.

(1). "The oipnion that the Nova could be explained by the encounter of a heavenly body with several bodies, forced itself upon me immediately after the first observations, and I have since been more and more confirmed in this view by the further observations which have been made. It is true that the question, whether there was not too small a probability in such an encounter of heavenly bodies, at first gave rise to doubts, but they soon disappeared on considering that, according to the Kant-Laplace hypothesis, it is difficult to conceive of a large celestial body unaccompanied by attendants. It is really surprising that in all the hypotheses accounting for new stars, the assumption of these simple conditions should have been left out of consideration."

If it is thought desirable to bring in the hypothesis of Laplace, the facts which it was principally intended to explain must not be left unconsidered. The most important peculiarity of a planetary system which here comes in question consists in the grouping of masses around a certain middle plane, and a (say) spherical grouping of masses is not compatible with the conception of a planetary system which is familiar to us, and which, in default of other experience, must be regarded as the only one admissable. Assuming now that every thing else in Herr Vogel's hypothesis is correct, it is possible to satisfy the observed appearances only on the assumption that the star was many months in close proximity to the individual members of the system, particularly when it is remembered that the reappearance of the Nova is to be explained in the same manner. From this it follows that the star must have moved in a direction which was inclined at an extremely small angle to the mean plane of the system. It will hardly be admitted that such a condition has any special probability, and the doubts which may arise in this aspect of the question are by no means dispersed.

2. "If we assume that a body, having a mass of the same order as that of the Sun, should suddenly pass close to a system like our own, whose central star had lost its light by gradual cooling, then enormous disturbances would be produced, and the collision of individual members of the system and the consequent production of luminous outbursts, would be inevitable."

The possibility of such a collision can of course not be denied; but it should be observed that the orbits will be changed in consequence of the great relative initial velocity, which is to be assumed according to (3), although on a materially smaller scale. The assumption of a moderate number of large planets will even then not make it easy, least of all for these, to show that a collision is "inevitable." Further, according to the observed phenomena, the collisions must have occurred very soon after the appearance of the Nova and probably at exactly that time, and no more occurred afterward, at least not after the beginning of spectroscopic observations, for a considerable change in the spectrum was not observed during the first appearance. All this cannot be said to show that the views advanced are very probable, quite apart from the consideration that we are justified in requiring a less general statement of the circumstances, in order to allow discussion in one or another direction.

(3) "Let us now suppose that the body which gave the continuous spectrum with absorption bands in the composite spectrum of the Nova, and which, as is well known, was moving in space with a velocity of about 400 miles per second, came close to a system moving with no more than ordinary velocity, in a direction with regard to which no special assumptions need be made."

This sentence shows the correctness of the remark I have made above, that Herr Vogel makes no attempt whatever to refer the velocity of 400 miles per second to normal conditions. Such an attempt can in no way lead to this end on the ground of the remaining assumptions, without encountering contradiction of the observations or inadmissibly large masses; this follows immediately from the formulæ of my article.

(4). "By the passage close to one of the larger or several of the smaller bodies of the system, perhaps also by direct collision with smaller bodies, the star entering the system would suddenly be brought to a high state of incandescence. At the time of the spectroscopic observations the body was in a part of the supposed system which was somewhat thickly filled with little bodies, and these, by their close passage and by partial collisions, maintained the brilliant incandescence of the surface and atmosphere of the penetrating body, which the extension of the continuous spectrum into the violet shows that they must have possessed. In this way some of the bodies themselves acquired an excessively high temperature and a more or less high velocity, giving rise to the spectrum with bright lines, and thus producing the same effect as the particles of the cosmical cloud in Seeliger's

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hypothesis. There is however this important difference, that the motion of the little bodies was regulated by the central star; they moved in an actual stream against the penetrating body, and therefore could not have moved toward it from all directions."

Leaving out of consideration the last statements, which, as shown above, are founded on error, I must object to this as in my first paper, that without greater definiteness the simple passage of two dark bodies can be assumed as the cause of their luminous outburst, and continuous or discontinuous spectra can be assigned to them at pleasure. Nothing whatever is known about the effect of such influences, and they would in any case depend upon so many attendant circumstances that it seems to me rash to support a hypothesis on such a foundation. To assume small planetary bodies as the cause of such occurrences, in particular, seems to me entirely inadmissible. The further development of the hypothesis can be recognized as correct only under entirely definite assumptions. If the small bodies should not meet the large one in a swarm which could be regarded as nearly continuous, other phenomena would manifest themselves than Herr Vogel seems to think. The hyperbolas described by the small particles would have a great curvature in the close vicinity of the body, since the initial velocity is only a fraction of the maximum velocity. Continuing on these hyperbolas, the luminous particles would soon be moving very nearly in the direction of the second asymptote. If we assume, as a condition which is favorable to the hypothesis, that the Earth is very nearly in the original direction of the particles' motion, *i. e.*, in the asymptote of the parabola, the particles would very shortly after their closest approach to the body, be moving in a direction inclined at an angle 2 α to the line of sight, α being the angle between the major axis and asmyptote of the hyperbola. From the values assumed by Herr Vogel (initial velocity 400 miles, greatest velocity > 740 miles) it would follow that luminous particles must be in evidence spectroscopically, having a motion of at least 230 miles per second *toward* the observer. It will be seen therefore that in this case phenomena would be produced which are not confirmed by the observations, and hence a modification of the fundamental assumptions becomes necessary. Such a modification, as already mentioned, consists in attributing to the stream of small bodies the character of a continuous stream. *But that is the essential feature of my hypothesis*, and the assumption of Herr Vogel would differ from it only in the highly problematical

way in which, according to him, the small bodies are brought to a state of incandescence. That the particles were moving under the influence of the central mass before they came within the sphere of attraction of the penetrating star, does not at all change the nature of the occurrence, and introduces only unimportant modifications. It may be well to mention that in the further development of this conception, which is in itself a very simple one, the known theorems of Laplace relating to very large perturbations readily lead to a correct judgment of the circumstances. These theorems can be applied with considerable certainty because the relative velocities involved are very great, (at least 400 miles per second), and the errors of the approximation are thereby materially reduced

(5) "The atmospheres of the central body and larger bodies of the system would also be greatly heated by inevitable disturbances of the surface-levels and consequent eruptions, and if these causes were not sufficient to produce a higher temperature in the surface of the bodies than in their atmospheres, (which however we might expect in the case of a body heated by the fall of smaller bodies from without), they would nevertheless give rise to a spectrum in which bright lines would predominate. In this then we have a simple explanation of the intensity-maxima in the bright hydrogen lines, which indicate a small motion in space, and which at first had the greatest intensity."

The "disturbances of the surface-levels" of the central body are no doubt assumed to be caused by the entrance of the body from without. If that were not the case, still another hypothesis would be called for, for it cannot be doubted that in any case such eruptions can be produced only when the attractions are very great and act at extremely short distances But if the passage of the cosmical body close to the central mass plays so important a role, it fits in very badly with the probability of the entire occurrence; for we must then assume the accidental coincidence of two events; (1) the cosmical body must move very nearly in the plane of the planetary system, (2) it must also pass extremely close to the central body. Moreover, Herr Vogel explains the reappearance of the Nova by an encounter with a distant planet, which adds a new assumption of little probability to the previous one.

I shall refrain from going further into particulars. What I have said will probably suffice to show clearly that hypotheses like that of Herr Vogel are in no way capable of serving as the basis of more extended considerations.

ASTRO-PHYSICAL NOTES.

All articles and correspondence relating to spectroscopy and other subjects, properly included in ASTRO-PHYSICS, should be addressed to George E. Hale, Kenwood Observatory of the University of Chicago, Chicago, U. S. A. Authors of papers are requested to refer to last page for information in regard to illustrations, reprint copies, etc.

Professor Vogel on Nova Aurigæ.—In commenting on Professor Vogel's hypothesis last month, we referred to the translation of his memoir as completed in that number, but owing to the exigencies of printing, the latter part, containing Professor Vogel's own views, had to be omitted. It is given in the present number, together with an article by Professor Seeliger in which the correctness of his original hypothesis is upheld.

In *A. N. 3198*, Professor Vogel replies to the remarks of Herr Belopolsky quoted in our Astro-Physical Notes last month. The fine lines in the spectrum of Venus, and the identity of details in the accidentally triple spectrum of the Nova obtained with the Pulkowa refractor, are not regarded by Vogel as a convincing proof that the details are real. The Harvard College photographs are materially different from Belopolsky's. With regard to the occurrence of iron lines in the part of the spectrum covered by the Pulkowa photographs, Professor Vogel says that six or seven such lines are found in his own photographs and ten in those of Campbell. It is natural that the supposed gaseous envelope should be more distinct with the 15 inch than with the 30 inch refractor, as the circles of chromatic aberration are brighter with the former instrument, and therefore easier to see.

Professor Vogel takes the occasion to say that his views have not been changed by the criticisms of Professor Seeliger, but that he does not at present intend to pursue the subject farther.

The Sky from Pike's Peak.—In the October number of ASTRONOMY AND ASTRO-PHYSICS, Vol. XII, p. 750, Dr Barnard describes the wonderful blueness of the sky as seen by him from the summit of Little Ouray mountain, Colorado, on Aug. 16 last. He also speaks of the fact that his view from the summit of Pike's Peak on the preceding day was spoiled by a sudden snow-storm.

I ascended Pike's Peak ten days later than the date of Dr. Barnard's visit and remained there during the night of Aug 24 and until noon of the 25th. During the first half of the night the sky was obscured by fast-drifting clouds, but leaving my cold bed in the half-ruinous stone building formerly used as the Government Signal Station, and going out of doors about two o'clock in the morning, I was delighted to find the clouds all gone and the sky ablaze with stars down to the very rim of the horizon. To eyes accustomed to the dim and smoky horizons of the East it was positively startling to see stars of the third and fourth magnitude shining with apparently undiminished lustre close to the edge of the celestial canopy. The number of visible stars seemed to have been multiplied indefinitly. I longed for a telescope. The most powerful instrument I had was an opera glass, and its revelations were superb.

After sunrise the sky was equally clear, only a layer of haze being visible far below upon the plains, and I noticed, at once, the absence of any perceptible atmospheric glare around the Sun. Upon covering the disc of the Sun with a bit of black glass I was able to follow the pure dark blue of the sky right up to the Sun's

edge. As far as I could determine there was absolutely no haze in the air above the peak. This experiment with the glass was made when the Sun was near the meridian, but earlier in the day, and when the Sun had risen but a few degrees above the horizon the absence of atmospheric glare was equally noticeable to the unaided eye.

I was the more interested by these observations because of the then recent visit of Professor Hale who had found the atmosphere there unsuitable for his attempt to photograph the corona without an eclipse. I am convinced that if Professor Hale's visit had been made near the end of August he would have found the air far better suited to his experiment than it was at the time when that experiment was tried.

GARRETT P. SERVISS.

The Spectroscopic Investigations of Mr. George Higgs.—The publication of a new photographic map of the normal solar spectrum, extending from λ 8345 in the infra-red to the extreme limit of the ultra violet, is an event calling for more than passing comment. For such a map, to prove itself truly worthy of publication, must rival in excellence the autographic records of the solar spectrum already in the hands of spectroscopists. In these days of refinement in instruments and methods it is a matter of no small difficulty to enter a field already occupied by leaders of research, and produce results of such evident merit as to leave little or no room for criticism. But this is what Mr. George Higgs, of Liverpool has done. His map of the solar spectrum, in its sharpness and clearness of definition, its greater extent, and the perfection of its photographic reproduction, offers at least some points of superiority over even the universally accepted standard of excellence which we owe to Rowland.

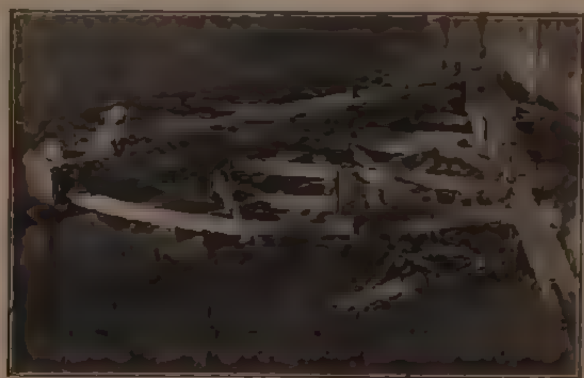
It therefore goes without saying that during a recent stay of a few days in Liverpool, we were quite ready to make use of the opportunity thus afforded us of visiting Mr. Higgs in his laboratory. In the present note it is not our intention to describe all that Mr. Higgs' kindness enabled us to see, but merely to point out one or two features of the work.

The Rowland concave grating employed is four inches in diameter, and has a radius of curvature of 10 feet, 2 inches. The scale of Higgs' original negatives is thus about half that of Rowland's. The form of mounting differs materially from that usually adopted. The grating and photographic plate are fixed at the opposite extremities of a bar forming the diameter of a circle 10 feet 2 inches in diameter. The slit is placed at the extremity of a movable radius, and can thus be set at any point on the circumference. In practice, its range is limited to the orders of spectra under investigation. The circular table which carries the grating, plate and slit is built of strips of varnished pine, and is thoroughly braced. As it stands on the second floor of a building facing on a busy street, precautions had to be taken to obviate the constant vibration. When the instrument is in use it rests upon three legs. Of these two terminate in rollers, the spindles of which are carried on packing of cork and india-rubber. The third rests on a spring-board. In this way little difficulty is experienced from vibration.

The accompanying cut, which has been made from a small photograph taken by Mr. Higgs, will serve to give an idea of the arrangement of the instrument. It will be seen that the grating-box is connected with the slit and camera by long wooden boxes, which effectually exclude the light, and render darkening of the room unnecessary. A heliostat, placed on a shelf outside the window, reflects sunlight upon a 2-inch quartz lens of about 3 feet focus, which forms an image of the Sun on the slit. When photographing the extreme ultra-violet a silvered

quartz screen is used to cut out the less refrangible rays. The maximum transparency of the screen is for light of λ 3200.

Mr. Higgs has acquired great skill in the use of dyes for sensitizing photographic plates for the lower region of the spectrum, and a special preparation of alizarin blue, described in a recent volume of the *Royal Society Proceedings*, enables him to secure excellent negatives as far down as λ 8340. As a long strip of spectrum is photographed on a single plate, different exposures are of course required for different regions. A screen, pivoted at one side of the camera, and moved by clock-work about a vertical axis during the exposure, makes it possible to produce negatives of equal density throughout the spectrum.



MR. GEORGE HIGGS' CONCAVE GRATING SPECTROSCOPE.

But the most striking difference between the methods of Mr. Higgs and those of other investigators is in regard to the adjustment of the apparatus. Ordinarily, a concave grating is adjusted once for all, and small changes are made from time to time as occasion seems to require. It is well known that one of the most rigorous requirements in the use of the instrument is the exact parallelism of the slit and the lines of the grating. It is usual, however, to regard this adjustment, when once made, as permanent, or so nearly so as to require infrequent attention. Mr. Higgs does not rely on any possible permanence, but completely readjusts the apparatus for every new exposure. A long lever, controlled by cords within easy reach of the observer at the eye-piece, gives the means of rotating the slit while observing the spectrum. The maximum sharpness of the lines can thus be obtained in a moment. The grating can also be rotated about a vertical axis through a small angle, and the remaining adjustments effected by the observer without leaving his seat. We have little doubt that Mr. Higgs' marked success is due to the care taken with the adjustments of the instrument, which, it should be added, was made wholly (with the single exception of the grating) by himself.

We are indebted to Mr. Higgs for a portfolio filled with a great variety of spectrum photographs. The solar spectrum is shown in various regions, in all of which the sharpness of the lines is remarkable. The series includes a large number of photographs made with low Sun, and the telluric spectrum has never been better portrayed. The A and B groups are particularly striking. Other photographs of the spectrum of opposite limbs of the Sun show the displacement of the lines due to the solar rotation. In many of the spectra the H and K lines

contain in the center of the broad absorption bands distinct double reversals. These are due to the solar faculæ. The astigmatism of the concave grating would cause the double reversal in any faculæ which happened to fall upon the slit to extend across the entire spectrum. We have seen similar double reversals in Professor Rowland's original negatives.

Mr. Higgs' photographic map of the normal solar spectrum is published in three sizes: the first in 44 parts and enlarged 4 times, the second in 45 parts, enlargement twice the original size, and the third in 15 parts, original size. The prices are £8 8s, £3 3s and £1 1s respectively. Mr. Higgs' address is 467 West Derby Road, Liverpool.

G. E. H.

The Change of Sensitiveness in Dry Plates.—In the November number of ASTRONOMY AND ASTRO-PHYSICS an interesting fact is stated by Dr. Max Wolf regarding the change in sensitiveness of dry plates when used for stellar photography. Dr. Max Wolf found that Lumière plates increased three-fold in sensitiveness after keeping for five months. The same thing has been stated by others regarding change in sensitiveness of dry plates when determined with the Warnerke Sensitometer.

During the past ten years I have made hundreds of experiments on the sensitiveness of plates, for day-light, candle light, and through red glass, but I have never noticed any change in sensitiveness when used for a landscape negative. Various brands of plates have been kept for a number of years without my suspecting any change in the sensitiveness when used in daylight. There is, however, a peculiar effect produced in the sensitiveness of any plate when used in feeble light, if it receives a preliminary or supplementary exposure.

In a paper read before the A. A. A. S., in 1892, and also in a paper presented before the Photographic Congress, in Chicago, last August, I showed that a collodion or emulsion plate may become from five to eight times more sensitive when used in candle light or feeble day-light, provided it had received a proper amount of preliminary exposure. A plate, however, subjected to such preliminary exposure, does not show increased sensitiveness when used in strong day-light, or for a landscape negative.

My explanation of this phenomenon is as follows: A photographic plate may receive a certaina mount of light, and when placed in the developer, show no visible blackening of the film. A plate, therefore, which has had a certain amount of preliminary exposure may be under such strain that a very small amount of additional exposure will enable the developer to reduce the bromide of silver. Under this conception the actinic effect should be measured by the intensity of the light multiplied by the time, plus a constant quantity; the constant being a function of the original sensitiveness.

It would appear from the observations of Dr. Max Wolf, that a chemical effect similar to preliminary exposure may take place in the plate when kept in the dark a certain length of time. The subject is one of great interest, and should be farther investigated.

G. W. HOUGH.

Northwestern University.

The Radiation of Heated Gases.*—The question as to the nature of the radiation given out by heated gases is clearly one of fundamental importance, bearing

* F. Paschen: *Ueber die Emission erhitzter Gase*, *Wied. Ann.*, Bd. 50; pp. 409-443.

as it does directly upon the spectroscopic study of comets and nebulæ. The subject has claimed the attention of numerous experimenters, who have endeavored to determine whether a gas or vapor can give a "characteristic," discontinuous spectrum in virtue of its temperature alone, or whether a chemical process is necessary to excite the characteristic molecular vibrations. Of these two views, the latter appears to have been the more favorably received. Until recently it appears that in all experiments in which a discontinuous spectrum has been obtained from a gas, some chemical action was at the time taking place at the luminous source. The question has been recently put to the test by Pringsheim, (see *Wied. Ann.*, Bd. 45, p. 428; Bd. 49, p. 347) who heated sodium and sodium salts in a porcelain tube, and examined the resulting emission spectroscopically. In no case was he able to render the gases luminous, except upon introduction into the tube of some reducing agent, and hence concludes that there is no ground for the assumption that gases can become luminous by a mere increase of temperature.

The recently published work of Paschen seems however to furnish abundant evidence in favor of the opposite view. The problem which he assigned himself was in the first place to devise a means of heating the gas under examination to a much higher temperature than had been previously attained, at the same time reducing to a minimum the mass of hot solid matter in the neighborhood of the slit of the spectroscope, and to examine the radiation by a more delicate method than has been hitherto employed. In both of these the writer was highly successful. As a means of heating the gas a narrow strip of platinum foil was wrapped around a glass rod in such a way as to form a cylindrical tube 4 cm. long and 3mm. in diameter, the edges being brought as close together as possible without contact. The glass rod was removed, and the cylinder placed vertically before and below the slit of the spectroscope and maintained at any desired temperature below the melting point of platinum by means of a constant electric current. The gas was conveyed by a glass tube into the lower end of the cylinder, and after becoming heated rose in a stream before the slit. Radiation from the platinum cylinder was carefully kept out by a metallic screen. The dispersing piece was a 60° prism of fluor spar. This was mounted upon the table of a spectrometer reading to 5″ and was kept in the position of minimum deviation by an automatic device. The customary lenses were replaced by concave silver-on-glass mirrors. The bolometer employed to investigate the spectrum was of extremely delicate construction. The sensitive resistance required less than 2 seconds to attain a constant temperature, since within this length of time after the beginning of a throw, the galvanometer needle was again completely at rest. The galvanometer itself was probably the most sensitive ever constructed. It is described by the writer in a previous article. (See *Wied. Ann.*, Bd. 48, p. 272).

The high sensibility was attained principally by the use of a number of extremely short magnets, the magnetic moment of the system being thereby reduced in a much smaller ratio than the moment of inertia. The needle gave a throw of 1 mm. for 3.3×10^{-11} Amp., the extreme theoretical sensibility of the bolometer being 5 millionths of a degree centigrade.

The temperature of the current of gas was measured by means of a Platinum Platinum Rhodium couple. The superior limit to the temperature employed was about 1250° C.

The gases examined were Air, Oxygen, Carbon dioxide and Water vapor. With air and oxygen, no radiation was detectedexcept that due to impurities consisting of water or carbon dioxide. The spectrum of carbon dioxide showed a

sharp maximum of radiation at about wave-length 4.75 μ, corresponding to a deviation (minimum) of 29° 21′. Near this point the galvanometer deflection rose rapidly from near 0 to 330 mm.

In the case of water vapor, a maximum of considerable intensity (180 mm.) appeared at wave-length 2.75μ, and several smaller maxima also occur in such a way as to suggest a band-spectrum. The spectrum of the gases from a Bunsen-burner, taken at a point 4 cm. above the last discernible trace of flame, exhibited all the principal maxima of CO_2 and water vapor as did also, with increased intensity, the spectrum of the flame itself. The intensity of these maxima was found to decrease rapidly with the temperature of the gas. Nevertheless, at 284° C. the principal maximum in the water-spectrum was still present, and at 73° C. the CO_2 maximum was clearly distinguishable. In the latter case, the highest temperature of the platinum cylinder was 120°. At this temperature Paschen considers it out of the question that any trace of dissociation can have taken place in CO_2. The same continuous spectrum is, however, present as in the Bunsen flame at a temperature of 1460° C. This fact indicates that a chemical process is not a necessary condition for the radiation from the Bunsen burner flame, or from the gases which arise from it. Moreover, at the highest possible temperature of the platinum cylinder, the CO_2 maximum was about ¾ as intense as in the Bunsen flame, and the simplest conclusion is that they are due to the same cause, namely, a simple heating.

From these observations, Paschen concludes that gases can emit discontinuous spectra in virtue of their temperature alone, thus contradicting the deductions of Pringsheim. He does not hesitate to admit, however, that chemical action may be the means of intensifying the emitted radiation.

Upon reading Paschen's paper, one cannot fail to be impressed with the great delicacy of his apparatus, and the accuracy of his results. The conclusions which he draws from them, will be interesting in the light of future work which is bound to be done on this subject. They appear, to be fair interpretations of the observed facts, and to be free from objectionable hypotheses. As to whether Water vapor and Carbon dioxide satisfy Kirchhoff's Law, no conclusion can be drawn, as their absorption spectra have not been studied. It is to be hoped that Mr. Paschen will push his investigations in this direction, so as to throw, if possible, more light upon this interesting question.

ROBERT R. TATNALL.

The Object-Glass Grating.—We have received the following letter from Mr. L. E. Jewell:

Since writing the paper on the "Object Glass Grating" in ASTRONOMY AND ASTRO-PHYSICS for January, a further study of the subject has shown the desirability of some changes in the methods proposed for constructing such a grating.

When a grating for use has been constructed upon a plate of plane glass, with parallel surfaces, then instead of the method suggested it would be much better to take another plate of plane parallel glass, and cement it to the film side of the grating. This process offers several advantages, the chief being that the photographic film is protected from injury in use, and if there is any irregularity in either film or plates it can be corrected by an optician after the grating has been made.

Another change thought desirable, would do away with some of the greatest photographic difficulties, *i. e.*, the construction of a photographic slit perfectly straight and symmetrical. The difficulty may be avoided by using as a slit the space between two wires stretched vertically by weights and rendered parallel by

fitting into adjacent grooves of similar screws which are pressed against the wires at the top and bottom. Screens with some soft, sticky, opaque material at their edges can be pressed against the outer edges of the wires, without affecting their parallelism or distance apart, so as to exclude all light except that which comes through the aperture between the wires. Furthermore the wires may be smoked, thus coating them with a thin black deposit of carbon which will not affect the slit unfavorably, but instead will prevent the reflection of light at the edges of the wires.

Greater usefulness may be given to an object-glass grating constructed in the manner suggested, if at the time it is copied from the original a circular disc be interposed so as to leave vacant a circular space (of a size to be determined by the diameter of the object-glass and other considerations) in the centre of the grating. In this way a fairly good image of the star may be obtained, so that in photographing stellar spectra requiring a long exposure, we may scratch a straight line across the middle of the photographic plate deep enough to cut through the film, adjust the plate so that the scratch will be parallel to the equator, and then having an aperture in the back of the camera in which an eye-piece may be mounted, we can so guide the telescope with the eye as to keep the star's image on the scratch in the film of the plate, while by slightly altering the clock rate we can lengthen the image in right ascension so as to produce a spectrum of the desired width. Likewise the scratch may be adjusted parallel to a right ascension circle and the star moved along the scratch by gradually altering the declination of the telescope.

The many photographs of the heavens that have been made have shown the necessity of controlling the motion of the telescope by hand so as to keep it constantly pointed in the direction desired, imperfections of clock-work and the adjustments of an equatorial necessitating some such control. This would seem to be nearly as desirable in photographing stellar spectra as in photographing the stars themselves.

Where the chief use of a telescope is the photographing of stellar spectra, the telescope tube should not be of circular cross-section, but of such shape as to permit the use of as long plates as the spectra on each side of the normal will cover with the dispersion it is desired to use. LEWIS E. JEWELL.

The Great Cluster in Hercules.—Professor Scheiner contributes an interesting popular article on this cluster in the December number of *Himmel und Erde*, in which the drawings of Herschel, Secchi, Lord Rosse, Trouvelot, and Harrington are compared with the results of photographic researches at Potsdam. The three radiating "canals" of some of these observers are shown to have no real existence. Narrower canals and outlying rows of stars, or "arms," can be traced to some extent on the photographs, but they are not more striking than similar fortuitous arrangements obtained by spatter-work. Counts of stars in circular areas of suitable radius justify the supposition that the general form of the cluster is globular. Professor Scheiner deprecates the writing of popular articles containing speculations which run ahead of scientific demonstration.

Change in the Brightness of Nova Aurigæ.—According to M. Bigourdan, of the Paris Observatory, a change of about one magnitude in the brightness of this unique variable took place in October and November of the past year. The light fell off notably between the middle of October and the 8th of November, then increased for a few days, but the brightness of Oct. 10 was not regained.

Chromatic Aberration of Telescopes.—An interesting discussion of various matters pertaining to telescopes, at the November meeting of the Royal Astronomical Society, is reported in the December number of the *Observatory*. Mr. H. Dennis Taylor read a paper on the amount of light lost for purposes of definition by the effect of chromatic aberration, and arrived at the conclusion that the loss is very considerable, varying from 9 per cent. in the case of a 6-inch refractor to 50 per cent. in that of the new 28-inch Greenwich equatorial. Owing to the great focal length of the Lick telescope as compared with its aperture, the loss in that case is small,—only 27 per cent.

As the total amount of light in the image is independent of the aberration, it is evident that light is not lost in the sense that it would be if absorbed by the object-glass. The effect of the loss of light referred to in a technical way by Mr. Taylor is equivalent to bad definition in the ordinary way of regarding the subject. Mr. Taylor's method of procedure seems to have been the following: starting with the color curve of an objective, obtained either by observation, or by computation from the known constants of the glass, the diameters of the circles of chromatic aberration at any given plane (assumed to pass through the focus) can be found, and hence what rays fall within and what without the limits of the theoretical star-disc. Evaluating the physiological effect of the rays so divided, by means of Abney's curve of luminous intensity in the normal solar spectrum, the percentage of light thrown outside the star-disc is obtained, and this light is regarded as lost.

While it is hardly fair to judge Mr. Taylor's paper by the brief abstract given in the report, the results already mentioned, taken in connection with ordinary experience, lead to the belief that he must have set too high a value on the scattered rays. Dawes' rule gave for the distance between double stars just admitting resolution by a telescope

$$\frac{4''.56}{a},$$

(a being the aperture), while theory gives

$$\frac{5''.52}{a},$$

the actual telescope thus exceeding its theoretical requirements. The difference is no doubt due to the fact that the illumination at the edges of the star-disc is very feeble, so that its full size is not seen except in the case of very bright stars. Dawes' formula was deduced from observations made with small telescopes, but the great telescopes of modern times have shown themselves capable of working up to nearly the theoretical limit. Hence it would seem that practically chromatic aberration is less injurious to definition than Mr. Taylor finds it to be.

Assuming that Mr. Taylor's theory is correctly outlined above, the explanation of its apparent variance with the results of experience may be as follows: Consideration of the color curve of a well-corrected objective shows that the greater part of the "lost" light falls very close to the circumference of the theoretical star-disc, which is after all a somewhat arbitrary limit; hence the effect of this light, which numerically would rank with widely scattered light of the same intensity, is merely to slightly enlarge the image. It is further to be observed that the distribution of light in the superposed circles of chromatic aberration is not uniform.

The Astronomer Royal stated that the color curve of the new 28-inch objective, obtained by observations with a McClean star spectroscope, seemed to differ from the color curves of other large instruments. The minimum focus (at a point

halfway between D and E) seemed to be higher than usual, although the outstanding color in ordinary observation was entirely satisfactory to the eye.

Attention may here be called to the fact that the McClean spectroscope is not suitable for such determinations, as the considerable chromatic errors of the apparatus and of the eye vitiate the results. A slit spectroscope should be used, and the adjustments of the instrument for each part of the spectrum determined beforehand by means of solar lines. In general the minimum focus of a large telescope lies somewhat higher than the place mentioned by the Astronomer Royal (D). In the Lick telescope it is at about λ 5650. J. E. K.

Notes on Stellar Spectra.—In *Astronomische Nachrichten*, No 2068, Espin called attention to the fact that the Hβ hydrogen line is very bright in ϕ Persei. Shortly afterward I examined the spectrum of that star and found Hα to be very much brighter than Hβ, as it is at present. I supposed the bright Hα line was well known, but am unable to find any statement that it has been observed by others.

Professor Pickering and Mrs. M. Fleming have announced in various places that the Draper Catalogue photographs show the Hβ line to be bright in the stars ψ Persei, π Aquarii, κ Draconis, υ Cygni, ν Sagittarii, χ Ophiuchi and Cord. Gen. Catal., 7191. I have examined the spectra of all these stars, and in every case the Hα hydrogen line is bright and very much easier to observe than the Hβ line. The hydrogen lines in the violet of these stars are probably dark for the most part; but photographs of the regions Hβ to Hδ of some of these stars show peculiarities of considerable interest, and make a detailed investigation desirable. Thus, in ϕ Persei the Hγ and Hδ lines consist each of two narrow bright lines about four tenth-metres apart upon a background but slightly darker than the ordinary continuous spectrum. In υ Cygni Hβ, Hγ and Hδ consist of narrow bright lines upon broad and nearly dark backgrounds.

Miss A. C. Maury found from the Draper Catalogue plates that Hβ and possibly Hγ in the spectrum of Pleione consist each of a narrow bright line on a dark background. The Hα line is bright in this star. Other observers have possibly called attention to that fact, but I am unable to find any such reference. Qualitative investigations of these spectra cannot be made advantageously by photography with the 36-inch telescope, owing to the large chromatic aberration of the great lenses in the blue and violet.

The spectrum of *Alcyone* is always classed as Secchi's type I, or A, as in the Draper Catalogue; that is, a spectrum containing dark (and usually rather broad) hydrogen lines. I have observed this star visually and found the Hα hydrogen line to be bright. It is not very intense, but in good seeing is easily visible with the 36-inch telescope. There is a narrow dark line in contact with it on the side of smaller wave-length, and possibly a still finer one on the side of greater wave-length. I do not think this line can be seen with a 12 inch telescope.

A photograph of the portion of the spectrum between Hβ and K shows Hβ, Hγ, Hδ and H to be dark, as was expected, and a few additional fine dark lines. —W. W. C. in *Publications of the Astronomical Society of the Pacific*, No. 32.

The existence of both bright and dark hydrogen lines in the same spectrum seems to be the most remarkable and mysterious spectroscopic anomaly yet discovered. So far it has been observed by Professor Campbell only, but few other observers have the facilities which are probably required for the purpose. An explanation of the phenomenon in question was suggested by Professor Frost at the Astro-Physical Congress in Chicago, but it seemed inadequate to account for more than a very slight contrast in the intensity of the lines.

The bright $H\alpha$ line in the spectrum of *Pleione* was noted by Keeler (ASTRONOMY AND ASTRO-PHYSICS, No 114, p. 352).

The Planetary Nebula SD. — 12°, 1172.—In No 32 of the *Publications of the Astronomical Society of the Pacific*, Professor Campbell gives some details regarding the spectrum of this nebula, which was discovered by Mrs. Fleming on the Harvard plates. The nebula is about 15″ in diameter and it has a nucleus or central star of the 9th magnitude. The three monochromatic images corresponding to the three strongest nebular lines at λ 5007, λ 4959 and λ 4862, were measured with a micrometer, the slit being open, and their diameters were found to be 11″, 9″ and 14″ respectively This would show that the hydrogen envelope is more extensive than the others but it is not stated whether allowance was made for the effect of chromatic aberration on the diameter of the image at the slit plate The differences are perhaps too great to be accounted for in this way.

The nebula resembles the great nebula of Orion in the relative strength of the hydrogen lines, and as it occurs in the same nebulous region, a possible common origin of the two objects is suggested.

New Variable Star. Walsingham Observatory Circular No. 38 announces that photographs taken with the Compton telescope show that the star Es.-Birm 57, R A., 11^h 39^m 58^s, Dec + 56° 23′ (1855), Mag 9.5, is variable. The star is now of the 8.5 magnitude, type III.

Stars with Remarkable Spectra.—Mr Espin's latest list (in *A. N. 3200*) contains over one hundred stars, most of which are of type III. The spectrum of R Coronæ contains bright hydrogen lines, a yellow line (D_3?) and numerous other bright lines and bands. Some changes were observed, indicating perhaps that the spectrum is a double one. The spectrum of T Coronæ is now certainly not nebular, as Lockyer stated it to be in 1889, and probably it is of type III. The $H\gamma$ line may be bright.

Observations of a Solar Prominence, and Reversal of the D_3 Line.—Mr W. B. Woods, of Washington, D. C , sends us sketches of a prominence observed by him on the north-east limb of the Sun, on Dec. 18, 1893 The prominence was not specially active, as indicated by the appearance of the hydrogen lines. Slight differences were suspected in the form of the prominence as observed in different lines of the spectrum. These differences are indicated in the sketches The latter part of the letter, relating to a supposed double reversal of the D_3 line, is given below.

"A feature of the observation was the apparent reversal of the D_3 line. This is imperfectly indicated in the sketch I had grave doubts as to the fact, at first, and returned to the D_3 line again and again, after observations of the other lines, and finally decided from four separate observations that the reversal occurred only at or very near the base of the prominence, of this fact, I am positive. I am not aware, either by experience of the past two years with the spectroscope, or the work of others, of the reversal of D_3, but it was so decided in this morning's observation that I thought it well to note it. A feature of this reversal was the unsymmetrical situation of the dark line with respect to the edges of the bright line. Its situation was toward the more refrangible side of the line, dividing the width (not height) of the bright line into four parts, it would occupy

the third fourth This dark line was unusually sharp, black and fine. The day was remarkable for the purity and quiet of the atmosphere and the definition the best in two years' work The grating has 24 000 lines to the inch."

Architect's Office, U. S. Capitol, W. B. WOODS
Washington, D. C.

Unit of Velocity in the Line of Sight.—It seems very unfortunate that there should be such a lack of uniformity among spectroscopists as to the unit employed in expressing velocities in the line of sight. Within the past year results expressed in four different units have been published in scientific journals These units are the German geographical mile, the *lieue geographique*, the English mile and the kilometer The last named is the only one which can lay claim to international acceptance, and the object of this note is to urge upon spectroscopists the uniform adoption of the kilometer in expressing velocities in the line of sight.

It appears almost superfluous to add other reasons for this simple extension of the use of the metric system in the interest of uniformity. We mention but one or two of the other points in favor of the kilometer.

The displacements of the spectral lines, from which the velocities in the sight line are deduced, are now universally reckoned in the metric system, either the ten-millionth or the millionth of a millimeter being employed. Symmetry demands the use of the same system in the other member of the proportion, and it may be recalled incidentally that Newcomb's official statement of the velocity of light is in kilometers per second.

The magnitude of the kilometer is also very convenient with the present accuracy of the determinations of stellar velocities in the sight line, and this convenience will continue as our accuracy increases. Decimals of a kilometer do not ordinarily need to be used in a single determination and the tenths of a kilometer are at present all that is required for the mean value of a series of determinations. It is difficult to see what actual advantage is gained by the employment of a larger unit even in such cases as that of Nova Aurigæ, where the uncertainty of a determination must for ordinary instruments amount to several kilometers, inasmuch as an idea of the measure of accuracy may be gained from the probable error or from the deviations of the separate determinations from the mean.

It is certain that neither the German geographical mile nor the *lieue geographique* will be adopted in English speaking countries, and equally certain that the English mile will not find acceptance on the Continent. The kilometer, however, ought to be alike acceptable to all parties.

In this connection may we allude to the fact that the metric units are real English words, and that the leading dictionaries prefer the spelling *meter* to *metre* and similarly for the other units with like endings.

While mentioning this the need of a proper term and abbreviation for the ten-millionth of the millimeter recurs to mind. The word "tenth-meter" is objectionable because it does not adapt itself to other languages. "X meter" is very clumsy and "Ångström unit" no less so Suggestions for a suitable international name and abbreviation or symbol for this unit have been in order for a long time. EDWIN B. FROST.

[It is to be hoped that Professor Frost's suggestion will meet with general approval. In this connection it seems proper to add that in the translation of Scheiner's *Spectralanalyse der Gestirne* on which he is engaged, Professor Frost has adopted the kilometer as the unit of velocity in the line of sight.]

CURRENT CELESTIAL PHENOMENA.

PLANET NOTES FOR MARCH.

H. C. WILSON.

Mercury during March will be passing between the earth and the Sun, as may be seen from the diagram in our last number, page 71. For the first two or three days the planet will be visible in the evening west after sunset. In order to see it one must look toward the west, just a little above the horizon. On March 14, 2h 18m A. M., Mercury will be in conjunction with the Sun, and after that time it will be morning planet.

Venus will be morning star and rapidly come out from the rays of the Sun. She will increase rapidly in brilliancy so that none can mistake her, greatest brilliancy being attained on the 22nd of March. Venus will be in conjunction with the waning moon, 12° 28′ north, March 4 at 9h 39m P. M. Central time.

Mars rises about 4 o'clock in the morning and is at such a southern declination that there will be little opportunity for observation of this planet in northern latitudes during March. It is in the constellation Sagittarius and moving eastward. Mars will be in conjunction with the Moon 4° 44′ north, March 1 at 11h 29m P. M. and again March 30 at 11h 38m P. M.

Jupiter will be in good position for observation in the early evening. His position southwest of the Pleiades is so well known by this time that it needs no mention. His motions during March will be eastward. Jupiter will be in conjunction with the Moon, 4° 40′ south, March 11 at 2h 40m P. M.

Saturn rises in the evening and will be in good position for observation after midnight. For the position of this planet in the constellation Virgo see the chart in our last number. Saturn will be in conjunction with the Moon, 6° 24′ north, March 23 at 3h 01m A. M.

Uranus is in the constellation Libra, southeast from Saturn (see chart page 73), and may be observed after midnight. Uranus will be in conjunction with the Moon, 3° 39′ north, at 6h 12m P. M., March 24.

Neptune will be in good position for observation during the early evening in March. The position of this planet in Taurus is unchanged from last month.

The asteroid *Juno* is in the constellation Libra about 5° northeast of the star β. It is making the turn of the loop in its apparent path and after the middle of the month will move westward.

PLANET TABLES FOR MARCH.

[The times given are local time for Northfield. To obtain Standard Times for Places in approximately the same latitude, add the difference between Standard and Local Time if west of the Standard Meridian or subtract if east].

MERCURY

Date 1894	R. A. h m	Decl. ° ′	Rises h m	Transits. h m	Sets. h m
Mar 5	23 31.8	+ 2 33	6 44 A. M.	12 37.6 P. M.	7 11 P. M.
15	23 27.0	+ 0 02	5 50 "	11 53.7 A. M.	6 57 "
25	23 06.7	− 4 33	5 09 "	10 54.1 "	4 40 "

Date. 1904.	R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m
			VENUS.		
Mar. 5	21 20.5	− 7 36	4 54 A. M.	10 27.0 A. M.	4 00 P. M.
15	21 25.0	− 8 59	4 24 "	9 52.0 "	3 20 "
25	21 42.5	− 9 23	4 04 "	9 30.3 "	2 56 "
			MARS.		
Mar. 5	19 00.6	− 23 16	3 42 A. M.	8 07.5 A. M.	12 33 P. M.
15	19 31.0	− 22 32	5 31 "	7 58.5 "	12 26 "
25	20 01.0	− 21 27	3 16 "	7 49.1 "	12 22 "
			JUPITER.		
Mar. 5	3 32.4	+ 18 25	9 18 A. M.	4 37.8 P. M.	11 58 P. M.
15	3 38.7	+ 18 49	8 43 "	4 04.8 "	11 27 "
25	3 45.8	+ 19 15	8 09 "	[illegible] 32.6 "	10 57 "
			SATURN.		
Mar. 5	13 34.1	− 6 54	9 02 P. M.	2 37.8 A. M.	8 14 A. M.
15	13 32.0	− 6 40	8 19 "	1 56.3 "	7 33 "
25	13 29.5	− 6 24	7 36 "	1 14.6 "	6 53 "
			URANUS.		
Mar. 5	14 51.5	− 16 01	10 57 P. M.	3 55.0 A. M.	8 58 A. M.
15	14 50.8	− 15 58	10 16 "	3 14.9 "	8 13 "
25	14 49.8	− 15 53	9 36 "	[illegible] 34.6 "	7 33 "
			NEPTUNE.		
Mar. 5	4 37.8	+ 20 35	10 13 A. M.	[illegible] 43.0 P. M.	1 13 A. M.
15	4 38.2	+ 20 37	9 34 "	[illegible] 04.1 "	12 35 "
25	4 38.9	+ 20 39	8 55 "	4 25.5 "	11 56 P. M.
			THE SUN.		
Mar. 5	23 05.0	− 5 51	6 31 A. M.	12 11.5 P. M.	5 52 P. M.
15	23 42.2	− 1 55	6 13 "	12 08.9 "	[illegible] 05 "
25	0 18.7	+ 2 01	5 55 "	[illegible] 05.9 "	6 17 "

Phases and Aspects of the Moon.

	Central Time. d h m
Apogee	Mar. 1 10 06 A. M.
New Moon	" 7 8 18 A. M.

[redacted]

Jupiter's Satellites for March.

Phases of the Eclipses of the Satellites for an Inverting Telescope.

I.

III.

II.

IV.

No Eclipse.

Configuration at 7h for an Inverting Telescope.

Day.	West		East.
1	·4 3·	○	·2 ·1●
2	·43· 1·	○	2·
[illegible]	·3 ·2 ·4	○	·1
4	1· ·3	○	·4 ·2●
5		○·	1· 2· ·3 ·4
6	2· ·1	○	·3 ·4
7	·2	○	1· 3· ·4
8	3· ·1	○·	·2 4·
9	○ 1· 3·	○	2· 4·
10	·3 2·	○	·1 4·
11	·31·	○	4· ·2●
12	4·	○·	·1 ·3 2·
13	4· ·12·	○	·3
14	4· ·2	○	1· 3·
15	4· ·1	○	3· ·2
16	4· 3·	○	1· 2·
17	·4 ·3 2·	○	·1●
18	.4 ·3 1· ·2	○	
19	·4	○	·3·1 2·
20	1· ·4 2·	○	·3
21	·2	○	1· ·4 3·
22	·1	○	3· ·2 4·
23	3·	○	1· 2· ·4
24	3· 2·	○	·4 ·1●
25	·3 ·21·	○	4·
26		○	·3 ·1 ·2 4·
27	○2· .1	○	·3 4·
28	·2	○	·1 4· 3·
29	·1 4·	○	·2 3·
30	4· 3·	○	1· 2·
31	4· 3· 2· ·1	○	

Phenomena of Jupiter's Satellites.

Central Time.

	h	m			
Mar. 1	12	13	P. M.	III	Tr. In.
	2	21	"	III	Tr. Eg.
	5	28	"	III	*Sh. In.
	6	18	"	I	*Oc. Dis.
	7	28	"	III	*Sh. Eg.
	9	45	"	I	*Ec. Re.
2	3	27	"	I	Tr. In.
	4	44	"	I	Sh. In.
	5	41	"	I	*Tr. Eg.
	6	57	"	I	*Sh. Eg.
	8	48	"	II	*Tr. In.
	11	13	"	II	Tr. Eg.
	11	21	"	II	Sh. In.
3	1	44	A. M.	II	Sh. Eg.
	12	47	"	I	Oc. Dis.
	4	14	P. M.	I	Ec. Re.
4	9	57	A. M.	I	Tr. In.
	11	12	"	I	Sh. In.
	12	10	P. M.	I	Tr. Eg.
	1	26	"	I	Sh. Eg.
	3	03	"	II	Oc. Dis.
	5	28	"	II	Oc. Re.
	5	37	"	II	*Ec. Dis.
	7	54	"	II	*Ec. Re.
5	2	12	A. M.	III	Oc. Dis.
	4	20	"	III	Oc. Re.
	7	17	"	I	Oc. Dis.
	7	29	"	III	Ec. Dis.
	9	17	"	III	Ec. Re.
	10	43	"	I	Ec. Re.
6	4	26	"	I	Tr. In.
	5	41	"	I	Sh. In.
	6	40	"	I	Tr. Eg.
	7	55	"	I	Sh. Eg.
	10	10	"	II	Tr. In.
	12	35	P. M.	II	Tr. Eg.
	12	40	"	II	Sh. In.
	3	03	"	II	Sh. Eg.
7	1	46	A. M.	I	Oc. Dis.
	5	12	"	I	Ec. Re.
Mar. 10	4	22	A. M.	II	Sh. Eg.
	2	46	"	I	Oc. Dis.
	6	10	"	I	*Ec. Re.
11	11	55	"	I	Tr. In.
	1	08	P. M.	I	Sh. In.
	2	08	"	I	Tr. Eg.
	3	21	"	I	Sh. Eg.
	5	45	"	II	*Oc. Dis.
	8	10	"	II	*Oc. Re.
	8	14	"	II	*Ec. Dis.
	10	31	"	II	Ec. Re.
12	6	25	A. M.	III	Oc. Dis.
	8	35	"	III	Oc. Re.
	9	15	"	I	Oc. Dis.
	11	29	"	III	Ec. Dis.
	12	39	P. M.	I	Ec. Re.
	1	18	"	III	Ec. Re.
13	6	25	A. M.	I	Tr. In.
	7	37	"	I	Sh. In.
	8	38	"	I	Tr. Eg
	9	50	"	I	Sh. Eg
	12	54	P. M.	II	Tr. In.
	3	18	"	II	Sh. In.
	3	19	"	II	Tr. Eg.
	5	41	"	II	*Sh. Eg.
14	3	45	A. M.	I	Oc. Dis.
	7	08	"	I	Ec. Re.
15	12	54	"	I	Tr. In.
	2	05	"	I	Sh. In.
	3	08	"	I	Tr. Eg.
	4	19	"	I	Sh. Eg.
	7	07	"	II	Oc. Dis.
	9	32	"	II	Oc. Re.
	9	32	"	II	Ec. Dis.
	11	49	"	II	Ec. Re.
	8	41	P. M.	III	*Tr. In.
	10	15	"	I	Oc. Dis.
	10	51	"	III	Tr. Eg.
	1	30	A. M.	III	Sh. In.
16	1	37	"	I	Ec. Re.

Mar. 19	3 30	"	III	Ec. Dis.
	5 20	"	III	Ec. Re.
20	8 24	A. M.	I	Tr. In.
	9 32	"	I	Sh. In.
	10 38	"	I	Tr. Eg.
	11 45	"	I	Sh. Eg.
	3 39	P. M.	II	Tr. In.
	5 55	"	II	*Sh. In.
	6 05	"	II	*Tr. Eg.
	8 19	"	II	*Sh. Eg.
21	5 45	A. M.	I	Oc. Dis.
	9 08	"	I	Ec. Re.
22	2 54	"	I	Tr. In.
	4 01	"	I	Sh. In.
	5 08	"	I	Tr. Eg.
	6 14	"	I	Sh. Eg.
	9 51	"	II	Oc. Dis.
	2 26	P. M.	II	Ec. Re.
23	12 15	A. M.	I	Oc. Dis.
	12 58	"	III	Tr. In.
	3 09	"	III	Tr. Eg.
	3 32	"	I	Ec. Re.
	5 30	"	III	Sh. In.
	7 33	"	III	Sh. Eg.
	9 24	P. M.	I	Tr. In.
	10 30	"	I	Sh. In.
	11 38	"	I	Tr. Eg.
24	12 43	A. M.	I	Sh. Eg.
	5 03	"	II	Tr. In.
	7 14	"	II	Sh. In.
	7 29	"	II	Tr. Eg.
	9 38	"	II	Sh. Eg.
	6 45	P. M.	I	*Oc. Dis.
	10 01	"	I	Ec. Re.
25	3 54	"	I	Tr. In.
	4 58	"	I	Sh. In.
	6 08	"	I	*Tr. Eg.
	7 12	"	I	*Sh. Eg.
	11 14	"	II	Oc. Dis.
Mar. 26	3 44	A. M.	II	Ec. Re.
	1 15	P. M.	I	Oc. Dis.
	3 02	"	III	Oc. Dis.
	4 30	"	I	Ec. Re.
	5 14	"	III	Oc. Re.
	7 31	"	III	*Ec. Dis.
	9 22	"	III	Ec. Re.
27	10 24	A. M.	I	Tr. In.
	11 27	"	I	Sh. In.
	12 38	P. M.	I	Tr. Eg.
	1 40	"	I	Sh. Eg.
	6 26	"	II	*Tr. In.
	8 33	"	II	*Sh. In.
	8 52	"	II	*Tr. Eg.
	10 57	"	II	Sh. Eg.
28	7 45	A. M.	I	Oc. Dis.
	10 59	"	I	Ec. Re.
29	4 54	"	I	Tr. In.
	5 56	"	I	Sh. In.
	7 08	"	I	Tr. Eg.
	8 09	"	I	Sh. Eg.
	12 37	P. M.	II	Oc. Dis.
	5 03	"	II	Ec. Re.
30	2 15	A. M.	I	Oc. Dis.
	5 18	"	III	Tr. In.
	5 28	"	I	Ec. Re.
	7 30	"	III	Tr. Eg.
	9 31	"	III	Sh. In.
	11 35	"	III	Sh. Eg.
	11 24	P. M.	I	Tr. In.
31	12 25	A. M.	I	Sh. In.
	1 38	"	I	Tr. Eg.
	2 38	"	I	Sh. Eg.
	7 50	"	II	Tr. In.
	9 52	"	II	Sh. In.
	10 16	"	II	Tr. Eg.
	12 16	P. M.	II	Sh. Eg.
	8 45	"	I	*Oc. Dis.
	11 57	"	I	Ec. Re.

NOTE.—In. denotes ingress; Eg., egress; Dis., disappearance; Re., reappearance; Ec., eclipse. Oc. denotes occultation; Tr., transit of the satellite; Sh., transit of the shadow; * Visible at Washington.

A Partial Eclipse of the Moon will occur on March 21. It will not, however, be visible in the United States except in the extreme western part just as the Moon is setting. It will be visible in Alaska, the Pacific Ocean and Asia. At the middle of the eclipse 0.248 of the Moon's diameter will be obscured. The following are the elements of the eclipse as given by the *American Ephemeris*:

Greenwich mean time of opposition in right ascension March 21, $1^h\ 27^m\ 17^s.1$.

Sun's right ascension	$0^h\ 03^m\ 24^s.38$	Hourly motion	$9^s.10$
Moon's right ascension	12 03 24 .38	Hourly motion	120 .73
Sun's declination	$0°\ 22'\ 10''.1$N	Hourly motion	$0'\ 59''.2$N
Moon's declination	0 36 09 .5N	Hourly motion	16 29 .3S
Sun's equa. hor. parallax	8 .6	Sun's semi-diameter	16 02 .9
Moon's equa. hor. parallax	58 10 .5	Moon's " "	15 50 .4

TIMES OF THE PHASES:

	Gr. Mean Time. h m	Central Time. h m	Pacific Time. h m
Moon enters penumbra	March 21, 11 57.4 A. M.	5 57.4 A. M.	3 57.4 A. M.
Moon enters shadow	1 25.3 P. M.	7 25.3 "	5 25.3 "
Middle of the eclipse	2 20.6 "	8 20.6 "	6 20.6 "
Moon leaves shadow	3 15.7 "	9 15.7 "	7 15.7 "
Moon leaves penumbra	4 43.7 "	10 43.7 "	8 43.7 "

Elongations of the Satellites of Saturn.

[In the diagram the points marked 0 are those of eastern elongation of the several satellites. Their positions at intervals of one day after eastern elongation are indicated by the symbols 1*d*, 2*d*, etc.]

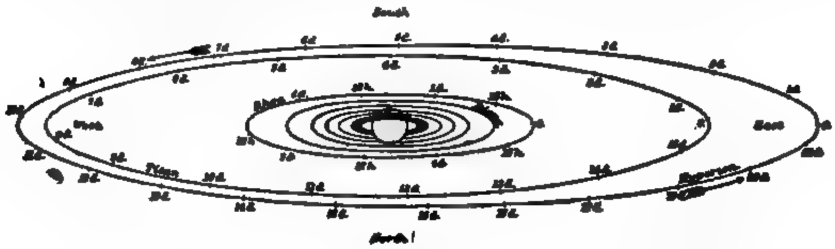


DIAGRAM OF THE APPARENT ORBITS OF SATURN'S SATELLITES.

MIMAS.

		h		
Mar.	2	3.2	A. M.	E
	3	1.8	"	E
	4	12.4	"	E
	4	11.0	P. M.	E
	9	4.8	A. M.	W
	10	3.4	"	W
	11	2.0	"	W
	12	12.6	"	W
	12	11.3	P. M.	W
	18	3.6	A. M.	E
	19	2.3	"	E
	20	12.9	"	E
	20	11.5	P. M.	E
	21	10.1	"	E
	26	3.8	A. M.	W
	27	2.5	"	W
	28	1.1	"	W
	28	11.7	P. M.	W
	29	16.3	"	W

ENCELADUS.

ENCELADUS CONT.

		h		
Mar.	21	4.3	A. M.	E
	22	1.2	P. M.	E
	23	10.1	"	E
	25	7.0	A. M.	E
	26	3.9	P. M.	E
	28	12.7	A. M.	E
	29	9.6	"	E
	30	6.5	P. M.	E
Apr.	1	3.4	A. M.	E

TETHYS.

Mar.	2	11.9	A. M.	E
	4	9.2	"	E
	6	6.5	"	E
	8	3.8	"	E
	10	1.1	"	E
	11	10.4	P. M.	E
	13	7.7	"	E
	15	5.0	"	E
	17	2.3	"	E
	19	11.6	A. M.	E
	21	8.9	"	E

DIONE CONT.

		h		
Mar.	22	9.2	P. M.	E
	25	2.9	"	E
	28	8.6	A. M.	E
	31	2.2	"	E

RHEA.

Mar.	2	9.9	A. M.	E
	6	10.3	P. M.	E
	11	10.7	A. M.	E
	15	11.1	P. M.	E
	20	11.5	A. M.	E
	24	11.8	P. M.	E
	29	12.2	"	E

TITAN.

Mar.	4	12.9	P. M.	W
	8	2.8	"	S
	12	9.9	A. M.	E
	16	7.4	"	I
	20	10.9	"	W
	24	12.9	P. M.	S
	28	7.9	A. M.	E

Minima of Variable Stars of the Algol Type.

[Given to the nearest hour in Central Standard Time.]

U CEPHEI.

R. A. .... $0^h\ 52^m\ 32^s$
Decl. .... $+81°\ 17'$
Period .... $2d\ 11^h\ 50^m$

Mar.	3	7 A. M.
	5	7 P. M.
	8	6 A. M.
	10	6 P. M.
	13	6 A. M.
	15	6 P. M.
	18	6 A. M.
	20	5 P. M.
	23	5 A. M.
	25	5 P. M.
	28	5 A. M.

ALGOL.

R. A. .... $3^h\ 1^m\ 1^s$
Decl. .... $+40°\ 32'$
Period .... $2d\ 20^h\ 49^m$

Mar.	6	4 A. M.
	9	1 "
	11	10 P. M.
	14	7 "
	17	3 "
	20	12 noon
	23	9 A. M.
	26	6 "
	29	3 "

λ TAURI.

R. A. .... $3^h\ 54^m\ 35^s$
Decl. .... $+12°\ 11'$
Period .... $3d\ 22^h\ 52^m$

Mar.	1	3 P. M.
	5	2 "
	9	1 "
	13	noon
	17	11 A. M.
	21	9 "
	25	8 "
	29	7 "

R CANIS MAJORIS.

R. A. .... $7^h\ 14^m\ 30^s$
Decl. .... $-16°\ 11'$
Period .... $1d\ 3^h\ 16^m$

Mar.	1	10 P. M.
	3	1 A. M.
	4	5 "
	5	8 "
	6	11 "
	7	2 P. M.
	8	6 "
	9	9 "
	10	12 midn.
	12	3 A. M.
	13	7 "
	14	10 "
	15	1 P. M.
	16	4 "
	17	8 "
	18	11 "

R CANIS MAJ., CONT.

20	2 A. M.
21	5 "
22	9 "
23	12 noon.
24	3 P. M.
25	7 "
26	10 "
28	1 A. M.
29	4 "
30	8 "
31	11 "

S CANCRI.

R. A. .... $8^h\ 37^m\ 39^s$
Decl. .... $+19°\ 26'$
Period .... $9d\ 11^h\ 38^m$

Mar.	6	1 P. M.
	16	1 A. M.
	25	1 P. M.

S ANTLIÆ.

R. A. .... $9^h\ 27^m\ 30^s$
Decl. .... $-28°\ 09'$
Period .... $0d\ 7^h\ 47^m$

Mar.	1	5 P. M.
	2	12 midn.
	3	11 P. M.
	4	11 "
	5	10 "
	6	9 "
	7	9 "
	8	8 "
	9	7 "
	10	7 "
	11	6 "
	12	5 "
	13	12 midn.
	14	12 "
	15	11 P. M.
	16	11 "
	17	10 "
	18	9 "
	19	9 "
	20	8 "
	21	7 "
	22	7 "
	23	6 "
	24	5 "
	25	5 "
	26	12 midn.
	27	11 P. M.
	28	11 "
	29	10 "
	30	9 "
	31	9 "

δ LIBRÆ.

R. A. ....$14^h\ 55^m\ 06^s$
Decl. .... $-\ 8°\ 05'$
Period .... $2d\ 07^h\ 51^m$

Mar.	1	5 A. M.
	3	1 P. M.
	5	9 "
	8	5 A. M.
	10	1 P. M.
	12	9 "
	15	5 A. M.
	17	12 noon
	19	8 P. M.
	22	4 A. M.
	24	12 noon
	26	8 P. M.
	29	4 A. M.
	31	11 "

U CORONÆ.

R. A. .... $15^h\ 13^m\ 43^s$
Decl. .... $+32°\ 03'$
Period .... $3d\ 10^h\ 51^m$

Mar.	4	1 A. M.
	7	12 noon
	10	11 P. M.
	14	10 A. M.
	17	9 P. M.
	21	7 A. M.
	24	6 P. M.
	28	5 A. M.

U OPHIUCHI.

R. A. .... $17^h\ 10^m\ 56^s$
Decl. .... $+\ 1°\ 20'$
Period .... $0d\ 20^h\ 08^m$

Mar.	1	2 A. M.
	2	10 P. M.
	3	6 "
	4	2 "
	5	10 A. M.
	6	6 "
	7	2 "
	7	11 P. M.
	8	7 "
	9	3 "
	10	11 A. M.
	11	7 "
	12	3 "
	12	11 P. M.
	13	8 "
	14	4 "
	15	12 noon
	16	8 A. M.
	17	4 "
	17	12 midn.
	18	8 P. M.
	19	4 "
	20	1 "
	21	9 A. M.
	22	5 "
	23	1 "

U OPHIUCHI Cont.			Y CYGNI.			Y CYGNI Cont.	
Mar.	23	9 P. M.	R. A.	20ʰ 47ᵐ 40ˢ	Mar.	16	2 A. M.
	24	5 "	Decl.	+ 34° 15′		17	2 P. M.
	25	1 "	Period	1*d* 11ʰ 57ᵐ		19	2 A. M.
	26	9 A. M.	Mar. 2	2 P. M.		20	2 P. M.
	27	6 "	4	2 A. M.		22	2 A. M.
	28	2 "	5	2 P. M.		23	2 P. M.
	28	10 P. M.	7	2 A. M.		25	2 A. M.
	29	6 "	8	2 P. M.		26	2 P. M.
	30	2 "	10	2 A. M.		28	1 A. M.
	31	10 A. M.	11	2 P. M.		29	1 P. M.
			13	2 A. M.		31	1 A. M.
			14	2 P. M.			

Test Objects for Small Telescopes.

[From Clark and Sadler's "Star Guide."]

Dividing Tests.

Name of Objects.	R. A. h	R. A. m	Decl. °	Decl. ′	Distance. ″	Position Angle. °	Magnitudes.		Aperture of telescope
14 Orionis	5	02	+ 8	20	1.15	203	5.8	6.0	4
4 Lyncis	6	12	+ 59	25	0.95	101	6.2	7.5	5
Σ 1333	9	11	+ 35	56	1.69	42	6.7	7.0	3
OΣ 229	10	42	+ 41	42	0.84	330	6.5	7.5	6
Σ 1606	12	05	+ 40	32	1.21	338	6.2	7.0	[illegible]
Cor. Bor. 1	15	14	+ 27	15	1.22	308	5.6	6.1	4
μ^2 Boötis	15	20	+ 37	47	{ 0.78 108.4	104 172	6.5 4.5	7.8 }	6
λ Ophiuchi	16	25	+ 2	14	1.65	44	4.4	5.4	3
OΣ 313	16	29	+ 40	21	0.99	152	7.0	7.6	5
Cephei 83	20	59	+ 56	13	1.62	349	6.4	6.8	3

Defining Tests.

Name of Objects.	R. A. h	R. A. m	Decl. °	Decl. ′	Distance. ″	Position Angle. °	Magnitudes.		Aperture of telescope
ψ^2 Orionis	5	21	+ 2	59	2.66	324	5.4	9.0	4
42 Orionis	5	30	− 4	55	1.73	218	5.5	9.2	6
κ Leonis	9	18	+ 26	41	{ 3.36 10.	205 65	5.1	10.1 11.5 }	6
49 Leonis	10	29	+ 9	14	2.39	158	6.2	8.4	3
84 Virginis	13	37	+ 4	07	3.56	234	5.7	8.0	3
6 Serpentis	15	15	+ 1	08	2.28	13	4.7	9.4	6
η Draconis	16	22	+ 61	46	5.26	142	2.8	9.0	4
68 Herculis	17	13	+ 33	14	4.41	62	5.1	10.1	5

NEWS AND NOTES.

It is possible that some subscribers may not receive the February number of this publication, either because they have not renewed their subscription, or because we have failed to notify them of the expiration of the same. We will be greatly aided in mailing if every subscriber, who has not already done so, will at once advise us whether or not he desires the publication continued.

Professor Schaeberle's Photographs of the Corona of the Total Solar Eclipse, April 16, 1893.—In the October number of this magazine we published a large plate showing the interior corona as obtained by Professor J. M. Schaeberle while in South America. In connection with that plate it was stated that owing to the delay in the reproduction of the plates, it was necessary to hold the long exposure photograph for publication in the next number. The photogravure company who had the work in charge after a further delay of a month, finally said it was impossible to bring out the details expected in the long exposure photograph of the outer corona and so gave up the task wholly. Another photogravure company in the city of Milwaukee tried to produce the second plate and likewise failed. Neither of those in charge of this particular work was satisfied with the plate secured, and after two further trials, at considerable expense, followed by complete failures, it was decided to give up further attempts.

Gold Medal for Professor Burnham.—Cablegrams from London announce the award of the Gold Medal of the Royal Astronomical Society to Professor S. W. Burnham of the University of Chicago for his discoveries and micrometrical measures of double stars and for his researches on the orbital motions of Binary Systems. This news is especially welcome to American astronomers, and will be favorably received throughout the scientific world, for no observer either living or dead has contributed more to this important branch of modern astronomy than has Professor Burnham, whose discoveries of new and very close pairs have created an epoch in the history of Double Star Astronomy. The discovery of double stars, begun by Sir William Herschel more than a century ago, and since continued by William and Otto Struve, Herschel and Mædler, Dawes and Dembowski, was regarded twenty-five years ago as practically exhausted. But the genius of Burnham working with only a six-inch telescope soon brought to light hundreds of close pairs never before detected, and opened the way to later discoveries of priceless value. Professor Burnham afterwards secured for a time the use of the Dearborn 18-inch refractor, and the Madison 15-inch, and thus extended the list of measures and new discoveries. His work at the Lick Observatory is too recent to need recalling to the readers of this Journal, but it may not be inappropriate to remark that his own stars now number nearly 1300, and include the most rapid and interesting pairs in the heavens.

It is understood that these stars will be made the object of special attention at the Yerkes Observatory, and that they will be carefully followed until their orbits are accurately known. Professor Burnham's catalogue of his new stars and his general catalogue of all the important stars in the northern hemisphere are to be printed among the first volumes issued by the Yerkes Observatory, and will constitute works on Double Star Astronomy which are destined to be "aere perennius."

The high honor conferred upon Professor Burnham is a tribute to pure science which will be fully appreciated by all American astronomers, but it is especially gratifying to the intimate friends of this modest, unselfish and renowned observer.

Other American astronomers who have received this Medal of recent years are.—Professor Simon Newcomb, Professor Asaph Hall, Dr B. A. Gould, Professor E. C. Pickering, and Dr. G. W. Hill. Last year the Gold Medal was awarded to Dr H. C. Vogel, and the preceding year to Professor G. H. Darwin.

No new comets have been discovered since October of last year. Brooks' comet *c* 1893 was barely visible in our 16-inch telescope on the night of Jan. 26. It was too faint to admit of measurement.

We looked very carefully for Holmes' comet on the same night but could find no trace of it. On the night of Jan. 12 we took a photograph of the region in which this comet should be, giving an exposure of an hour with the new 6-inch camera. There is a slightly oval stain, 20′ in diameter, on the negative just where the comet ought to be. It has no central condensation and is so suspiciously like a dirty water stain that we hesitate to say anything about it without verification, which we have as yet been unable to get.

Discovery of Comet *b*, 1893.—Notes on the independent discovery of this comet by Messrs. Rordame, Quenisset, Miller, Johnson, Roso de Luna, Sperra, have been printed in these *Publications*, 1893, pages 154-5. A full account of Mr. Sperra's observations is given in ASTRONOMY AND ASTRO-PHYSICS, 1893, page 757. The Committee on the Comet Medal, having carefully considered the case, and having asked the advice of the editors of the leading astronomical journals, has adopted the following resolutions

I. That a copy of the Comet-Medal shall be struck, having the *obverse* as usual and the *reverse* blank, and that on the reverse of this copy shall be engraved the words

To Commemorate the Discovery of Comet b, 1893.

II. That this Medal shall be preserved in the cabinet of the Astronomical Society of the Pacific, and no award made for the discovery of this comet.

III. That a copy of No. 32 of the Society's *Publications* shall be sent to each of the gentlemen named above

Committee on the Comet-Medal

EDWARD S. HOLDEN,
J. M. SCHAEBERLE,
CHAS. BURCKHALTER

[Dated]

December 1, 1893 —*Publications of the Astronomical Society of the Pacific, No. 32.*

An International Cipher Code for Aurora Observations.—Dr. M. A. Veeder of Lyons, New York, read a paper at the Chicago Meteorological Congress, August, 1893, bearing title "An International Cipher Code for Correspondence Respecting Aurora and Related Conditions." The points made in that paper are

1 Selection of such points as are important for observation and code-correspondence

2 The place should involve more than the data for preservation that are incidentally secured.

3. There should be a system of intercommunication that would promptly

furnish the views of sun-spots, magnetic storms and auroras continuously if possible.

4. There ought to be a daily synoptic chart to go with the ordinary daily meteorological data.

Any persons interested in taking meteorological observations in connection with the system now recommended by him should correspond with him for instructions. His excellent work in this direction is gaining world-wide attention.

Quick Adjustment of the Equatorial.—On the hypothesis that the maker has placed the axes at right angles with each other, and the tube at right angles with the declination axis, the following adjustments remain to be made by the astronomer, viz.:

The adjustment of the finder.

The bringing of the polar axis into the merdian.

The setting of the circles.

The elevating of polar axis to the latitude of the place.

In the first place when the telescope *head* is placed upon the pier make the polar axis cut the meridian *west* of *north* and *east* of *south*.

To adjust the finder bring a star into the center of large glass, then by means of the adjusting screws attached to finder bring the star to center of field of latter. The finder is then adjusted.

To bring the polar axis into the meridian, turn the instrument to zenith (approximately), place a lever of precision north and south across the lower (eye) end of tube (which is at right angles to line of collimation), turn instrument on decl. axis until the bubble comes to center, (clamp in decl. axis), turn the level east and west across end of tube. Turn instrument on polar axis until bubble comes to center. The instrument now points to the zenith. Set the hour circle vernier to 0 hours and 12 hours respectively. Set the decl. circle to the latitude of the place (the latter is only approximately correct). Loose in decl. axis and turn the objective down to take in a star (Sirius) about to cross the meridian. Bring the star to center of the field, hold it there by shifting north end of instrument to the *east* in the pier until the sidereal time indicates star's R. A. The polar axis is now in the meridian.

To elevate the polar axis to the latitude of the place, turn the instrument to a star of known decl. (Sirius). If the axis is at the proper elevation the decl. circle will read the star's declination. If the circle does not thus read (and this is usually the case) note the differences between the circle reading and the star's decl. as given in the catalogue. Shift the decl. circle *one-half* of this difference, bring the tube again into the meridian by means of the hour circle, and set the decl. circle to read the latitude of the place. (Do this by turning instrument on decl. axis). Place the level north and south across the end of tube as before; bring the bubble to center by raising or turning (as the case may be) the north end of instrument, by means of the adjusting screws.

Test your adjustment by setting instrument for some star and see if it appears in field on correct sidereal times.

By means of the preceding method the writer has put an equatorial into good adjustment in a single evening. L. W. C.

Volume XXV of the Astronomical Observatory of Harvard College has been received. It contains the comparison of positions of stars between 49° 50′ and 55° 10′ of north declination in 1855.0, and observed with the meridian circle

during the years 1870 to 1884, by William A. Rogers under the direction of Joseph Winlock and Edward C. Pickering.

Table I gives the values of the systematic corrections required to reduce the co-ordinates given by different catalogues to the system of the Astronomische Gesellschaft. Table II is a comparison of the computed with the observed positions. Then follows the separate results of 8612 zone stars in right ascension and declination for the Epoch of 1875.0 and reduced to the system of the Gesellschaft, separate results of zone stars for the proper motions μ and μ' and other minor features.

The values of the proper motions given in this volume are to be considered as provisional, and the values of the reductions to the system of this zone catalogue are given in table I. These numbers are the results of three approximations. In the first it was assumed that all residuals exceeding 0.5^s in right ascension and $5''$ in declination were due to proper motion and were omitted in the first discussion. In the second approximation these values of the proper motions were introduced in a second discussion of systematic deviations from the system of the zone catalogue and new values of the proper motions were then obtained. At this point in the discussion, it became possible to distinguish with considerable certainty between residuals which were due to a real progressive movement in the stellar position and those which were due to the accidental error of observation. The list of selected stars was revised at this point, and residuals exceeding the values in right ascension and declination above given which appeared to belong to the class of accidental errors of observation were still excluded from the list. In the third approximation the same order of procedure was followed, and values given are the sum of the three sets of corrections obtained in this manner.

This volume is a work of years, by Professor Rogers, a part of which only is represented in its printed pages. The skill and care represented in its results are best known to those astronomers acquainted with the details of his elaborate work.

Volume XXIX of the Harvard College Observatory Publications contains six papers as follows:

1. Meteorological and other observations made at Willows, California, in connection with the total solar eclipse of Jan. 1, 1889, by W. Upton and A. L. Rotch.

2. Longitude of Smith College Observatory by Mary E. Byrd and Mary W. Whitney, from which is derived the following result:

The longitude of Smith College Observatory west of Greenwich is

$$4^h\ 50^m\ 33^s.096 \pm 0^s.044$$

3. Photometric observations of Asteroids by Henry M. Parkhurst.
4. Observations of Variable Stars by Henry M. Parkhurst.
5. Magnitude of bright stars north of + 70.
6. Relative places of β Persei and companion stars, from observations with the Meridian Circle by Arthur Searle.

An Extraordinary Phenomena.—On the morning of the 20th of December a celestial body of extraordinary appearance was observed by the citizens of North and South Carolina and Virginia. We quote a description received from Wilmington, N. C., through the kindness of Mr. E. S. Martin.

The weight of the evidence goes to show this remarkable fact that the lumi-

ous body passed from about West, S. of E., until it reached a point some 15° about the eastern horizon when it seemed to pause and remain stationary apparently for the space of 15^m or 20^m and then disappeared. This can be accounted for (if a meteor) by the path being along the line of sight from the observer, and if a spectroscope had been used this could have been determined and its rate of motion.

That it was a tremendous body is evident from all the reports; those here and elsewhere reporting its appearance as about the size of a large table 6' to 8' in diameter, others as large as a hogshead, and others a wheel of 6' to 8' diameter, that it was brilliantly white throwing off sparks—moved apparently over this city towards the eastern horizon a little north of where the Sun rose, that the sound of its motion through the air could be heard, that it left an intensely thick stream of vapor nearly its entire way from west to east which was visible for 30^m and even after sunrise for some time, and which slowly drifted, and towards the east assumed a zigzag shape. According to some very intelligent witnesses, after remaining apparently stationary, as heretofore mentioned, it seemed to explode though no sound was heard and a rain of stars (as they expressed it) fell from it towards the horizon and near the point where the Sun had just risen.

The time of passing this point was about 6^h 30^m A. M., E. S. T., Dec. 20, 1893 and the body remained visible until 6^h 45^m when it exploded as aforesaid.

The Chicago Academy of Sciences--Section of Mathematics and Astronomy, Jan. 3rd, 1894.—The annual meeting was held at the Dearborn Observatory, Evanston, Ill., Professor Hough, President, in the chair. After the transaction of routine business, the President introduced Professor Malcolm McNeill, of Lake Forest University, who read a paper on "*The Life and Times of Tycho Brahe.*" The speaker began by recalling the conditions which had surrounded Tycho in his youth, and gave an account of the circumstances which led him to become an astronomer, of which one of the most important was the celebrated new star that blazed forth in Cassiopeia. He then discussed with care the instruments employed by Tycho, and the degree of precision of which they were capable. It was shown that some of them had the form of circles mounted on polar axes, which enabled the observer to find the declination of the body and the arc by which it was out of the meridian, the latter being measured on a circle at right angles to the axis of the movable circle. Professor McNeill also pointed out the rough similarity of Tycho's sights to those employed by modern astronomers, and observed that in dealing with his arcs Tycho had corrected for the so-called "eccentricity of the sextant." The speaker gave an interesting account of the influence of Astrology on Tycho, and of his official relations to the King of Denmark, and in turn sketched the mystic influence of Tycho upon Kepler and his successors. At this time Astrology was hardly distinguished from Astronomy. Professor McNeill did not think Tycho's rejection of the Copernican system was very remarkable, considering the roughness and inaccuracy of the data then available, but he said Tycho's real greatness rests upon his fine observations which proved so valuable to Kepler, and upon the energetic effort made to eliminate all possible sources of error. He called attention to the great importance of Tycho's work in forming a new catalogue of stars independent of that of Ptolemy, which had been generally employed by preceding astronomers. In conclusion the speaker gave the important sources to which astronomers are indebted for their knowledge of Tycho, and commended the work of Dr. Dreyer on the "*Life of Tycho*

Brahe" as a reliable and interesting contribution to the literature of the subject

Dr. Kurt Laves of the University of Chicago read the second paper of the evening on the "*Application of the so-called Lunar Equation in the Motion of the Earth for the Determination of Certain Important Constants in Astronomy*" After having derived the equation and discussed the constants entering into it, he called attention to the fact that the method of using the Lunar Equation for a determination of the solar parallax is unavailable because the constant of Nutation which is introduced into the equation by the mass of the Moon, produces a very large probable error in the determination of Solar Parallax. By a total differentiation of the Lunar Equation it follows that the equation is to be applied only for a determination of the mass of the Moon, and by introducing the resulting mass of the Moon into the equations which follow for the Precession and Nutation, we get a very reliable method for finding by a theoretical process the Constant of Nutation, the probable error of which is $\pm 0''.012$ This probable error is about the same as that in the determination of the Nutation from the right ascensions of stars near the pole. The weak point which has hitherto rendered the results inexact lies in the large probable error of $P = 6''.52 \pm 0''.02$ But the author stated that a more accurate value of this constant of the Lunar Equation could be deduced from careful micrometrical measures of small planets which come near to the Earth at the time of opposition than could ever be found from observations of the Sun. He stated that Dr. Gill had recently obtained a very good value of P by means of heliometer observations of small planets, and said that it would be of high importance if similar measures could be undertaken in America. Dr Laves gave the value of the Constant of Nutation resulting from the above method as

$$N = 9''.255 \pm 0''.012.$$

In the discussion which followed, Professor Hough, Professor Burnham and several others took part.

The present officers were re-elected for the ensuing year, after which the meeting adjourned. T J J. SEE, Recorder

Astronomical and Physical Society of Toronto, Canada.—Dec. 26th meeting, Chairman, Mr. Robert B. Ellis.

Letters read from Dr. Joseph Morrison, F. R. A. S., of Washington, promising papers, from Professor E. C. Pickering, LL. D., Director of Harvard College Observatory, accepting honorary membership

Mr. Mungo Turnbull of Toronto was elected an active member

Librarian G. G. Pursey reported reception of a copy of Professor J. E. Keeler's finely illustrated paper on "Physical Observations of Mars during 1892," sets of *The Sidereal Messenger* and of ASTRONOMY AND ASTRO-PHYSICS for several years; also several volumes on various subjects, gifts of members

Mr. George E. Lumsden presented a photograph of some beautiful frost-tracery on a window-pane, also drawings of Jupiter and Venus delineated at the telescope during bright sunshine.

Mr. A. F. Miller reported seven noteworthy groups of sunspots, three in the northern hemisphere and four in the southern. He said these spots could be shown well by projection on a sheet of white paper, simply using a telescope made with ordinary spectacle lenses. Messrs. Miller, A. Elvin, R. Dewar and W. Collins said the spectacle-lenses could be made round by paring with strong scissors, danger of breakage being avoided by holding them under water

Mr Lindsay called attention to the valuable scientific papers made available by the recent binding of the reports received from the Royal and other societies.

Miss A. A. Gray read from *Popular Astronomy* an interesting article by Professor E. E. Barnard of Lick Observatory, describing the peculiar appearances presented on the mornings of Oct. 21, 22 and 23, 1893, by the Brooks comet (*d* 1893). John A. Copland contributed a paper on the same subject by Mr. E. W. Maunder, F. R. A. S., of Greenwich Observatory, published, with copies of Mr. Barnard's negatives, in *The London Daily Graphic*.

Meeting of Jan. 9, 1894.—Fourth annual meeting. Chairman, Vice-President John A. Paterson, M. A., delivered the annual address, eloquently reviewing the past year's work.

Mr. F. H. Young, M. A., of Belleville, was elected an associate member.

On motion of Mr. Thomas Lindsay, seconded by Mr. D. Geo. Ross, the following officers and members of council were unanimously re-elected for the year 1894: Honorary President, Hon. G. W. Ross, LL. D., Minister of Education; President, Mr. Charles Carpmael, F. R. A. S., F. R. S. C., Director of the Toronto Observatory; Vice-Presidents, Mr. Larratt W. Smith, D. C. L., Q. C., and Mr. John A. Paterson, M. A.; Treasurer, Mr. James Todhunter; Assistant Treasurer, Miss S. L. Taylor; Corresponding Secretary, Mr. G. E. Lumsden; Recording Secretary, Mr. Charles P. Sparling; Assistant Recording Secretary, Miss A. A. Gray; Librarian, Mr. G. G. Pursey; Assistant Librarian, Miss Jeane Pursey. The council is composed of the executive officers and Messrs. E. A. Meredith, LL. D., A. Elvins, A. F. Miller, A. Harvey and D. J. Howell.

Mr. John A. Copland was re-elected British and foreign correspondent.

Having signified their assent, which was done in terms flattering to the society, Professor E. C. Pickering, LL. D., Director of the Harvard College Observatory, and Professor S. P. Langley, LL. D., Secretary of the Smithsonian Institution, were elected honorary members, and Professor James Keeler, LL. D., Director of the Allegheny Observatory, was elected a corresponding member.

Mr. Pursey laid on the table various publications including the latest reports of the British Astronomical Association and The Astronomical Society of the Pacific, as well as a presentation advance copy of the first volume of "Webb's Celestial Objects for Common Telescopes," just published by, and received direct from Messrs. Longmans, Green & Co., of London. In addition to the compliment thus paid by the publishers, Rev. T. E. Espin, F. R. A. S., editor of the new edition, in his preface, credits the Society with having suggested valuable features to be introduced into the book. The first volume deals entirely with the solar system, the second with the stars.

Mr. Todhunter, the treasurer, presented an encouraging annual report, showing that the assets of the Society, including telescopes, globes, books, etc., had been valued at nearly $1,100, that there are 103 active members, 22 life, honorary and corresponding members, and 8 associate members, and that, after meeting all engagements, there was a small balance in bank.

Mr. Sparling reported that during 1893 twenty-five regular meetings had been held, with an average attendance that indicated a well-sustained interest, summer and winter, in the work of the Society.

On motion of the treasurer, the annual fee for active lady members was placed at $1, and the life membership fee for a lady member at $10.

Miss A. A. Gray read a list of phenomena.

A striking display of aurora in the northeastern sky at 6.10 P. M. on Wednesday, 3rd January, was described by Messrs. John A. Copland, G. G. Pursey, Andrew Elvins, J. Hollingworth, of Beatrice, and a lady member. It was also observed by Dr. Larratt W. Smith, Mr. W. T. Moore and many other citizens. Mr. Copland said when he first noticed the display, all the streamers were deep crimson, seeming to roll into each other, and then shoot toward the zenith. At 6.20, the deep red color was rent in two places by white bars, a brightly white streamer being toward the east. White and red kept interchanging, with crimson predominating, until 6.30, after which the display gradually faded.

JOHN A. COPLAND.

PUBLISHER'S NOTICES.

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James E. Keeler, Observatory, Allegheny, Pa.; Henry Crew, Northwestern University, Evanston, Ill.; Jos. S. Ames, Johns Hopkins University, Baltimore, Md.

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Manuscript for publication should be written on one side of the paper only and *special care should be taken to write proper names and all foreign names plainly*. All drawings for publication should be *smoothly* and *carefully* made, in *India Ink* with lettering well done, because such figures are copied exactly by the process of engraving now used. If drawings are made about double the size intended for the printed page, better effect will be secured in engraving than if the copy is less in size. It is requested that manuscript in French or German be typewritten. If requested by the authors when articles are sent for publication, *twenty-five* reprint copies, in covers, will be furnished free of charge. A greater number of reprints of articles can be had if desired, at reasonable rates.

Rates for advertising and rates to news agents can be had on application to the publisher of this magazine.

CONTENTS FOR JANUARY, 1894.

Astronomy and Astro-Physics.

VOL. XIII, No. 3. MARCH, 1894 WHOLE No. 123.

General Astronomy.

PHOTOGRAPHIC NEBULOSITIES AND STAR CLUSTERS CONNECTED WITH THE MILKY WAY.

E. E. BARNARD.

That region of the Milky Way lying north and east of Orion, is singularly rich in large diffused nebulosities. I have lately secured a number of photographs of this part of the sky with the Willard lens (6 in. diameter, 31 in. focus.)

Some of these nebulosities were previously known to me, but they are so large and so diffused that their observation and location with a telescope is a matter of difficulty and uncertainty. They are, however, specially suitable for the photographic plate.

After the opening of this Observatory in 1888, while sweeping over this region, I found a very large, weak, diffused nebulosity some half a degree south of the nebula N. G. C. 2245. This was mixed up with several considerable stars. I also found a 10 mag. nebulous star about half a degree north preceding 2245.

In the photographs all these nebulæ are clearly shown, along with others not seen previously.

The large diffused nebulosity, mentioned, is shown very strongly on the plates, as an irregular elliptical mass about 36′ in diameter. It involves several considerable stars. DM + 10° 1159 and + 10° 1160 being respectively in the northern and southern parts of the nebula. Its position would be closely represented by the mean of the position of these stars, or for

$$1860.0 \quad 6^h\ 23^m\ 27^s \quad + 10^\circ\ 7'$$

close preceding DM + 10°.1159 is a singularly well defined vacancy, like a comma, in the nebulosity.

N. G. C. 2245 is shown as also is 2247—though from the small extent of nebulosity about this latter star it is but little blurred on the plate. Visually with the 12 in. the nebulosity is decided.

The 10 mag. nebulous star, before mentioned, is clearly shown, with a quite decided nebulosity. A small dark space exists in its nebulosity, also, south of the star. Position of this nebulous star is 1860.0 $6^h\ 23^m\ 14^s \pm 3$ $+ 10^\circ\ 32'.6 \pm$.

The most remarkable object in this region, however, is the nebulosity connected with 15 Monoceros (this nebulosity is questioned in N. G. C).

This group of bright stars is mixed up with a knotted and wispy nebulosity extending faintly and irregularly north and westerly to an extent of nearly 2°. It is a very wonderful object and with a longer exposure — which I hope soon to give — promises to be one of the most remarkable nebulæ known It is irregular in outline, but quite well defined, with numerous black gaps running into it, and in general conforming with the peculiarities of the Milky Way in that region, showing it to be actually mixed up with the stars of the Milky Way, resembling in this respect a large nebula I have recently found by photography in Cepheus. There is a sinuous, vacant strip in the Milky Way here that runs from 15 Monoceros, at first to the west for some 3°, and then northerly nearly to γ Gemini.

These remarks refer to a photograph made 1894, Jan. 24, with $2^h\ 30^m$ exposure with ξ Geminorum central. Another exposure, covering nearly the same region, of $3^h\ 35^m$, made Jan. 25, confirms the statements, but from the poor condition of the sky it scarcely showed more than the shorter exposure.

In *A. N.* 2918 I have given an account, along with a sketch, of a large nebulous ring enclosing the cluster G. C. 1420, the nebula itself being N G. C. 2237. The place of G C. 1420 for 1860.0 is $6^h\ 23^m\ 29^s\ +5°\ 2'.5$.

The nebula (2237) was discovered by Swift very many years ago, and was independently found by me in January, 1883.

The sketch referred to was made with the 12-inch in 1889.

I have recently (Jan. 9, 1894) secured a fine photograph of this object with the Willard lens. As stated in *A. N.* 2918, from its diffused nature this nebula is specially suited for photography. This photograph verifies the statement most emphatically and shows how utterly impossible it is to adequately deal with such an object visually.

I will quote from my previous article on this subject in *A. N.* 2918, in speaking of its appearance with the 12-inch in 1889:

"What I had seen previously and what Swift had sketched, was simply a brightish knot in a vast nebulous ring that entirely surrounded the cluster. By estimation the average outer diameter of the ring is 40′ and the inner diameter 20′. The inside of the ring is apparently free of nebulousity, the stars of the cluster shining on a perfectly dark sky. The outer edge of the ring is somewhat diffused and irregular, some projections occuring near

the following portion. The inner edge is more definite and especially so following—it is less definite in the preceding part. In the north preceding section of the ring are several knots, the largest of which, *a*, is the one previously seen by Swift and myself. I am not sure that there is not a very small break in the continuity of the ring at the point *b*. South following the ring and close to it is the nebulous section of a large ellipse which seems to be a portion of another great ring; I am not sure that this is not connected with the first by a nebulous strip."

Comparing the photograph with my sketch, I find the sketch is correct so far as it goes, but with the 12-inch I had grasped only the brighter details—the great mass of it not being seen in the 12-inch.

The photograph shows the nebula to be about 1° in diameter and very irregular in brightness and outline. It is a mass of unequally condensed nebulosity involving the star cluster and specially heavy north of the bright stars. The nebulous knots or condensations, shown in my sketch, are conspicuous on the photographs, as is also the "nebulous section of a large ellipse," which is connected with the main mass; the full extent of this section is shown in the sketch.

The photograph shows that there is no nebulosity—or if any it is very feeble—immediately about the bright stars. They apparently shine in a vacant space in the south part of the nebula.

The entire nebula seems to be definitely terminated and to leave no suggestion of a greater extent being revealed through a prolonged exposure.

One degree south of the center of the nebula, and free of it, and following about ½°, is a very thin nebulous strip 10′ or 12′ long extending north and south with a faint star in its south end, like a slender comet with a nucleus.

In one of these photographs north and east of Orion there is an extremely faint and large diffused glow near the stars ν and ξ Orionis. This extends south of ξ and acquires a slight density in about $6^h\ 5^m + 13°$. It is the most diffused glow of nebulosity I have yet seen on any of the photographs. It might almost be called the ghost of a nebula, so faint and vague is it.

I have now covered, photographically, a large portion of the Milky Way—from the Scorpion to Orion—securing characteristic photographs of the different regions. These pictures are very wonderful. They give us views not only beautiful but most intensely interesting and valuable. One thing very apparent from them is that the features of the Milky Way do not repeat them-

selves and that the different regions have apparently a different order of structure as well as a different order of brightness of the stars. In one region the cloud forms will consist of coarse stars while in another they are made up of stars apparently comparable with dust particles. This may be due in the main to a greater distance from us in the one case But I believe that many of these cloud forms are made up actually of comparatively small stars.

As we ascend from the south through the Scorpion, Sagittarius, etc , we find here and there nebulæ and compressed clusters scattered over the Milky Way, with nothing to imply any but an accidental connection of them with the Milky Way. But after entering Cygnus we come to a region in which vast masses of diffused nebulosity are present and are unquestionably actually mixed up with the ground work of stars. From this on as far as Monoceros we meet here and there with these affected areas.

These masses, however, are almost invariably mixed up with a group of stars brighter than the general average in that region A magnificent specimen of these I have found on one of my plates in Cepheus which was given an exposure of seven hours. This is a mixture of bright stars and nebulosity. The diffused portion of this nebulosity conforms in its peculiarities with the general structure of the Milky Way, showing it to be actually mixed up with the ground work of stars

The brightest star of this group is DM + 56° 2617, and its position for 1855 0, 21^h 34^m $29^s.8$ + 56° 49′.7.

These collections of nebulosity and stars are extremely interesting and suggestive taken in connection with the possible light they may throw upon the nebular theory. Indeed the loose clusters of bright stars can readily be separated into classes.

In the first there is no nebulosity mixed with the stars, such are the Hyades, the Dolphin, Præsepe in Cancer, M 11, the well known cluster in Perseus, etc.

In the second, stars and nebulosity are freely mixed together, such are the Pleiades, G. C., 1420, 15 Monoceros, M 8, the nebula in 6^h 23^m + 10° 7′, the great nebula and cluster in Cepheus just referred to. G. C. 1366 is also of this class, as one of my plates shows it to be a mixture of bright stars and nebulosity. The nebulosity is about ½° in diameter. In the 12 in. the 8m star referred to in G. C. is found to be surrounded with a faint and small but decided nebulosity. In the finder (3¼-in.) the 8^m star is seen to be surrounded by a group of small stars—the whole being a loose cluster. The cluster is enveloped in feeble nebulosity. This shows strongly on the plate.

A splendid example of this admixture of stars and nebulosity is Wolf's great nebula near α Cygni, a fine photograph of which I have secured with 366 minutes' exposure.

If the nebular theory is true we have here the process of evolution readily illustrated—where a group of stars is being evolved from diffuse nebulous matter. According to this idea the second class of star clusters is still in an unfinished condition—the process of evolution still going on before us, while in the first class the work of Sun making is complete.

There is one point, however, and it may be an important one, where the Pleiades differ from the rest of these nebulous clusters. In its case the nebulosity is condensed about the individual stars, in nearly all the other clusters referred to the nebulosity does not seem to attach itself to any individual star but to simply involve the group, the stars themselves not showing any special tendency to condensation individually.

A photograph which I have made in 1892 with five hours exposure was χ Cygni, shows γ Cygni to be surrounded by numerous large patches and strips of nebulosity.

I have sent a list of other photographic nebulosities to *Knowledge* along with several photographs for reproduction, which will perhaps appear in the February or March number of that journal.

Mt. Hamilton, 1894, Feb. 1.

Postscript.—On another photograph taken Feb. 1, 1894, with $2^h\ 10^m$ exposure, the $9^m.5$ star Dm + 23 .1313 is found to be closely nebulous, a very small dense nebulosity gives it a fuzzy appearance. This nebulosity is heaviest south and following. The place of the star for 1885.0 is

$$\alpha = 6^h\ 11^m\ 35^s.8 \quad \delta = +\ 23^\circ\ 19'.3.$$

On this same plate is a faint narrow curved nebulosity in about, 1860.0,

$$\alpha = 6^h\ 8^m \quad \delta = +\ 23^\circ\ 0'$$

It is nearly $\frac{1}{2}^\circ$ long, extending north and south and convex to the east.

The cluster M35, occupying the middle of the plate, does not show any evidence of nebulosity. This therefore is one of the non-nebulous clusters. It is however a very beautiful object.

The condensed cluster N. G. C. 2158, south preceding M35, also does not show any traces of nebulosity.

The nebulous star N. G. C. 2175 which is better shown on this plate than on previous ones, is quite a striking object. The nebulosity is about $\frac{1}{2}^{\circ}$ in diameter, nearly circular, but irregular at the preceding edge. The 8 mag. star is in the center of it. The density of the nebulosity in its preceding part is somewhat irregular. Otherwise it is nearly uniform in its light. There are quite a number of considerable stars mixed up with it north.

Another photograph of the region about 15 Monoceros on February 1, 1894, with 3 hours' exposure, shows the great nebula that envelopes 15 Monoceros very much better than the previous picture. Its full extent—in diameter—can be taken roughly at 3°. It clusters densely about the groups of stars and then spreads out in a weak, diffuse light with rifts in it and irregularly terminated along the edges of a vast vacancy in the Milky Way. The condensation, which is very strong, is not at 15 Monoceros but 12′ south preceding that star, where it becomes a compact mass, with numerous wisps and holes in it. The whole group of three or four bright stars are involved in this denser wispy light, but 15 Monoceros itself does not seem to be specially connected with the nebulosity further than to be apparently in it—that is there are no indications of condensation about this the brightest star of the group. This remarkable nebula—the denser part of it—is worthy of study with a more powerful photographic telescope. The condensed part of the nebula is only a few minutes in diameter—but it would readily photograph in a large instrument. The place of 15 Monoceros for 1860.0 is $\alpha = 6^h\ 33^m\ 16^s + 10°\ 1'.3$. It is thus described in the catalogue (N. G. C.) "15 Monoceros Cl !, ? neb."

During the exposure for this picture, at $10^h\ 6^m$-7^m Standard Pacific Time, the sky was suddenly illuminated, comparable to that from a full moon, this quickly increased for two or three seconds and then disappeared. Rushing quickly to the slit in the little dome, I examined the sky in all directions for some traces of a meteor, but no train could be seen. From the moment I saw the light until I was examining the sky did not exceed 10 seconds. This I knew must be the light from an enormous meteor, in the north or east, but as I was facing the south I saw only the illumination from it. Mr. Perrine, however, was more fortunate as he saw the meteor itself in the east. What struck me as remarkable was the sudden disappearance of all traces of the meteor.

On another photograph February 2, 1894, with 3 hours exposure, in the region of δ Cassiopeia, there are shown two very singular fan-shaped patches of nebulosity close to γ Cassiopeia. One is a little following and north of γ; the other slightly north and

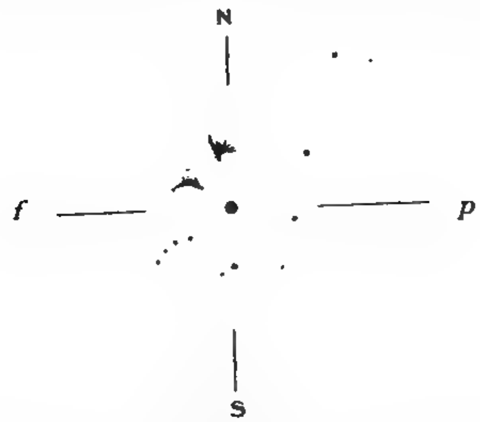
following. Each is about 24′ distant. These are about 15′ in diameter and point nearly towards the star.

In *Knowledge* for January, 1894, p. 17, I have a paper on the subject of Photographic Nebulosities. At the close of that paper I have called attention to what is apparently a large diffused nebulosity about α Orionis.

I have there cautioned any one from accepting this as real until verified. The star falls near the edge of the plate and I have no means of telling if it is real or not. Unfortunately I have not yet been able to get another picture with exposure sufficient to decide if this is real. All the other nebulosities I have mentioned, are verified either by the telescopic observation or by another photograph. This, however, is not verified and possibly is not real.

February 3, 1894.

Postscript No. 2.—Another photograph of the region about γ Cassiopeia on February 6 with 1 hour and 50 minutes exposure, again shows the two queer looking nebulæ near γ.



Nebulæ Near γ Cassiopeia.

In the latter half of the exposure the sky became thick and finally stopped the work. Before beginning this exposure, I carefully examined the sky close to γ with the 12-inch and a power of 80, with a field of 42′. The sky was fine. It was with the utmost difficulty that I could see these two nebulæ. They were excessively dilute and faint, and never would have been detected if the photographic plate had not revealed them. They, however, photograph very readily, and I think would be shown with considerably less than an hour's exposure.

The enclosed sketch shows their position with reference to γ Cassiopeia. It is on twice the scale of the original negative.

February 7, 1894. E. E. Barnard.

ELECTRICAL CLOCK CONNECTIONS FOR OPERATING THE CHRONOGRAPH.*

G. W. HOUGH

The earliest employment of a clock for making automatic signals by the use of electricity, was by Wheatstone in 1841. The clock was made to make and break the circuit every minute, by means of a disk attached to the escapement wheel arbor. During the preliminary stages of the chronographic method for recording transits in 1848-9, there was considerable discussion regarding priority in the invention of an automatic connection for making a clock record its beats on a moving fillet of paper. It would appear from the evidence that several persons at about the same time independently solved the problem. Professor O. M. Mitchel, Director of the Cincinnati Observatory; Professor W. C. Bond, Director of the Harvard Observatory; Dr. John Locke, Cincinnati, and Joseph Saxton, Esq., U. S. Coast Survey, each devised a clock connection.

O. M. Mitchel used a light wire cross supported on a horizontal axis and connected with the bottom of the pendulum by means of a spider's web. At every alternate oscillation of the pendulum one arm of the cross dipped in a cup of mercury, thereby completing the electric circuit. In my use of this connection at the Cincinnati Observatory in 1859, considerable trouble was experienced from the breaking of the spider's web. I soon found that the apparatus was just as effective by dispensing with the web and allowing the pendulum to strike the arm of the cross and tilt it enough to complete the circuit. The apparatus in this form I subsequently used for a number of years at the Dudley Observatory. The friction required to be overcome for operating the cross, materially affected the rate of the clock. If the clock was rated to run free, when the connection was installed, the rate was changed about two seconds daily.

W. C. Bond used a light flat spring six inches in length, terminating in a platinum point. The spring was bent to form a "V" at the center, and was mounted horziontally near the middle of the pendulum rod. By means of a pin secured to the pendulum, at every oscillation, the platinum point was lifted out of a mercury cup in which it rested, thereby breaking the circuit.

Dr. John Locke used a tilt-hammer struck by the teeth of the escapment wheel (solid connection).

* Communicated by the author.

Joseph Saxton arranged a tilt-hammer, which was struck by a piece of glass projecting from the middle of the pendulum (solid connection).

In 1851, the U. S. Naval Observatory used a clock by Frodsham, arranged with two knife edges at the bottom of the pendulum, which simultaneously passed through separate globules of mercury. I am not informed who devised this form of clock connection.

During the early experiments with clock connections, it was thought that it would be injurious to the clock to allow the electric circuit to pass through it, and hence the aim was to construct a connection which would not be open to this objection.

At Greenwich in 1854, contact springs pressed together by the pendulum at every oscillation, were used, but as the clock rate was not satisfactory, the method was changed. Later, the connection was made by means of contact-springs operated by a wheel of 60 teeth placed on the escapement shaft.

The employment of a single globule of mercury at the bottom of the pendulum, according to Chauvenet, was suggested by Joseph Saxton, U. S. Coast Survey.

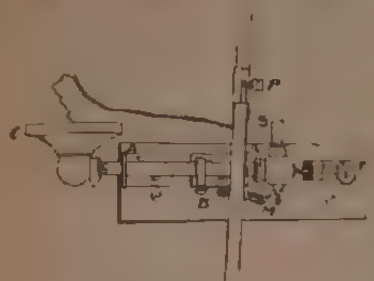
At an early date a break-circuit was devised in Germany, in which two reservoirs of mercury, having capillary tubes inserted in the side, were employed. The ends of the capillary tubes were brought so nearly together that the mercury formed a bridge between them. A plate of mica, attached to the pendulum, cut the thread of mercury at every oscillation, thereby breaking the circuit.

In 1860, Dr. F. Brunnow used a light bar magnet about two inches in length, which was pivoted at the bottom and set in a vertical position. When this apparatus was placed in front of the pendulum at the center of oscillation, the bar needle was attracted by the steel mercury jar and the magnetic contact was broken every second. Dr. Brunnow found that in order to make it work well, it was necessary to use a very weak battery and a relay. I used this connection for a short time in 1860 at the Dudley Observatory. The description of the apparatus will be found in Brunnow's Astronomical Notices, June, 1860. This apparatus is entirely disconnected from the clock, and when used with a weak battery, has a very slight effect on the rate. If, however, it is used in an ordinary circuit, there would be sticking between the platinum surfaces, requiring a more powerful magnet, and hence, absorbing so much more energy.

From 1867 to 1870, for operating the O. M. Mitchell disk

chronograph, I used a wire arm about 4 inches in length attached to the pendulum at right angles near the point of suspension. A fine platinum wire at the end of the arm dipped vertically in a cup of mercury at every alternate oscillation. The effect of this connection on the clock's rate was practically *nil*, but one cannot use it with the ordinary chronograph because the circuit is closed for too long a time. This connection was used for two consecutive years without being disturbed.

In 1870, I adopted the then well-known method of causing a platinum wire attached to the pendulum, to pass through a globule of mercury placed at the bottom, or preferably near the middle of the pendulum. Instead, however, of using an isolated column of mercury, which requires frequent renewal and is *not* constant in its effects on the clock rate, I used a siphon communicating with a large reservoir of mercury, thereby keeping the globule of mercury constant for a year or more without attention. A connection of this kind was used on a clock at the World's Columbian Exposition, for operating the Printing Chronograph. And the foreign astronomers who saw it were especially interested in its examination. As a description has never been published, a working drawing showing its construction in detail may be of value.



Clock Correction.

In the diagram, *p*, platinum wire attached to the pendulum by means of a collar and set screw, so that it may be set at the proper height, and also turned to bring the wire over the centre of the globule of mercury. *S*, an iron siphon made of ⅜-inch gas pipe and inserted in the bottom of the mercury cup *C*. The cup *C* is made of hard wood and may have a diameter of two or more inches. The siphon is supported at *a* on a block of hard wood and mounted, so that it may freely slide in the direction of its length. The collar *b* has a pin on the under side which works in a slot. By means of the set screw in the collar *b*, the siphon may be adjusted in a vertical position. A milled head screw *t* presses against the siphon leg, and a spiral spring *m* holds the siphon firmly against the screw *t*. The object of the adjusting screw *t* is to be able to make the seconds equal without stopping or interfering with the clock. When the connection is set up, the quickest and most accurate adjustment is made by allowing the pendulum to swing,

so that the platinum wire will just pass over the mercury globule and break circuit equally on each side. If this adjustment is properly made, the connection will operate a year or more without attention. By having a surface of mercury of the dimensions specified, the loss from oxidation will not lower the level of the globule an appreciable amount for a long period.

When I used this apparatus at the bottom of the pendulum, the siphon was fastened to a flat iron bar of the same length as the pendulum, in order to compensate for the effect of temperature. But when used at the middle of the pendulum, it is not of so much importance. In all forms of mercury connections, however, if impure mercury is used, the oxide may sometimes adhere to the point of the platinum wire, in which case the circuit will not be established for some seconds after being closed. A drop of kerosene oil placed on the globule will usually eliminate this source of trouble. If, however, one uses pure mercury and not less than seven cells of gravity battery, the connection is no more liable to fail than if made between platinum surfaces.

Recently I have employed the gravity cells in an indirect manner. Seven gravity cells are joined in series with three small storage cells, and the current for operating the chronograph derived from the latter. By this plan, one gets nearly the same voltage and a considerable increase in the amperes, provided the circuit is of low resistance. Gravity cells used in this manner, will run six months without cleaning; it is necessary, however, to add water and sulphate of copper from time to time.

The connection which I use when operated by a twelve pound pendulum, describing a semi-arc of 1°.5, changes the normal rate of the clock 0.11 sec. daily, and the semi-arc of oscillation is shortened about 0°.03. If applied to a heavier pendulum, the effect would be still less in distributing the normal rate.

In all the methods hitherto devised, however, for making a clock record automatically, more or less work is required to be done, which is prejudicial to the maintenance of a uniform rate. The chief objection to the solid connection, lies in the fact that too much work is required to be done by the clock. As friction is never constant, it should be reduced to a minimum to get the best results.

METEORIC ASTRONOMY

DANIEL KIRKWOOD.

III.

The Andromedes, or Biela Meteors.

Another swarm of meteors, now well known, is that produced by the dissolution of Biela's comet. The story of this wonderful body, its separation into two parts in 1845, its further breaking up into fragments so small as to be separately invisible, except when rendered luminous by coming in collision with the atmosphere of the Earth—all are now matters of familiar history.

The two parts into which Biela's comet separated had, before passing out of sight in 1846, attained a distance from each other of more than 200,000 miles. This distance before the next perihelion passage in 1852 had become 1,500,000 miles, and before another period had been completed, the fragments, by further division, had become invisible. In 1872, however, the separated parts were seen as a meteoric shower. The phenomenon was again repeated in 1885, when, during the display, a meteoric stone of ten pounds weight, fell at Mazapil, in northern Mexico. This interesting object is therefore regarded by some astronomers as an actual part of Biela's comet. The last shower from this cluster, November 23, 1892, was very generally observed throughout the United States. It was thus noticed by the *Scientific American* of December 17th:

"Persons who happened to be in the open air on Wednesday evening, November 23, had the privilege of witnessing a phenomenon of more than ordinary interest. A brilliant display of celestial fireworks commenced about six o'clock and lasted several hours. Meteors at the rate of several hundred per hour were watched and counted by numerous spectators. The writer, at early twilight, counted 150 meteors in thirty minutes. Later, a neighbor made the number 700 per hour, and the whole number seen at a single station during the display must have amounted to thousands. How are the phenomena to be accounted for? How frequently do they occur? And when may they be again expected? were questions asked by many observers during the display in southern California.

"Aged persons remember Biela's comet, a telescopic body having a period of six years and eight months, or three periods in twenty years. One of its returns was due in the latter part of 1845. Instead of appearing alone, as on former returns, it was

seen as two separate bodies, as far apart as the Moon and the Earth. The dissolution of this wonderful body had therefore commenced, and the return (in 1852) was accordingly looked for with still increasing interest. It came true to time, but the fragments still further apart. That was the last time it ever appeared *as a comet*. It was due in 1859, 1865, or 1866, 1872, 1878, 1885, and in November, 1892. This process of falling to pieces began, as we have seen, in 1845, and advanced from year to year till the fragments became too small to be individually seen. They pass, however, through our atmosphere, become ignited, and are thus rendered visible. In a work entitled "*Comets and Meteors*," published several years since, it was said of this shower's predecessor: "This cometary mass will be in close proximity to the Earth about the last of November, 1892. Another brilliant meteoric shower may therefore be expected at that epoch."* This is the shower just seen, as predicted in 1873. The study of these periodic showers has established many facts in regard to their phenomena—facts now to be found in recent works on astronomy.

At Leland Stanford, Jr., University, Palo Alto, California, the meteors were observed by Professor W. J. Hussey. A single person could see on an average from 50 to 60 fairly bright meteors every five minutes, "corresponding to a daily rate of 400,000,000 to 500,000,000 on the hemisphere turned to the radiant." Mr. C. D. Perrine of San Francisco, observed from 7^h 32^m to 8^h 50^m, and in this interval of 1^h 18^m counted one thousand and thirteen. "An observation of a few moments again at 10^h 15^m showed them seemingly as frequent as before."†

From different parts of the country the present writer received newspaper reports, as well as MS descriptions. A somewhat careful comparison gave 300,000,000 as an approximate estimate of the whole number visible during the fall of six hours. Efforts of this nature, however, may yet be regarded as premature.

Of the several star showers now well known, the Andromedes afford the best opportunity for satisfactory study. The Leonids visit us but once in 33 years. The Perseids have become so widely diffused around the orbit as to be regarded annual visitors; but with their spread around the pathway of the comet their simultaneous numbers have become inconsiderable. The Andromedes appear in large and interesting swarms; the period is between six and seven years. The life of Biela's comet was but 80 years

* Comets and Meteors, p. 88.
† Pub. A. S. P., Vol. IV, p. 255

—from 1772 to 1852. The very interesting discovery of the relation between comets and meteors had not been made when the dissolution of Biela's comet was announced in 1846. It may not be wise to anticipate future developments. Whether the present condition of the August stream indicates decadence—the final dissipation of its constitutional parts, as well as that of other streams are questions which may be left for future astronomers. The sporadic shooting stars observed on every clear night may be but fragmentary remains of meteor streams long since extinct.

History.—This interesting body was discovered by Montaigne, at Limoges, March 8, 1772. At its second known apparition it was seen by Pons, November 10, 1805. Its periodicity, however, was not discovered till after its third appearance, when seen by Biela, February 27, 1826. At this return its elliptic motion was demonstrated. Its first *predicted* apparition—the fourth observed,—was in 1832, when it was detected by a French astronomer. The fact of its crossing the Earth's path in November of that year having been misunderstood by the illiterate public, created temporary alarm. Its position in 1839 was unfavorable for observation and it escaped detection. In 1845-6 occurred the well known and often described division into two parts, and in 1852, its last return *as a comet.* The meteoric visits have been already described, at least those from 1872 to the present time. It is now known, however, that the comet had been in the process of dissolution before 1845. The meteoric phenomena of 1838 and of 1798 were undoubtedly derived from this source.* In short, the facts as now known, indicate that several cometary or meteoric bodies have, since 1772, or perhaps for some time before that epoch, been moving nearly in the path of Biela's comet. The meteors seen by Brandes in 1798 as well as those reported by E. C. Herrick December 5-7, 1838; also the body detected through the agency of Klinkerfues in Germany, and Pogson in Madras, in 1872,—all doubtless belong to the same cluster. Its future phenomena will be watched with interest

THE SMALL NEBULA AT M. 57.†

EUGEN VON GOTHARD

In reference to the small nebula found by E. E. Barnard of the Lick Observatory near the Ring Nebula of Lyra, I beg to say

* See the catalogues of Quetelet and E. C. Herrick. Also Newton's lecture "The Story of Biela's Comet." *Am. Journal of Sci.*, February, 1886.

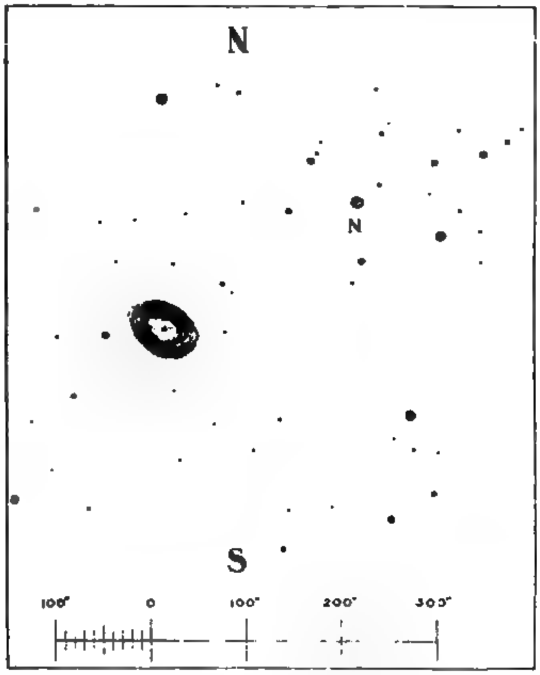
† Translated by Miss Madelaine Hummel

that it has been photographed by me years ago; it can be seen on some of my plates, but it is so small and looks so much like the surrounding stars, that it was impossible to identify it.

Of the numerous photographs, which I have taken of the Ring Nebula in order to study the small central star, which I discovered by means of photography, I have chosen those that have been exposed the longest, such as:

I.	1888, June	13, $9^h\,50^m - 11^h\,50^m$;	Exp. = 2 hours.	
II.	1891, Sept.	2, $10^h\ 7^m - 11^h\ 7^m$ 3, $9^h\,20^m - 11^h\,20^m$	" 3 "	
III.	1891, Sept.	8, $9^h\ 5^m - 12^h\ 5^m$ 9, $8^h\,55^m - 11^h\,55^m$	" 6 "	

I have measured in the Plate I, which shows the most sharply defined pictures, the coördinates (α and δ) of the stars, and drawn



enlarged to twenty times the original the accompanying sketch, comparing it with Plate III, and perfecting it, so as to show all the stars that can be seen on Plate III. The nebula is marked N.

I found the distance of the nebula from the central star of the Ring Nebula in Plate III to be as follows:

$$\text{Dist.} = 2.316 \text{ millimeters} = 245''.6$$
$$\text{Pos. Angle} = 305°$$

Unfortunately the Ring Nebula has been so much over exposed, that the star can hardly be seen, which greatly diminishes the correctness of the measurements; on the other plates the nebula is too faint for any measurement.

Considering the coincidence with the results given by Mr Barnard,* I think I may take it for granted, that I have not mistaken some other object for the nebula, but that it really is on my plates.

On this occasion I must again refer to the inestimable advantages of stellar photography by means of which I was enabled to confirm those observations which have been made with the best, the largest and the most favorably situated instrument in the world.

HERENY ASTRO-PHYSICAL OBSERVATORY
January 24, 1894.

A PHOTOGRAPH OF THE PLEIADES AND TWO ASTEROIDS.

H. C. WILSON

A photographic plate was exposed to the Pleiades for four hours on the night of Jan. 30, by the writer at Goodsell Observatory. The telescope used has an 8-inch objective with three lenses by the Clarks. It was driven by clock-work and the errors produced by irregular driving and refraction were corrected every minute or two by the observer, who was looking through a 5-inch finder at the star Alcyone, keeping it continually bisected by two cross-wires. As a result a very fine picture was obtained of the nebula involving nearly the whole group of bright stars and exhibiting marvelous details of structure resembling those of the great nebula of Orion. The curious straight lines of nebulosity running in some cases from star to star are shown, but are not quite so narrow and hard-edged as shown in the reproductions of previous photographs. The connection of the nebula with the brighter stars of the Pleiades is so obvious that one could hardly doubt it after inspecting the photograph.

All the star images are round and well defined, the very faintest star visible in our 16-inch telescope being easily seen on the plate, while many more can be made out with a magnifying glass.

* *Astronomiche Nachrichten*, No. 3200. Dist. 242″.8. Pos. Angle 303°.3.

The structure of the nebula about Merope is very curious. There are many approximately parallel and slightly curved lines of light passing the star at an angle of about 30° to the meridian. The same structure is apparent, though less marked, in the diffuse part of the nebula which extend to the south and east of Merope. From the southern edge of the image of electra a bright streak of nebulosity about 20″ in width, proceeds on a straight line toward Alcyone. It extends about one-third of the way to Alcyone, tapering to a point. About 1′ south of this a narrower parallel streak extends half the distance of the former, and 5′ south another streak, almost as bright as the first and nearly parallel, extends from a point 2′ west of Electra to a point 5′ or 6′ northwest of Merope, taking in on the way the two brightest stars between Electra and Merope. Through the middle of the group and especially around Alcyone the background is filled with mottled patches of nebula. A series of elongated patches form a line extending from the star almost midway between Alcyone and Maia eastward, joining a curved row of stars, almost to the limits of the group.

The region about Maia is especially interesting. A very bright hornshaped patch of nebula runs out from the west edge of the star image curving immediately northward, and extends to a distance of 3′ north of the star. The nebula here is full of irregularly parallel streaks similar to those about Merope but making only a very small angle with the meridian. Some of them run to and beyond the bright stars north of Maia. A series of rather broad and diffuse patches extend from the middle of the group on a diagonal toward the northwest, reaching to a pair of comparatively bright stars in that direction.

All these features agree very closely with those in the reproduction of a photograph by Mr. Isaac Roberts, given in *Knowledge* May, 1891.

Among the stars on the plate were found two straight lines each about 1.2 mm. long, having, aside from their length, the same structure as the star images. These were at once suspected to be minor planets which, because of their motion during the four hours of exposure, impressed lines instead of round dots upon the plate. When these were examined under the microscope a gap was found in the middle of each trail corresponding to the five minutes in the middle of the exposure, when the plate was covered in order that the driving clock might be wound and the telescope readjusted. This completely verified the supposition that the lines were the trails of minor planets. The same aster-

oids were photographed again, one of them on Feb. 1 and both on Feb. 3. We have measured their approximate positions from the three photographs obtaining the following results:

Asteroid.	Central Time.			R. A.			Decl.		
		h	m	h	m	s	°	′	″
a.	Jan. 30	9	22	3	38	13.2	+ 23	27	49
	Feb. 1	7	32	3	39	27.5	+ 23	28	57
	Feb. 3	8	30	3	40	51.6	+ 23	30	38
b.	Jan. 30	9	22	3	41	24.6	+ 24	50	29
	Feb. 3	8	30	3	44	17.0	+ 24	54	43

The brightness of an asteroid is somewhat difficult to estimate from the trail, since the rate of its motion as well as the duration of exposure enters as a factor into the intensity of the trail. As an approximation we may divide the trail into parts equal in length to the diameter of star images of the same intensity. The ratio of brightness will be the number of parts thus obtained. It remains then to determine the brightness of the stars thus used for comparison.

In the present case the asteroid trail *a* was found to be equal to ten stars whose magnitude was estimated to be 14 on Argelander's scale. This according to the usual formula would give the magnitude of *a* as $14 - 2.5 \log 10 = 11.5$. Asteroid *b* was found to be equal to twelve stars of the fifteenth magnitude, its resulting magnitude being $15 - 2.5 \log 12 = 12.3$.

The identification of an asteroid in the list of nearly four hundred is something of a task unless an accurate ephemeris happens to have been computed for that particular one for the time of the observation. A large number of such ephemerides is published in the *Berliner Jahrbuch*. Each covers, however, only one month near the time when the planet is at opposition, and in the present case the region photographed was not opposite the Sun. Another table in the *Jahrbuch* gives the time of opposition, and the right ascension and declination at that time, of each minor planet A little study of this table will generally enable the observer to exclude all but five or six of the known asteroids as too far from the given region. For the remaining number it is necessary to calculate the latitude and longitude or right ascension and declination of each from the elements of its orbit, in order to compare them with the same coordinates measured from the photograph. Where the elements have been brought up to date, this process is not so very difficult, but when the elements given belong to an epoch several years back it is necessary to calculate the perturbations of the orbits by the large planets, a process involving much labor.

In this instance the asteroids Nos. (33), (184), (196), (203), (207), (235) and (309) were found by inspection to be somewhere in the vicinity of the region of sky photographed. The calculation of the latitudes and longitudes, however, showed that only (203) was within the region of the photograph, its place falling within 3^m of longitude and 5′ of latitude of that of *a*. The two were therefore assumed to be the same. The asteroid *b* is probably a new one, although it is possible that perturbations which were not allowed for in reducing the elements of (207) and (309) from 1889 and 1890 respectively to 1894 may have sufficiently changed their orbits to bring one or the other of them into the place of *b*.

Since the above was written a telegram has been received from Berlin *Rechen-institut*, to which the observations were referred, giving the following ephemeris of *b* from circular elements determined by Berberich. We infer from this that the planet is a new one and that further observations are desirable.

Greenwich M. T	R. A.			Decl.	
	h	m	s	°	′
Feb. 23.5	4	04	28	+ 25	30
27.5	4	09	28	+ 25	[illegible]
Mar. 3.5	4	14	48	+ 25	48

TWO NEW VARIABLE STARS.*

M. FLEMING

A recent examination of the photographs of stellar spectra forming part of the Henry Draper Memorial work at the Harvard College Observatory, Cambridge, has led to the discovery that the stars A.G.C. 157 in R.A. 0^h $10^m.4$, Dec. − 32° 36′, Magn. 8, and B.D. + 1° 3417 in R.A. 17^h $14^m.5$, Dec. + 1° 37′, Magn. 9.5 are variable. The first named star is in the constellation Sculptor and its magnitude varies from 6.5 to 10. The second is in the constellation Ophiuchus and it varies from the magnitude 8.5 to 12.5. The approximate positions for 1900 are those given above.

HARVARD COLLEGE OBSERVATORY,
Cambridge, Mass., Feb. 13, 1894.

* Communicated by Edward C. Pickering, Director of Harvard College Observatory.

COMETARY SHOOTING STARS*

HUBERT A. NEWTON

I have to apologize somewhat in that I came to the rooms not expecting to speak to you. I have, however, one point which I think will interest the members of this Society if they will give me a few minutes to develop it, and that is the force which acts on the small bodies sent off from comets and which form our shooting stars.

There are in the comets so many questions that we cannot answer, so many curious and wonderful phenomena that are unexplained, that I am sure you will accept my explanation of any of them that seems plausible, as a matter of interest. From a comet there is continually driven off matter forming the tail, a light substance, and astronomers are agreed that the force that acts on the matter which forms the tail is a repulsive force from the Sun acting inversely as the square of the distance, the force of the repulsion being greater than that of attraction.

Not only is this true, but different parts of that tail are acted upon by repulsive forces of different powers; otherwise the tail would form across the sky a single line intead of a broad, expanded mass of light such as we see. From the comet, however, there are driven off also, or there are separated, other things entirely distinct from the tail, small bodies, which are not thus driven away, which are not visible, but follow along closely in the path of the comet, and whenever the occasion comes, that is, when we go through a group of them, those give us our shooting stars.

The Biela comet, in the period about 1840, passed near to Jupiter. At that time it was turned pretty sharply out of its orbit, the inclination of the orbit being turned several degrees, and the node being carried forward also several degrees, represented by several days in the time at which we crossed the path of the comet.

After 1840 the bodies which formed the meteors that were met in 1872 and in 1885 were separated from one or other parts of the Biela's comet. I say after 1840, because if they had been separated earlier they would have given us a different radiant in the skies, the one given by the Biela meteors of 1838. The radiant was changed, the node was changed, all to correspond to the new

* Address reprinted Nov. 22, 1893, from Proceedings *Amer. Philos. Soc.*, Vol XXXV., as corrected by the author.

orbit, and these bodies could not have been turned in that way had they been before scattered, because the force that acted on them, the attraction of Jupiter, would have scattered the group instead of giving us that single compact group through which we passed in 1872 and 1885 in the course of four or five hours, and the bulk of them in even two hours.

In 1872, the comet was something like 200,000,000 miles away from the bodies that we met as we passed through them on the 27th of November, giving us a brilliant shower Thirteen years later we passed through the group again and then we were something like 300,000,000 miles ahead of the comet. So that some of the particles leaving the comet between 1840 and 1870, had fallen behind and others between 1840 and 1885 had gained.

What should separate those particles? What are the forces which carried off those particles so many miles—200,000,000 miles on the one hand and 300,000,000 miles on the other, in round numbers? The force that acts on them must be a force acting in one plane, that is, the plane of the orbit of the comet. Any force acting in other planes would have scattered the group and we would not have met them as a single definite group at the times named; but if it acts in the plane, only scattering them on the plane, they would be together as we saw them.

In that plane, it must be either an impulsive force acting once or it must be a constant force acting continually. The only bodies in that plane are the comet and the Sun, and if the force is a continuous force it must be from the comet or from the Sun. It is almost inconceivable to suppose that the comet could have sent them off, either impulsively or continuously, in such a way as to give us the distance of 200,000,000 and 300,000,000 miles in the course of thirty years; it would require far more than any velocity that we can give in our terrestrial experiments, and we have no reason to suppose that there is any such power of impulsion. Moreover, if the impulsion came from the comet, they would go in all directions and their character, as being in a plane, would have been entirely lost.

We are then thrown back on this one hypothesis, that the Sun is the source of that force. In other words, we are led to extend the idea that I gave you in the beginning, and which is accepted by astronomers, that the material which goes off from the comet, after it leaves it, is subject to a force like that of attraction but differing in its intensity. In the case of the tail, it is a repulsive force. To satisfy these conditions of separation, part in one direction and part in the other, from the comet, we must have an

attraction in the one case exceeding the attraction of gravitation and, in the other, an attraction less than the attraction of gravitation. In other words, these little bodies of hard matter that go off from the comet and follow very nearly in its train are acted on not in proportion to their mass like the force that acts on the planets in their orbits.

I see no escape, myself, from this conclusion. What it means, I must leave to you to decide. Our experiments make it very improbable that the attraction of matter differs in any way from proportion to the mass. It looks to me as though the more natural explanation is that, in some way, the materials which go off from the comet carry with them a load of electricity, or something of that kind, by which they have a permanent repulsion or permanent attraction sufficient to change the orbit altogether, not in kind, but in a steady change, throwing them into a new orbit with a new peroid, and thus scattering them.

What that added force must be, we cannot very well tell, because it differs according to the place in the orbit where the disintegration takes place. If that disintegration takes place near the Sun, it is one thing; if it takes place near Jupiter, it is another. It looks more to me as though there was a disintegration all along the line of the comet's orbit, giving us small particles with all sorts of loads of electricity and all sorts of differences of central attraction and differences of orbits, and thus they get widely scattered so as to give us the showers a long distance from the comet itself. The amount of this change would have to be something like the tenth part, possibly, or something less than that. I should think that all the phenomena could be explained by a change amounting to one-tenth of the attraction; that is, if the small particle carries a load of electricity such as to diminish the attraction to say nine-tenths of the original attractive force of the Sun, or increase it to eleven-tenths, it will explain the phenomena.

If that is the explanation, we come to this further conclusion of interest, that the space through which these comets move is not such that the electricity which the particle carries can be lost. Another practical point would be that, in the discussion of the separation of these comet masses that through the telescope we see going off as the comets pass the Sun, there might fairly be introduced an unknown correction of the force of central attraction.

A MEMBER: Have you gentlemen, who have made a study of this very interesting subject which you have been discoursing on,

arrived at any hypothesis as to what broke up the Biela comet?

PROFESSOR NEWTON: I can only answer as a working hypothesis, in my own mind, is that a mass, not surrounded by an atmosphere, coming down from the cold into a warmer region near the Sun, becomes heated up, and in that heating there is a disintegration going on. If you put the pieces of a meteorite into a vacuum, and heat them, you will get gases that will be something like those which are thrown off from the tail of a comet, and the comet coming down near the Sun, with the hot, scorching effect entirely undiminished by a thick atmosphere, would have pieces broken off, giving fresh surfaces. An immense amount of action of some sort follows, and those pieces would naturally go off under such excitement, carrying with them, as I conceive, a load of electricity. The process goes in on almost all our comets. It is not in Biela alone that we see comets going off to pieces. Scores of comets have shown that same breaking up under the telescope.

LONG FOCUS TELESCOPES

WILLIAM ROLLINS

The obstacle to the increase in size and number of large telescopes is the cost. How can this be reduced? As the chief expenses of a great equatorial are the mounting and the dome with its necessary machinery these must be given up. Several other types have been proposed but when telescopes having focal lengths of several hundred feet are used only the horizontal will be practical. At present this is but little used and before its introduction can become general some new form must be given to it to overcome the difficulty of controlling the revolving mirror when the observer is stationed at the other end of the telescope many feet away. To be sure the rate of the driving mechanism can be controlled from any distance by electricity, but for setting the mirror the observer ought to sit beside it.

I propose to reflect the light from the revolving mirror to a fixed mirror distant half the focal length of the objective, and to send it back again to the observer sitting in a chair beside the driving mechanism. In the cut A is the revolving mirror, B the objective, C the fixed mirror and G the eyepiece or sensitive plate. If the focal length of the objective is one thousand feet then the fixed mirror will be five hundred feet from the revolving mirror.

If the reflecting form is chosen there will be no objective and the fixed mirror will be parabolic and placed at its focal length from the eyepiece at G. This form of mounting will be cheap, only two wooden sheds being required, one for the fixed mirror, the other larger and divided by a partition into two rooms, one for the mirror with its driving mechanism, the other, a dark room taking the place of a camera inside which the observer will sit while looking at the image on the plate in front of him thus doing away with a guiding telescope. Rods and wires placed at his side will enable him to control the revolving mirror, while a slide in the partition will admit of direct access to the driving mechanism. A telescope of this type with a focal length of five hundred feet ought to be of value in lunar photography even if the aperture was not larger than twelve inches. The two most probable causes of failure will be the difficulty of building a driving mechanism perfect enough to project a steady image so far and distortion of the image from flexion of the mirrors. I propose to overcome the last by mak-

[redacted]

Astro-Physics

A STUDY OF NOVA AURIGÆ AND NOVA NORMÆ.*

WILLIAM H. PICKERING.

Considering that there are but about a dozen well authenticated instances of the appearance of new stars in the history of astronomy, it is an interesting fact that the last two should have appeared within two years of one another, and that our earliest record of their existence should in each case be due to photography. This fact taken in connection with the other, that a large proportion of the new stars have appeared within recent times, makes it evident that these phenomena are not of such great rarity in nature as was formerly supposed.

Unfortunately our only record of the spectrum of Nova Normæ consists of a single plate. This was taken in the Bache telescope at Arequipa with small dispersion, the total distance from F to H being but a few millimeters. With such a dispersion it is impossible to determine much more than the general characteristics of the spectrum. A comparison has been made with the spectrum of Nova Aurigæ taken under similar circumstances (see ASTRONOMY AND ASTRO-PHYSICS for January, Plate IV). It is found that each prominent bright line in the Nova Aurigæ has a corresponding bright line in Nova Normæ, and an examination of the negative shows that each bright hydrogen line has an accompanying dark one by its side, in both spectra. In both cases the dark line is toward the more refrangible end of the spectrum. It is impossible to determine the relative velocities of the bodies producing the bright and dark lines from these spectra, but it is evident that they are not very different in the two cases.

Turning now to theoretical considerations, the relative velocity of the components of Nova Aurigæ, although high, cannot be considered as very remarkable, being less than three times as great as is frequently found in that comparatively quiescent body, the Sun. Moreover it is less than twice as great as the velocity which would be produced by a body falling from an infinite distance and just grazing the solar surface. But the high velocity in the case of the Nova had evidently no connection with the mutual attraction of the bodies, since it persisted with but slight diminution for several weeks, the two bodies, according to the generally accepted theory, traversing in the meantime a dis-

* Communicated by the author

tance equal to several thousand times the diameter of the Sun. The very fact that so little change of velocity was detected, would indicate that the combined mass of the bodies must have been comparatively small.

The high relative velocity of the components must then have been due either to a very high initial velocity in both bodies, or to an outburst of some sort, which produced a velocity not much higher than we ordinarily find in the Sun. Let us compare now these two hypotheses. The former is the one almost universally adopted, but the latter I think should not be discarded without some further consideration.

On the hypothesis of high initial velocities, it is generally considered that the two bodies are moving nearly in the line of sight, and that Nova Aurigæ for this reason furnished unusually favorable conditions for our investigation of its nature. But apparently Nova Normæ furnished equally favorable conditions, although unfortunately little advantage was taken of them, owing to our not knowing in time of its existence. The probability is about one in one hundred and twenty that in both instances the line of relative motion should be inclined less than thirty degrees to the line of sight, and that the star giving bright lines should in both cases be going the same way. If we merely require that the line of motion should lie within sixty degrees of the line of sight, the probability that the two spectra should be similar is increased to one in eight, but we must admit a maximum actual velocity of the two stars twice as great as that which has been observed by the spectroscope. Moreover, bright line stars are rather rare, and even if we assume that the bright lines were produced by the collision, it is rather singular that in both cases one star only should be so affected. The probabilities, as far as they go, are therefore unfavorable to the correctness of this hypothesis.

Again, how are we to account for the successive reappearances of Nova Aurigæ? Since the relative motion is not due to gravitation, the two bodies must still be receding from one another. It is not probable that each reappearance should be due to a fresh collision with a new and previously unknown body.

It is much easier to attack a hypothesis than to frame a new one, but let us imagine a rather dark Sun, with a viscous surface and gaseous interior. After many centuries of quiescence and contraction a series of eruptive prominences upon an enormous scale burst forth, spreading in every direction, and completely enveloping the star upon all sides. When they first appear, they

present a spectrum of bright lines, but in a few hours the gases first emitted have receded to a considerable distance from the star, and have cooled down owing to the rapid expansion involved by their recession, which causes them to fill a very much larger sphere than that originally occupied by them. Thus the star very soon becomes enveloped in an atmosphere whose outer regions are comparatively cool. The velocity of the emitted gases is so great, however, that they do not fall back at once, as in the case of the Sun, but continue to recede from the star in all directions. In the case of Nova Aurigæ this motion was at the rate of of about 25,000,000 miles a day.

This cold advancing atmosphere produces a series of dark absorption lines. It at the same time cuts off the light from the hot advancing prominences behind it, although a faint reversal of the dark lines in Nova Aurigæ was for a time detected. The hot receding prominences, however, extending away for perhaps millions of miles behind the limb of the star, give out a light whose wave-length cannot be absorbed by the cold advancing atmosphere. They therefore shine with their full brilliancy. The phenomenon may last for several weeks or months until the internal pressure is relieved.

Assuming the maximum total relative velocity indicated by the spectroscope to be 800 miles, the velocity involved in the irruptions upon this hypothesis does not exceed 400 miles per second. A solar irruption is mentioned by Young whose velocity equalled 300 miles per second, and prominences are mentioned by him whose altitude reached 400,000 miles. The reversal of the bright lines in the spectrum of the *Nova* would seem to indicate that the cooled gases were not receding from it indefinitely, but that a downrush of the outer layers had already occurred.

If the bright and dark lines of a *Nova* owe their origin to a series of eruptive prominences in a star, we should expect that the lines would be somewhat irregular in structure, and that these irregularities would vary from night to night. In the case of the dark lines this would be due largely to the existence of bright lines behind them, which they more or less completely concealed. This effect is readily noticeable in the photographic spectra, and is described also by Dr. Vogel (ASTRONOMY AND ASTRO-PHYSICS, 1893, p. 901).

If the light of the *Nova* was due chiefly to the presence of prominences, we should expect it to fluctuate appreciably from night to night. If the light was due chiefly to heat generated by collision, it would be more uniform. The *Nova* first appeared, as

shown by the Harvard photographs combined with one of Dr. Wolf's, about December 10, 1891. Ten days later it had doubled in brightness, and it continued to fluctuate, becoming alternately brighter and fainter for the next two months.

If we consider a bright line variable star to be one so far cooled, as to emit very large prominences only at stated periods, as does our Sun for instance, we may consider a *Nova* to be one still further cooled, emitting them perhaps for the last time, but with such intensity that the included gases recede in all directions, forming an atmosphere of extreme tenuity, but whose depth may possibly be measured by hundreds of millions of miles. Such a body, when it had reached a state of equilibrium would resemble planetary nebula in appearance, and probably in its spectrum also.

HARVARD COLLEGE OBSERVATORY,
January 25, 1894.

NOTE ON THE SPECTRA OF COMETS.*

H. KAYSER

The origin of the bands usually seen in the spectra of comets is not yet sufficiently explained, although numerous researches have been made on this subject. The principal reason for this fact is the want of light in the spectrum which, forcing the observer to use a very wide slit, produces several inconveniences: 1. All fine details of the structure of the bands vanish. 2. The maximum of light is displaced from the apparent edge of the bands towards their middle. 3. The edge is shifted, and 4. Exact measurement of the wave-length is impossible.

By means of photography Campbell (ASTRONOMY AND ASTRO-PHYSICS, Vol. 12, p. 652, 1893) has lately been able to measure wave-lengths with a precision hitherto unattained, and it is therefore interesting to compare his results with those obtained by laboratory observation of carbon bands. Campbell himself has made this comparison between his measurements and those of Professor Runge and myself, but as it seems to me, he has committed some mistakes. Our measurements (*Abhandl. b. Berlin. Acad.*, 1889) refer to the arc, which shows two series of band-groups. The first series contains bands attributed at present, by nearly all spectroscopists, to carbon itself and not to hydro-

* Communicated by the author.

carbon, since Liveing and Dewar have adopted this opinion after having opposed it for a long time. The edges have the following wave-lengths:

First Group (According to Angström and Thalén)	Second Group	Third Group	Fourth Group.	Fifth Group.
619–595	5635	5165	4737	4382
	[illegible]	5129	4715	4371
	5541	5098 (according to Angström & Thalén)	4698	4365
			4685	
			4677 (Watts)	

The second series belongs very probably to a compound of C and N, most probably to cyanogen. The edges of these band-groups have the following wave-lengths.

First Group.	Second Group	Third Group.	Fourth Group.	Fifth Group.
4606	4216	3884	3590	3370
4578	4197	3872	3586	3350
4553	4181	3862	3585	(According to Liveing and Dewar.)
4532	4168	3855		
4515	4158			
4502	4153			

Campbell has doubtless found several groups of these two series. With the eye were observed, the first C group at 601; two edges of the second C group at 563 and 558; three edges of the third C group at 5162, 5129, 509; by photography, five edges of the fourth C group, 4736, 4716, 4697, 4683, 4675. The fifth C group is very much weaker than the others and it is therefore not remarkable that it has been overlooked. Campbell has observed a very bright line at 4366, but this cannot represent the fifth group, as it coincides with the third or weakest edge of the group. He has observed the following of the cyanogen bands; the first three edges of the second group, 4214, 4196, 4178; the first two edges of the third group, 3881, 3870. The fourth and fifth groups are not within the scope of Campbell's observations, but he might have been able to observe the first group. The reason that this has not been the case is perhaps to be found in the fact that this group is much weaker than the other cyanogen groups and that it is covered by the end of the fourth C group which is much stronger and therefore may very easily hide the first Cy group, this is also the case in the arc spectrum.

It can therefore safely be said that the bands of carbon and cyanogen occurring in the arc spectrum have been found in the spectrum of comets. But Campbell has measured still other lines, especially one group situated between 4366 and 4235, which we have not found in the arc spectrum. Campbell supposes that this part of the spectrum has not been covered by our work, but here he is mistaken; there are no strong lines in this part of the

arc spectrum, but only the extremly faint ends of the fifth C group and of the second Cy group. Campbell further speaks of a series of very bright lines between 4098 and 4017 which are also wanting in the arc spectrum. That these two groups of lines or bands belong neither to the carbon bands nor to the cyanogen bands is proved by the regular distribution of the latter, into which they do not fit (Deslandres, C. R. 112, p. 661, 1891). Hasselberg and H. C. Vogel believe that they have found bands of CO in the spectrum of comets; but these unexplained bands do not belong to CO, nor has Campbell found any CO bands. There are, however, some bands in the spectrum of burning hydro-carbon which have often been drawn and measured and have been photographed by H. W. Vogel, and Eder. C. P. Smyth draws, in the spectrum of the acetylene flame (*Edinburgh Astron. Observations*, Vol. 13, 1871), one broad, faint band at 4363 and another strong, narrow one at 4306; Lecoq gives 4368 and 4309, Angström and Thalén for the second 4311, Watts 4313. According to Eder (*Denkschr. d. Mathem.-Naturw. Classe d. Wiener Acad.*, 57, 1890) the first band begins at 4359, the second at 4315. The bands are most distinctly seen on the beautiful photograph of the spectrum of the Bunsen flame by H. W. Vogel (*Sitzungsber. d. Berliner Acad.*, 1888). Campbell gives two of his lines as very bright at 4366 and 4313; I have no doubt that he has here seen the two bands of burning hydro-carbon. It seems worth mentioning in this connection, that the fifth group of the C bands is no more visible in the flame-spectrum photograph than in the spectrum of comets.

Campbell's first group of lines seems thus sufficiently explained, but I do not know of an explanation for the second group between 4098 and 4017. Campbell erroneously compares the first three of these lines with lines measured by us in the second group of Cy, which are much too weak to have been seen in the comet. Some very strong lines appear on Vogel's photograph at 403, which seem to form the edges of a band shaded on the less refrangible side, but it is improbable that they have anything to do with Campbell's lines. This part of the flame spectrum, however, has as yet not been thoroughly examined. Huggins has also observed some maxima of light in the comet *b* 1881 between 410 and 400 (*Chem. News*, 44, p. 183).

The principal result of this comparison is that in the spectrum of comets there are not only visible the bands of carbon and cyanogen which appear alone when vapor of carbon is made luminous by electricity, in the presence of nitrogen, but that some

other bands also appear, the chemical origin of which we do not know, but which it has been proved are seen in the spectrum of burning hydrocarbon. The spectrum of comets, therefore, more resembles that of a *burning* compound of carbon than that of a compound of carbon made incandescent by electricity.

A STUDY OF DIFFRCTION GRATINGS.—FOCAL ANOMALIES.

A. CORNU.*

At the present time diffraction gratings furnish almost the only method for the accurate determination of wave-lengths of light. Although these instruments have been brought to great perfection in the hands of Rutherfurd and Rowland, as regards the definition of the lines in the spectra which they give, they still show occasionally various anomalies which perhaps cast some doubt upon the rigor of the optical principles upon which they are based. It will then be well to study these irregularities in a detailed manner, to determine their laws and causes, and the conditions necessary for estimating their effect upon the accuracy of measurements, to eliminate the errors which they introduce, and hence to attain greater perfection in the construction and use of diffraction gratings.

This somewhat thankless task has frequently claimed my attention since my first observations made a long while ago upon the focal properties of gratings.† I have been led to construct an engine which rules lines which are automatically spaced according to known laws, in such a way that the irregularities whose origin I desired to determine could be produced and increased at will. From the great multitude of practical difficulties which complicate the construction of gratings, I have endeavored to sort out the systematic causes of irregularity, and to separate from them all considerations of a purely geometrical nature. I shall later on have occasion to give a brief description of this engine, whose successive modifications have suggested to me several interesting results, which under different heads I shall ask permission of the Society to communicate successively. Almost all of these are kinematical or geometrical theorems from which has disappeared all trace of the long and laborious methods by which I arrived at them.

* Translated from *Journal de Physique* September, 1893.

† *Comptes Rendus*, Vol. LXXX, p. 645, 1875, *Association francaise*, *Congres de Nantes*, p. 376, *Revue Scientifique*, No. 12, Sept. 18, 1875.

Focal Anomalies.

Among the minute irregularities which occur in gratings perfect in definition and in most other respects we must include systematic errors in the position of the focus incompatible with the theory of a perfect grating. All my observations have led me to attribute these anomalies to two distinct and purely geometrical causes:

1st. In the case of *plane gratings*, to the presence of a slight curvature of the ruled surface.

2d. In the case of both *plane and curved* gratings, to the existence of a regular variation in the distance of the rulings.

Very often these two causes co-exist and thus make the law of the grating very complicated.

Anomalous Curvature of the Surface.—This curvature, which as a rule is approximately spherical, and usually convex, is accounted for by the difficulty of obtaining an absolutely plane surface. When the surface is uneven, the definition is imperfect, and the lines are wanting in sharpness. Whenever the ruled surface is of the nature of a surface of the second degree, having a plane of symmetry parallel to the rulings, the lines in the spectrum may be perfectly well defined. Even the inevitable astigmatism may be corrected by a method which I have indicated elsewhere.*

Hence in the present study of the focal properties of gratings, we may set aside the case of curvature of the surface in a plane parallel to the rulings, and consider only the curvature normal to them. This amounts to supposing the grating ruled on a cylindrical surface, of which the rulings are generating lines.

The ruled surface is then characterized simply by its radius of curvature, R, so that all problems connected with it, being reduced to cases of plane geometry, are much simplified.

Anomaly in the Distribution of Rulings. Characteristic Law.—Continuous inequality of spacing of the lines is explained by the difficulty of ruling them at exactly equidistant intervals. We may represent the distance, s, between the lines, beginning with a line which passes through the origin, by the formula†

$$s = bt + c^2t,$$

in which the variable t (representing for instance the number of turns or fractions of a turn of the screw of the dividing engine), may take the value 1, 2, 3, — , n. The disturbing term ct^2 is pos-

* *Ann. de Chim. et de Phys.*—6th Series, Vol. 7, p. 19.

† A term of small value in t^3 would introduce sensible aberrations into the formation of focal images. The gratings which we consider, however, are too nearly perfect for these to be appreciable.

itive, $(c > o)$, when the width of the spaces increases with t; and negative, $(c < o)$, in the contrary case.

Kinematical Interpretation of the Above Law. Characteristic Parameter.—This law of the increase of spacing of the lines may be interpreted by a diagram which takes into account the relation between the coefficients b and c.

Suppose the grating to have been ruled by means of a screw, turning with equal increments of angle δt, t increasing positively. If we have $c = 0$ the rulings will be equidistant, the step of the screw being constant, and the screw-thread will form a perfect helix, whose development upon a plane surface is a straight line. If $c > 0$ the rulings become more and more widely spaced, and for $c < 0$ they become more and more closely packed; the screw has then a variable step, which if the screw is long enough will end, in one direction or the other, according to the sign of c, by becoming zero when

$$\frac{ds}{dt} = 0.$$

This will take place at the distance given by

$$s_0 = -\frac{b^2}{4c},$$

which we shall designate later on by

$$-\frac{1}{2}\mathrm{P}.$$

Hence we easily conclude:—

When a grating shows a progressive variation in the spacing of its lines represented by the formula $s = bt + ct^2$, we may consider it as having been ruled by means of a screw, the development of whose thread upon a plane surface would be an arc of a parabola,* of which the axis is parallel to that of the screw. The distance of the apex of the parabola from the origin,

$$s_0 = -\frac{b^2}{4c},$$

is a characteristic parameter of the screw, and of all gratings ruled by means of it, for it is independent of the number of subdivisions of a revolution, that is, of the mean distance between the lines.

The Laws of Focal Anomalies.—We shall now prove the following important result:—

* The *parabolic helix* is made use of in the rifling of gun barrels.

The focal anomalies of the grating in the plane normal to the rulings are completely defined by two linear constants, the radius of curvature R, of the surface, and the parameter P, of the screw of the ruling engine. These two constants can be derived from optical and geometrical data of experiment by two very simple equations which may be found as follows:—

Let ρ, ρ' (Fig 1) be the respective distances from M, the center of the grating, of the source A, and focus A′ of the incident and diffracted pencils

α, α' the angles which the axes of these pencils make with the normal at the point of incidence

R, the radius of curvature of the section MS of the grating perpendicular to the rulings.

P, the characteristic parameter of the law of distribution of the rulings.

ε, the mean grating-space.

Consider a cylindrical wave emanating from a point A and meeting two consecutive lines on the grating at M and M′. The difference of path of the two rays will be

$$AM - AM', \quad \text{or} \quad \rho - (\rho + \delta\rho) = - \delta\rho.$$

We have also

$$- \delta\rho = \delta s \sin \alpha$$

$$\text{and} \quad \rho\delta\varepsilon = \delta s \cos \alpha$$

in which δs is the small grating-interval MM′ corresponding to the variation δt in the equation $s = bt - ct^2$, (where s is reckoned positive in the direction MS), and α is the angle CMA. Each of the rulings through M and M′ becomes a center of diffracted waves, and two such waves will reach any point A′ in the same phase, if the difference of path M′ P + M′ P′ is a positive or negative integral number of wave-lengths. In that case we have

$$\delta\rho + \delta\rho' = - m\lambda \quad \text{or} \quad \delta s (\sin \alpha + \sin \alpha') = m\lambda, \qquad (2)$$

a wave-length, as well as a grating-space, being considered infinitesimal.

If we consider a third line M″, (defined by a new constant increment δt of the variable t) as associated with the second line M′, the condition that the diffracted ray from M″ shall reach A′ in the same phase, is the same as before, but t, α and α' must be replaced by $t + \delta t$, $\alpha + \delta\alpha$, and $\alpha' + \delta\alpha'$. $m\lambda$ and δt remain constant. Hence, differentiating equation (2)

$$\delta^2 s\,(\sin\alpha + \sin\alpha') + \delta s\,(\cos\alpha\delta\alpha + \cos\alpha'\,\delta\alpha') = 0. \quad (3)$$

Denoting the infinitesimal angles, C, A and A′, by $\delta\omega$, $\delta\varepsilon$ and $\delta\varepsilon'$ we have

$$\begin{array}{lcl} \delta\alpha = \delta\omega - \delta\varepsilon & & \rho\ \delta\varepsilon = \delta s \cos\alpha \\ & \delta s = \mathrm{R}\,\delta\omega & \\ \delta\alpha' = \delta\omega - \delta\varepsilon' & & \rho'\,\delta\varepsilon' = \delta s\cos\alpha' \end{array} \Bigg\} \quad (4)$$

Equation (3) may now be written, on substituting for $\delta\alpha$ and $\delta\alpha'$, in terms of δs, and dividing by δt^2, in the form

$$\frac{\delta^2 s}{\delta t^2}(\sin\alpha + \sin\alpha') + \left(\frac{\delta s}{\delta t}\right)^2\left(\frac{\cos^2\alpha}{\rho} + \frac{\cos^2\alpha'}{\rho'} - \frac{\cos\alpha + \cos\alpha'}{\mathrm{R}}\right) = 0 \quad (5)$$

The quotients $\frac{\delta^2 s}{\delta t^2}$ and $\frac{\delta s}{\delta t}$ may be replaced by the derivations $\frac{d^2 s}{dt^2}$ and $\frac{ds}{dt}$. At the *middle-line* of the grating, which passes through the origin, and for which therefore $t = 0$, we have from the equation $s = bt + ct^2$,

$$\frac{d^2 s}{dt^2} = 2c, \qquad \left(\frac{ds}{dt}\right)^2 = b^2.$$

The quotient of these is $\mathrm{P} = \frac{b^2}{2c}$.

Equation (5) now takes the symmetrical form

$$\frac{\cos^2\alpha}{\rho} + \frac{\cos^2\alpha'}{\rho'} = \frac{\cos\alpha + \cos\alpha'}{\mathrm{R}} - \frac{\sin\alpha + \sin\alpha'}{\mathrm{P}} \quad (6)$$

Also since $e = b\delta t$, equation (2) may be written

$$e\,(\sin\alpha + \sin\alpha') = m\lambda,$$

e representing the *mean grating space*. Such are the laws of focal anomalies.

Discussion of these Formulæ. Conjugate Focal Curves.—Equation (6) gives the relation connecting the focal distance $\rho' = \mathrm{MA}'$ (Fig. 1) of a cylindrical wave of wave-length λ, diffracted in the spectrum of the mth order, with the distance of the source, $\rho = \mathrm{MA}$.

1. This equation being symmetrical with respect to ρ and α on the one hand, and ρ' and α' on the other, the points A and A′ are true conjugate foci; hence we may either regard A as source and A′ as focus, or the reverse.

2. For any position of the source, (ρ = const., α = const.), equation (6) leaves the position of the focus A′ indeterminate,

hence this equation represents the geometrical focus in polar coordinates (ρ', α') of all positions which the focus of the diffracted pencil conjugate with the source can occupy in the plane of diffraction. This locus is thus the *focal curve* corresponding to the given position of the source.

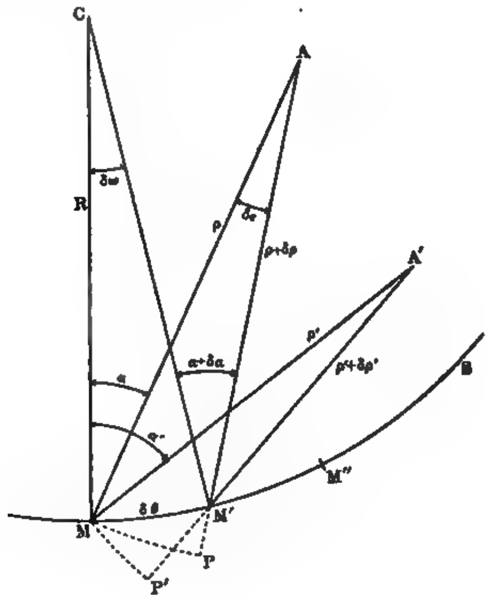


FIGURE 1.

3. The *focal curve* A′ does not in general pass through the source A; hence there is a *family of focal curves*, whose parameter [redacted]

zero, in which case the two conjugate curves coincide, and their common equation is

$$\frac{\cos^2\alpha}{\rho}-\frac{\cos\alpha}{R}+\frac{\sin\alpha}{P}=0. \tag{9}$$

This curve has thus the property of passing through all the foci and the source. It is *unique* for a given grating, and depends only upon the radius of curvature R and the parameter P. It is clearly independent of the mean grating-space.

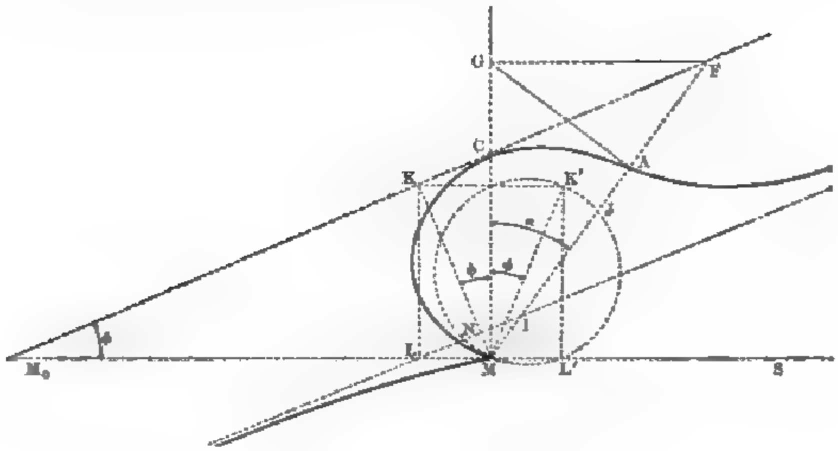


FIGURE 2.

I propose to call this the *principal focal curve.*

This curve takes various forms depending on the ratio between R and P. These forms are derivable from the type of the Cissoid of Diocles, and return into the type itself when $R=\infty$. Equation (9) may be put into the following forms:

$$\rho=\frac{\cos^2\alpha}{\frac{\cos\alpha}{R}-\frac{\sin\alpha}{P}}=\frac{PR}{H}\frac{\cos^2\alpha}{\cos(\alpha-\varphi)} \tag{10}$$

where

$$R=H\sin\varphi \qquad \text{and hence } \tan\varphi=\frac{R}{P}.$$

$$P=H\cos\varphi \qquad H^2=P^2+R^2$$

which leads to a simple geometrical construction (Fig. 2).

The above equation may be written in the form

$$\rho=R\cos\varphi\left(\frac{\sin^2\varphi}{\cos(\alpha+\varphi)}+\cos(\alpha-\varphi)\right) \tag{11}$$

which shows that the radius vector ρ is, like that of the cissoid, the sum of two others, namely of a straight line and of a circle. This leads to a second method of construction.

Any point A may be found from equation (10), by the aid of the straight line M_0C which joins the center of curvature C of the grating, and the point M_0, taken such that $MM_0 = P$. Let fall a perpendicular FG upon MG from the extremity of a radius vector of this line, and from G, a perpendicular GA upon the radius vector. A is then a point on the curve. The curve has as an asymptote the line LN parallel to MF when $\alpha = 90° - \varphi$, and distant from the origin by an amount $MN = R \cos \varphi \sin \varphi$, as may be seen by constructing the perpendiculars MK upon M_0C, KL upon M_0M and LN upon MK.

The second method of construction, derived from equation (11), consists of prolonging the radius vector MJ of the circle described on MK′ as diameter, by an amount equal to MI, the radius vector of the asymptotic line LN defined previously. The diameter of this circle is $MK' = R \cos \varphi$, and its equation is

$$\rho = R \cos \varphi \cos (\alpha - \varphi).$$

The point K′ is taken symmetrical with K about the line MC.

The figure corresponds to the case $c > 0$, $P > 0$. The rulings will then be more widely spaced toward the right, for $P = -2s_0$. (*Vide supra*).

This second definition of the principal focal curve leads to several immediate verifications by considering known results as special cases of the above.

Suppose the grating to become more and more nearly perfect as regards spacing, while the curvature remains unchanged. The point C will remain fixed while M_0 approaches infinity. In the limit the focal curve reduces to a circle [upon the diameter MC], and this is the circle employed by Rowland in his admirable concave gratings.

If in the concave grating there is still a minute systematic error of ruling, the angle φ is not absolutely zero, and the principal focal curve is sensibly a circle whose diameter through M makes the very small angle φ with the normal to the grating. This is the result recently obtained empirically by Rydberg (*Academie de Stockholm*, t. XVIII., No. 9).*

Finally, in the reverse case, in which the grating is sensibly plane, while the spacing shows a marked systematic variation, the point C is at infinity and φ is a right angle. The principal

* [Also, *Phil. Mag.*, March, 1893].

focal curve is then a cissoid, having an asymptote which passes through M, and is normal to the grating surface. We thus find the arrangement of the foci of the spectra which I pointed out in my first researches.

I confine myself at present to these purely geometrical results, reserving for a future article the description of experimental methods which furnish numerical verifications of these laws.

IS THERE OXYGEN IN THE ATMOSPHERE OF THE SUN?*

N. C. DUNER

This question has been much discussed, its solution presents difficulties arising, perhaps, from the inadequacy of the methods which have been employed for studying the spectra of gases.

The ordinary method consists in enclosing the gas in tubes, and causing it to be traversed by an induction spark, with or without a condenser. There is, however, no proof that the spectra thus obtained are identical with those which are seen in examining a celestial body whose atmosphere contains the same gases under different conditions of pressure and incandescence.

It is true that the spectrum of hydrogen obtained with spectrum tubes is identical with that given by the chromosphere; but it is known, on the one hand, that there is another spectrum of hydrogen which has not been found in any celestial body, and on the other hand, that hydrogen differs essentially from other gases, such as oxygen, nitrogen and chlorine, and is to be regarded rather as a gaseous metal than as a metalloid. It is known, moreover, that with the exception of carbon, no certain spectroscopic proof has been found of the presence of a single metalloid in the stars, or in comets, or in nebulæ. But is it possible to believe that all the celestial bodies, including the Sun, are deprived of all metalloids, while oxygen, in the form of water vapor, has been proved to exist in the atmospheres of several of the planets?

Let us now pass to a consideration of the spectral characteristics of oxygen. There are four different spectra of emission which are believed to belong to oxygen, namely; two line spectra, one band spectrum, and a continuous spectrum. In the spectrum of the Sun neither the line nor the band spectrum has been seen, and as for the continuous spectrum, it would evidently be impossible to prove that it exists. M. Egoroff has demonstrated that in ad-

* Translated from the *Comptes Rendus*, cxvii, No. 26, Dec. 26, 1893.

dition to these there is an absorption spectrum of oxygen, and that it is represented in the solar spectrum by the two strong bands A and B. M Cornu having subsequently proved that, setting aside some relatively faint lines produced by metals in the Sun's atmosphere, there is a complete identity between the band *a* and the bands B and A, the conclusion may be drawn that this band is in all probability also a band of oxygen.

Sir D. Brewster has already proved that all these bands are *telluric bands*, that is to say, that the gases which produce them are found, at least in a large measure, in the atmosphere of the Earth. But while it is easy to prove that the bands due to water vapor are exclusively telluric, since they entirely disappear during excessively cold weather, it is otherwise in the case of the bands of oxygen. The latter are no doubt weaker when the Sun is high, or when they are observed at stations greatly elevated above the Earth's surface, but still they are always visible, at least in their stronger parts. It may consequently be supposed, and in fact it has been supposed, that oxygen in the solar atmosphere may have contributed to their formation. I believe, however, that I can demonstrate that these bands are purely telluric.

Let us suppose that the bands A, B and *a* are partly telluric and partly solar. There would then be a particular place on the surface of the Sun,—namely, that place which is neither approaching nor receding from the point of observation by the combined action of the rotary and orbital motions of the Earth and Sun,—where the two parts, solar and terrestrial, of a line belonging to one of the bands, would have the same wave-length. At other parts of the surface, however, the wave-length of the solar part would either exceed or fall short of that of the telluric part, and it is easy to see that in the neighborhood of the solar equator the separation of the parts would become visible in a spectroscope of sufficient power, so that the line would appear double, having two components differing more or less in intensity.

Now in my researches on the rotation of the Sun, I have examined the lines in the band *a* hundreds of times. At points on the east limb in the vicinity of the solar equator, the wave-length of a given line differs from that of the same line on the opposite limb by 0.12 revolutions of the micrometer of my spectroscope,—a quantity large enough to sensibly alter the configuration of the small group of lines to which my observations were ordinarily directed. Consequently if the lines had not been exclusively terrestrial, but partly of solar origin, the two different parts would have appeared separated by so large a space that the duplicity

of the line could not have escaped even an inattentive observer, and even if the intensity of the components were quite different.

There is another series of observations which affords an ample confirmation of my own, namely, the researches of M. Cornu on the groups A, B and a. In these observations M. Cornu recognized the different nature of the lines belonging to these groups by the *oscillations* of the solar rays when the slit of the spectroscope was made to pass rapidly across the image of the Sun. In so oscillating, the oxygen lines should have appeared double if they have a double origin, solar and terrestrial, and this appearance could not have escaped the practiced eye of M. Cornu. On the whole, I do not hesitate to maintain that *the bands* A, B *and* a *have a purely telluric origin*.

Is there then no oxygen in the Sun? I have already, in what precedes, expressed my opinion on this subject. But how is it that none of the spectral characteristics of oxygen are to be seen? The explanation is not easy, and different answers have been given. It has been supposed that the metalloids are dissociated by the extremely elevated temperature of the Sun, which is explaining an incomprehensible phenomenon by a hypothesis scarcely more intelligible. The fact can be invoked, as it was by M. Scheiner, that in spectrum tubes the incandescence of metalloids ceases in the presence of metallic vapors; but I have already observed that one can hardly be certain that the phenomena observed in spectrum tubes are applicable to the celestial bodies; moreover, in the first case the incandescence is produced by the electric spark, while in the second it is the result of heat only. It may well be supposed, when we bear in mind the diversity of the spectra of the metalloids, that in the particular conditions of the solar atmosphere the spectrum of oxygen is of such a nature as to preclude detection. And finally we may suppose, with M. Egon de Oppolzer, what is perhaps not the least reasonable condition, that in the absorbing layers of the Sun, oxygen exists in such a state of rarefaction that it is incapable of producing any sensible absorption.

But these are merely so many purely hypothetical speculations, which it is useless to dwell upon here.

A SHORT REVIEW OF MY THEORY OF THE SUN.*

A. BRESTER Jr.

§ 1.

Explanation of the Automatic Check Which Prevents Any Considerable Motion in the Interior of the Sun's Mass.

The interior tranquility of the Sun is not to be thought of merely as the "conditio sine qua non" of its permanent stratification, but as *a priori* much more probable than the ordinary theory of violent agitation. Every one knows that the single cause of terrestrial cyclones lies in the disturbing action of the Sun. Since an analogous cause does not exist on the exterior of the Sun its atmosphere will there be just as calm as would ours, if the Sun did not exist. "But," someone will ask "is it not the unequal cooling that causes these supposed disturbances?" To this I reply that the incandescent and dissociated gaseous state of the Sun's mass makes all unequal or sudden cooling impossible.

The solar gas in fact is so hot that it must contain very many separated molecules always ready to reunite whenever, by continual loss of heat, their temperature is sufficiently lowered. In this manner, they hinder, by the heat produced in their union, any appreciable fall in temperature. Just as steam, in losing heat, cannot be cooled down below 100° until all its mass is converted into water, so the incandescent solar vapor cannot be lowered in temperature so long as any of its material capable of being condensed at that temperature is not yet so condensed.

Now if the incandescent solar gas has not already reached this final state, all cooling there is impossible; and *a fortiori*, all unequal cooling. A quiet interior should, therefore, not surprise us. The molecules always ready for combination at the least loss of heat come under two heads:

(1.) There are those vapors cooled down to their dew-point which are found throughout the solar atmosphere and in the photospheric layers, and bear visible witness to their saturation by the fog of condensed particles with which they fill more or less the exterior layers of the Sun. In a mist uniformly saturated the temperature will always remain stationary, however great the quantity of heat there absorbed or developed.

* Read at the Congress of Astronomy and Astro-Physics, Chicago, August, 1893. Continued from December number.

(2.) There are also molecules of dissociated matter cooled to a point where their re-combination becomes possible. These molecules coöperate with those of saturated vapors to maintain a constant temperature; because, in continually combining in proportion to the loss of heat through radiation, the new heat thus produced will counteract the cooling, and will not, on the other hand, produce any increase of temperature, since such an increase would be immediately absorbed, not only by the evaporation of the mist already mentioned, but also by the chemical dissociation of those same molecules which in combining were the source of the new heat.

If, in general, solar phenomena teach us that the Sun is in a state of repose and that this quiet is such that the Sun, in spite of its gaseous state, presents the appearance of a solid, made up of superposed layers, which do not vary, and of parallel zones, always the same; and if here periodic phenomena are regularly repeated; then, I say, this quietude agrees very well with the known properties of matter. Because we see in the physical and chemical condensations by means of which the Sun renews its lost heat without cessation, an automatic check hindering those motions which, without it, cooling would certainly produce.

§ 2.

Explanation of the Intermittent Outbreaks of Heat Which this Check is to Prevent.

If all the vaporized and dissociated molecules required but an infinitesimal lowering of temperature to condense or to combine all at once, there would be perfect tranquility as a result; but none of the characteristic phenomena now observed would follow therefrom. Among all these segregated molecules, there would necessarily be some, which, not being ready to combine at the least reduction in temperature, would unite in an intermittent fashion and would, in this manner, cause the periodic production of *vis viva* which I have named "heat eruptions," (*eruptions de chaleur*). Such would be the separated atoms, A and B, whose association into the molecule AB is not hindered by too high a temperature but by too great a number of other intervening molecules. It is clear, of course, that the innumerable molecules, R, interposed between the molecules, A and B, would act as an obstacle to their union. Thus it was, for instance, that in cooling a mixture, A + B + R, of dissociated water, A + B, and of carbonic

acid gas, R. Deville* could collect an explosive gas, that is, of molecules, A + B, which, according to other experiments of Deville, would surely have been recombined had they not been prevented by the presence of the molecules, R, of carbonic acid. If, therefore, in some solar stratum, similar combinations, A + B + C, are being produced by a constant loss of heat, the molecules, A + B, although ready for association, can not immediately combine. The molecules R, would then operate to oppose this by a state of aggregation corresponding to the temperature of the stratum in question. Now as such a radiation would diminish very rapidly, the number of the molecules R (especially if their state of aggregation is that of a mist-like condensation of a saturated vapor), the remaining molecules, R, would finally become too few in number to hinder the union of A + B. Then all at once and so far as possible these molecules would unite. They would produce a sort of *heat eruption* But the heat then produced, while opposing further cooling, could never cause, no matter how great it might be, the least rise in temperature, since the heat would be at once absorbed by the vaporization or the dissociation which it will again cause in the molecules, R, recently condensed. And this work ended, the temperature would never be higher than before, since an increase of heat would cause an increase of dissociation, while, the only source of heat is the formation of new molecules AB.

At the termination of this outburst of heat we find again the molecules R, which that heat had separated, in the same state as at first—that is—in the state in which by their presence, they prevented the combination of the remaining molecules A + B; and, consequently, by their condensation preserved the solar heat and in this manner, made way for a new outburst again to disunite them; and so on.

It is clear, moreover, that a heat eruption will never be produced in all points of the same spheroidal layer at the same time. For at different points of the stratum there would be

(1) Some slight inequality in chemical composition and

(2) A much more important inequality in the emissive and reflective power of neighboring layers more or less misty in their composition—an inequality which will temporarily cause a loss of heat peculiar to each part of the layer. Now as it is only this loss which, in connection with the chemical composition, determines

* E. H. Sainte-Claire Deville; *Lecons sur la dissociation propessees devant la Societe chimique*, 1864, Debray, *Dict. de chimie par A. Wurtz. Art. Dissociation*, p. 1174.

an *eruption de chaleur*, each outburst will, in consequence, have an extent, a duration and a motion entirely its own.

Such is the explanation of the manner in which the chemical energy of the dissociated elements can oppose, in an intermittent manner, the cooling of the Sun, and of how it can produce in the exterior layers, periodic heat-eruptions which are repeated at sensibly equal periods, while the recombination constantly going on in the dissociated strata prevents any obvious change in their chemical composition."

These heat-eruptions which, while maintaining the temperature constant, prevent all interior movement in the solar mass, would take place perhaps unperceived if they were not characterized by sharply defined luminous phenomena which are shown to be exactly such as are required by theory. It is clear that where a heat eruption is forming there will most often be cloud-like or misty matter, R, which, being highly incandescent, will emit a white light with a continuous spectrum, and that where such an eruption is already formed, on the contrary, that white light preexisting will disappear again. But the disappearance of that white light will not be the only luminous effect of a heat eruption; for that eruption being caused by a chemical combination of the gaseous molecules A and B, will produce a spectrum of bright lines, that is, a "chemical luminescence" like that which one observes in our laboratory flames, where, according to recent work of a large number of physicists,† it is independent of the temperature, the principal, if not the only cause of bright line spectra. Such are the intermittent luminous phenomena which my theory of a gaseous, tranquil globe with periodic heat eruptions leads us to expect, and such are also the intermittent luminous phenomena which the Sun actually presents.‡

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* We have in these outbursts of heat which occur in the Sun (1) *the condensation of vaporized matter*, and (2) *the chemical combination of dissociated matter*. These two act as intermittent sources of heat, each, so to speak, repairing the damage which the other has done. But there is in the Sun still another source of heat, *viz.* (3) *the contraction of the mass of the Sun*. Possibly, in my theory, sufficient account has not yet been taken of this third source for, as we shall see later, it exerts great influence upon the intermittent character of the *eruptions de chaleur*.

† E. Wiedemann, *Pogg. Ann.* 37, pp. 177-218; R. v. Helmholtz, *Die Licht und Wärmestrahlung verbrennender Gase*, Berlin, 1890; W. H. Julius, *Die Licht und Wärmestrahlung verbrannter Gase*, Berlin, 1890; Langley and Very, *Amer. Jour. Sci.* 1890, Vol. 40, pp. 97-113; G. Wiedemann, *Lehre von Elec.* IV, p. 526; W. Siemens, *Wied. Ann.* 18, p. 311; E. Pringsheim, *Wied. Ann.* 45 (1892), p. 428; *Nature*, June 8th, 1893.

‡ The manner in which solar phenomena accord with the predictions of my theory is all the more remarkable since in developing my theory I never had the Sun especially in view, but rather, the explanation of *variable red stars*. This explanation was published in 1888 and may be briefly summed up as follows.

§ 3.

Explanation of the Peculiar Rotation of the Photospheric Layer and of some Local Disturbances which that Rotation should Produce.

Before studying the intermittent luminous phenomena which the Sun exhibits, we must first say a word or two concerning its rotation, and the configuration of its gaseous portion.

The cloudy photospheric layer of the Sun shows no sensible flattening, the solar gas itself nevertheless has an appreciable equatorial enlargement which has often been observed and photographically established.* The independence of these two configurations has, however, but little to surprise us. For the matter which gives to the photosphere its dazzling whiteness, not being gaseous and having, in consequence, a specific gravity in general quite different from that of the gaseous stratum in which its condensation took place, would naturally be repelled from that layer towards new ones, where, preserving its initial speed and changing its angular velocity, in consequence it would come to rest at a surface where various circumstances (especially the specific gravity of the medium), had given it a stable position in the photosphere.

The independence of the photosphere, as regards the stratified and flattened gaseous layers, being explained in this way, it is clear that the photosphere, receiving at various latitudes the condensed matter from upper strata, will in consequence rotate the more quickly the nearer one approaches the equator. It is clear, I say, that the photosphere should have at different latitudes not only different angular velocities, diminishing toward the poles, but also a chemical composition which (in the photosphere as well as in the atmosphere which covers it) will be the same only in two parallels equidistant from the equator.†

In variable red stars, the intermittent condensation of molecules R, produces obscuring clouds at intervals, upon the exterior of the star.

The thicker these clouds become the more they shut off the light from the interior of the star, thus preparing the way for a new maximum. For as soon as the star reaches its minimum, the condensation of the molecules R will have attained a certain limit. The molecules A and B will now begin to reunite, thus producing an "*eruption de chaleur,*" which, in turn, will vaporize the clouds of the minimum, and will restore the maximum by opening to view the constantly brilliant central portion of the star.

* Secchi, *le Soleil*, I, p. 340, 390, II, p. 482, etc.; Young, *The Sun*, p. 173; Lockyer, *Chem. of Sun*, p. 424, *Solar Physics*, p. 49; Holden, *Himmel und Erde*, I, p. 443.

† If this explanation of the equatorial acceleration is correct, the coronal atmosphere should take part in the rotation of the Sun. This prediction has been confirmed by the observations of Deslandres during the solar eclipse of 16th of April, 1893. In two equatorial regions of the corona, 180° apart, he found the relative velocities in the line of sight to be from 5 to 7 kilometers per second.

Deslandres, *Compt. Rend.*, May 15, 1893, and *Nature*, May 25, 1893.

The main part of the gaseous mass of the Sun has not only a shape different from that of the photosphere, but it rotates at a different rate. *For while the main part of the Sun's mass turns on its axis as a whole, it is only in the cloud-like photosphere that there exist those variable angular velocities discovered by Carrington.* Here is a prediction which the observations of Mr. Crew have clearly confirmed. He has proved by means of the displacement of the Fraunhofer lines that the rate of rotation of the absorbent solar gas is the same in all latitudes, the period being approximately 26.23 days. Wilsing has found likewise that the faculæ rotates with uniform angular velocity at all latitudes. Since the uniformity of that value seems to prove that it is only at the photospheric level that there exist these anomalous angular velocities, it is remarkable that the velocity determined by Wilsing is greater than that obtained by Crew. Very recently, however, in repeating the researches of Wilsing, Belopolsky has found a smaller value exactly equal to that of Crew.*

Granting that the main part of the Sun rotates as a solid in about 26 days, the question now comes up of determining the different angular velocities in the different zones of the photosphere. These different velocities are not to be identified with those of spots, for all astronomers agree that, from time to time during the whole life of a spot, and especially when it is changing its form, it makes, now and then, those sharp advances, leaps, as it were,† and these incidental accelerations are not due to a temporary increase of velocity of the photosphere, which carries them with it. For a group of spots never shows a common acceleration, but each preserves its individuality.‡ If we can grant that as long as the spots do not change form, they have the same velocity as the photospheric clouds, then, whenever they do change form, they rotate more rapidly than the photosphere. And as the law of Carrington and Spoerer is deduced only from the *mean* motion of spots, *the velocity of spots, such as results from that law, is greater than that of the photospheric clouds.*

If, consequently, at a latitude of 21°, the spots rotate in 26 days just as the main part of the gaseous mass of the Sun, the photospheric clouds will there rotate less rapidly, and, in order

* H. Crew: *Amer. Jour Sci.*, Sept., 1889, *ibid.* Vol. 35, p. 159. Wilsing: *Publ. Astrophysik Obser. Potsdam* 1888. *Astr. Nach.* No. 3153. Belopolsky: *Astr. Nach.* No. 3158. According to Wilsing the result of Belopolsky is 0°.56 smaller than he himself gave it. Introducing this correction, one obtains for the daily rotation 13°.71, which is practically identical with value obtained by Crew.

† Young: *The Sun*, p. 110. Secchi: *le Soleil*, I, p. 141.

‡ Spoerer: *Sonnen flecken beob. im J. 1880-84*, p. 422.

to see these clouds rotating with the velocity of the body of the Sun, they must be observed at a lower latitude.

My theory requires that this lower latitude lies between the parallels of 10° and 15°, that is, in the zone where the extended observations of Carrington and Spörer* have shown that spots are produced most abundantly. Now this is a requirement which I think in accord with the detailed observations of Dunér.

Dunér sought, as did Crew, to determine the angular velocity of the absorbing layer of the Sun by means of the displacement of the Fraunhofer lines. But he obtained quite another result. For, in place of finding an angular velocity everywhere the same, he discovered on the contrary, that the velocity diminished like that of the spots, as the latitude increased.† At first sight, the observations of Dunér and Crew seem to contradict each other, but, upon a closer examination, it is not difficult to explain their apparent contradiction. "All the Fraunhofer lines,"—to quote Young,‡ "are not due entirely or even principally to the gaseous layer situated above the upper level of the photosphere. . . . The principal absorption probably takes place in the interstices between the photospheric clouds, and below the general level of their upper limit." Now it is evident that in the interstices of these clouds, the absorbing gas will have acquired the velocity of the clouds themselves and consequently, the Fraunhofer lines produced by it will indicate velocities diminishing with latitude. *Only those lines which originate above the general level of the photosphere can show the real motion of the solar atmosphere.* Now those were just the lines which Crew measured. For no less than seven of them are found in the catalogue of 273 lines which Young on Mt. Sherman saw reversed on the exterior of the photosphere; while this catalogue does not contain either of the two lines 6301.72 and 6302.72 which were the only ones used by Dunér.

There is then no difficulty in admitting that the velocities obtained by Dunér are not those of the solar atmosphere, properly so-called, but those of the photospheric layer. Their numerical values correspond completely with the requirements of my theory. For they show (1), that the photospheric strata everywhere rotate more slowly than the spots, and (2), that at the lat-

* Spoerer: *loc. cit.* p. 198; Carrington: *Redhill Obs.*; Secchi: *le Soleil*, I, p. 136, Tab. B. Young: *Elements of Astronomy*, p. 129. One must not confound the latitude of 11°, where spots appear most frequently with the mean latitude of spots at maximum, *viz.*, about 16°.

† Duner: *Astr. Nach.*, No. 2968, p. 270. *Sur la rotation du Soleil.*

‡ Young: *The Sun*, p. 63; Scheiner: *Spectralanalyse der Gestirne*, p. 194, 198, etc.

itude of 11° is found the only parallel where the angular velocity of the photospheric clouds is exactly equal to that of the atmosphere which covers them. At higher latitudes, the photospheric clouds rotate more slowly than the upper atmosphere; at lower latitudes they rotate faster.

As this difference in velocity between the photospheric clouds and the atmosphere covering them should cause in the superficial layers of the clouds either an acceleration or retardation, it would be only at the eleventh parallel where the superficial layers would not be seen slipping over the deeper layers like a veil.

In all other parallels these veils will be formed, and they will slip the faster the more they recede from the eleventh parallel towards the poles or the equator. Consequently, if the spots are holes in the photosphere, these holes will present openings which are larger, the nearer they are to the eleventh parallel. At latitudes quite different from 11° their orifices will be rapidly closed as it were by a sliding cover. And at latitudes of 35° or more this screen will be already there even before the spots have had time to develop. I have in fact calculated that while this screen at 15° requires not less than 19 days to traverse the length of a large spot (measuring one geocentric minute), the screen at 30° will cover such a spot in only four days. At 45°, the time required is only about two days. Now even in the "*zones royales*" (where my theory demands still other causes better adapted to the explanation of spots), any considerable spot requires several days or weeks* for development, while any spot occurring at higher latitudes will clearly remain invisible, or at most, will appear as one of those "veiled spots," those "*crateres sans tache*," or "rudiments of spots" which Secchi and Trouvelot have observed all over the surface of the Sun.†

From the preceding considerations, one is led to believe that, no matter how great may be the interior tranquility of the Sun, the anomalous rotation of the photospheric zones produces there nevertheless some agitation. But as that agitation (insensible at the two eleventh parallels) takes places without possible change of temperature, it remains localized in the photospheric level where it is produced, and can disturb the solar gas only in the immediate neighborhood of the photospheric layer.

Let us now see how in our tranquil Sun there are heat eruptions which hollow out the photospheric spots and there kindle the prominences and the coronal rays.

* Young: *The Sun*, p. 94; Secchi: *le Soleil*, I p. 60, Perry: Lockyer's *Chemistry of Sun*, p. 406.

† Trouvelot: *Amer. Jour. Sci.*, March, 1876; Secchi: *le Soleil*, I, p. 113, 58; Young: *General Astronomy*, p. 189.

§4.

Explanation of the Spots and Faculæ.

The spots are holes in the cloudy photospheric layer, channeled out in a place when the combination of previously dissociated molecules produces there an "eruption of heat." This heat will then cause a local vaporization of the cloudy condensed matter and will thus produce a hole, which will be seen as an obscure spot, not only because the cloudy matter, which was there the sole source of white light, will have disappeared, but also because this matter, without, however, changing in temperature, will then be transformed into a considerable mass of absorbing gas, which will prevent the light from the deeper and hotter layers from reaching us. This luminous absorption in the spot cavity will produce, however, only a relative obscurity. For the white brilliancy of the deeper layers is so intense that after the absorption above described it is still, even in the darkest part of the spots, more dazzling than the Drummond light.*

If such is the condition of the spots in our tranquil Sun, *their temperature must be equal to that of the adjacent photosphere.* I am well aware that MM. Henry, Secchi, Spörer and Langley have observed that the spots emit less heat than the other parts of the photosphere, but this by no means proves that they have a lower temperature. I see there merely the very simple effect of a smaller emissive power. And this emissive power must be smaller, for at a given temperature a gas emits less heat than a cloud of condensed particles. It is for the same reason that the flame of illuminating gas diminishes perceptibly in emissive power if it is converted into a Bunsen flame by introducing air. This flame, although increasing in temperature, diminishes very sensibly in radiating power, as may be perceived by the hand held at a short distance.† And if the spectra of spots show an increase in the intensity of several absorption lines, this does not demonstrate that the temperature has been lowered, for such an increase in intensity will be observed if, the temperature remaining unchanged, the absorbing gas is (as my theory requires) greatly increased in mass.

The spots are thus in the photosphere what pools of water are in a layer of melting ice. This water, though formed by heat, does not differ in temperature from the surrounding ice. In the

* Young: *The Sun*, p. 125; Leverrier: *Compt. rend.*, 8th Feb., 1869, p. 319.
† Tyndall: *Heat a Mode of Motion*, p. 498.

spots and the other parts of the photosphere there are thus only differences of latent heat, which consequently leave the masses in relative repose.

The photospheric matter, when vaporizing to form a spot, while not changing in temperature, increases very considerably in volume. It will thus cause a pressure, which while aiding to render the spot conical, on account of the less resistance of the upper level, must crowd back the surrounding clouds and will give rise to the increase of level and brightness which is ordinarily observed in the edge of a spot? For this reason aspot without faculæ is as rarely found as a terrestrial valley without neighboring hills. This increase of level will moreover, be more marked on the side where the spot is moved by the rotation of the Sun than on the opposite side; for the motion of the expanding gas being on the first-mentioned side contrary to the rotary motion, must produce there a stronger compression. Does not the observation of Warren de la Rue, that, in general, the spots which are seen on the eastern side of the solar disk seem larger than those which appear on the opposite side, find a simple explanation in this crowding of the eastern edge of the spot? We also know that the photographic negatives obtained at Kew by MM. de la Rue, B. Stewart and Loewy have rendered evident this greater developement of the faculæ on the following side of the spot, where they sometimes even form veritable tails of faculæ.*

If my theory of the spots easily explains the formation of the faculæ which surround them it also accounts for the faculæ which seem to precede them, and which, studied especially by Secchi, Spoerer and quite recently by Mr. Sidgreaves, may be considered as *precursory faculæ.*† Such faculæ will be formed (1) by the raising of the level, which must be caused in the upper photospheric layers by every formation of gas in the deeper layers; (2) by an increase in a given place of the condensation in the adjoining chromospheric and photospheric layers, a condensation which according to my theory is necessary to prepare for a future "eruption of heat." Such also seems to be the origin of the facular net-work which Mr. Hale has discovered.‡

* B. Stewart: *Proc. Roy. Soc.*, 13 p. 168; Secchi. *le Soleil* I. p. 165; De la Rue, B. Stewart and Lœwy: *Proc. Roy Soc.* 14 p. 39.

† Samter: *Himmel u. Erde*, I, p. 44; Sidgreaves: *Monthly Notices*, Dec. 1891; Meyer: *Die Kinder der Sonne*, 1891, p. 15; Sidgreaves: *Astronomy and Astro-Physics*, 1892, p. 212.

‡ The faculæ which according to Professor Hale cover the entire photosphere would appear to be prominences in process of formation, *i. e.*, prominences which are still white, such as one sees during an eclipse. Concerning these white prominences we shall have something to say in the following section. Hale: *Astronomy and Astro-Physics*, 1892, p. 415.

My theory also explains the various motions shown by the spots. They owe their different angular velocities in different latitudes to the cloudy zones in which they are borne. If they frequently move a little more rapidly than these clouds it is because the gas, which in the growing spot pushes back the matter of the photosphere, must move particularly toward the side where the resistance is least. Now as this side is that of the foremost edge of the spot "every time a spot undergoes sudden changes *it ordinarily advances on the solar surface by making a sort of leap*."* This leap will also take place when, as the gaseous contents of a spot are recondensing, the vacuum thus produced will draw in again the surrounding photospheric matter. For this matter rushing in, preferably on the side where it already moves in the direction of suction, will fill up the spot from behind, once more giving to its center a sudden acceleration.

It will be seen likewise that the centrifugal motion impressed on the matter at the center of a nascent spot cannot remain rectilinear, but that, being quickly retarded it will deviate into spirals as soon as it has been conducted by this movement into zones of new velocities. That is why the spots show spiral forms without actual rotation. If this explanation is correct, the spirals will be seen most strongly curved at the posterior side of the spot,† at the northern and southern sides of the same spot these spirals should be curved in opposite directions,‡ and spirals will be observed especially in spots which have just finished their growth or those on the point of disappearing.§ For if the photospheric clouds, where dispersed by the vapor produced at the center of a nascent spot, are spun into spirals, they will also return into spirals when the vapor, condensed anew, shall have produced a partial vacuum into which the photospheric matter will rush from all sides.

As to the *motion of the spots in latitude*, it is a remarkable coincidence that this movement changes sign in the same parallel of about 11°,|| where—according to my theory—the difference in velocity between the photospheric zones and their atmosphere changes sign also; it is, I say, this remarkable coincidence which I think has put me on the road to a plausible explanation. This explanation, however, needs a lengthy development and can not be summarized here.

* Young. *The Sun*, p. 110. Secchi. *le Soleil*, I, p. 141.
† See fig. 45 in Secchi's *le Soleil*, I, p. 88.
‡ Young, *The Sun*, p. 67.
§ Secchi. *le Soleil*, I, p. 88.
|| Carrington. *Obs. of the Spots of the Sun*. Secchi: *le Soleil*, I, p. 121. Young *The Sun*, p. 110. Spoerer. *Publ. Astr. Obs. Potsdam*, IV, 4, p. 418.

The separation of spots into definite zones has already been briefly explained in the section preceding. I have there explained what, according to my theory, is the *principal cause* of that distribution. But my theory requires several other causes which have been developed at length in my above mentioned memoir.

The change in the spectra of spots which Lockyer has discovered, namely, that at the approach of a minimum they show more and more distinctly the well-known lines of the terrestrial metals,* is, in reality foreseen from the diminution in latitude which they then show. For the photospheric zones where the spots are hollowed out float in the equatorial enlargement of the solar gas, and consequently contain the principal metals which, in virtue of their specific gravity, at higher latitudes remain buried in the depths. In comparing (1) the table in which Lockyer has so clearly shown that 1879 to 1880 there were sudden appearances of new lines, and (2) the table in which Spoerer has also very plainly shown that in the same years the spots also completely changed changed in latitude,† it is immediately seen that these two simultaneous changes are precisely what my theory demands.

My explanation of the periodicity in the number of spots can be *epitomized* as follows: while in the photospheric gas the molecules R, considered above, are still very numerous, there must be an exceptional combination of favorable circumstances for the molecules A and B to be able to produce a "heat eruption" of any account. That is the minimum period. But when after a long continued condensation of the molecules R, which are in the neighborhood of the photosphere layer, the molecules A and B, at first being much scattered, cease to be sufficiently separated and commence in turn an active combination. Then comes the period of incessant heat eruptions and the maximum period.

The molecules A and B by reason of this combination may lose the energy which they have; but other molecules, A′ and B′, etc., will replace them perhaps. But these in turn will exhaust themselves. Thus new spots will ever succeed one another without cessation, and we would wait in vain for a return of the minimum period were there not in the photospheric layers molecules, P and Q, with a stronger affinity, which await only condensation and rapid effacement of the molecules still separating them, to

* Lockyer: *Chem. of Sun*, p. 313-325

† Lockyer, *loc. cit.*, pp. 318, 325 The perfect agreement between the observations of Lockyer and the predictions of my theory give strong support to the view that the surrounding gaseous layers have curvatures different from that of the photosphere. If this hypothesis be correct this gas should have, in each latitude, a different chemical constitution. It is for this reason that the equatorial spots and prominences show so many metallic lines.

produce in turn their heat eruptions. These eruptions are then powerful agents of destruction to the molecules AB newly formed and restore as much as possible to its former state the composition which existed at the preceding minimum.

If this is a correct explanation, the periodicity of the spots depends in the first place on the molecules $P + Q + A + B + R$, of which the photospheric matter which produces the spots is composed. But as this matter has, according to my theory, at each latitude a somewhat different composition, the spots also should have at each latitude a somewhat different period. This is a conclusion which the observations of Spörer have perfectly confirmed. It is only necessary to consult the tables which Spörer has published showing the relation between the frequency of spots and their respective latitudes during the ten epochs into which he has divided each eleven-year period.* These tables show very clearly that when the maximum occurs at 16°, the maximum at 30° has already taken place two years previously, while that at 5° takes place two or three years later. They show in addition that the variations in the different local activities differ not only in phase, but also in amplitude and duration.

If we speak nevertheless of a periodicity in general of the solar activity, that periodicity has *no reference to any activity of the solar mass as a whole*, but is simply the resultant of all the local activities of different phases producing in *the total number of spots*, a maximum when their *mean latitude* is about 16°. The most of the faculæ, eruptive protuberances, and coronal rays being, as we shall presently see, *the effect of pre-existing spots*. When the Sun has a maximum of spots, it will evidently have at the same time a maximum of all those other marks of its activity.

§ 5.

EXPLANATION OF THE PROMINENCES.

The prominences are the transient "luminescences" at a place where in the quiet atmosphere of the Sun, elements hitherto dissociated are combining in proportion as their continual loss of heat makes this possible.

In order to make clear just how the diverse phenomena which these prominences present completely agree with the above definition, I must recall that if the elements A and B are combining in the prominences, that combination will take place only when

* Spoerer *loc cit*, p. 411-415.

the cooling shall have sufficiently condensed the molecules R which separate them; and that, this combination having taken place, *the "heat eruption"* then produced will separate anew the molecules R, recently condensed, and give them again the power of hindering the combination of the rest of the molecules A and B.

If such is the chemical process which causes the prominences, they should then show intermittent condensations. Now this is a requirement of theory corresponding, in fact, to observations. For in observing prominences during an eclipse, some of them appear to be made up of a white, dust-like material with a continuous spectrum. Among the white prominences which are then seen, there are some which the spectroscope completely fails to show, and that part of the ordinary prominences which the spectroscope does show us is, so to speak, only the skeleton. The white prominences have already been observed by Littrow, Carrington and Lias, who have also commented upon its fleeting character. It has especially been studied by Tacchini and Handrikof (August, 1887). Swift also made a careful study of it during the eclipse of Jan. 1, 1889.*

There are besides many other phenomena which prove that, in the solar atmosphere, these intermittent flashes of a white matter are not at all rare. I will cite here only (1st) the "*violes roses*" which Secchi has observed in the interior of spots and which are, no doubt, caused, as he thinks, by *the visible transformation of a white preexistent matter*;† and (2nd), the faculæ which Hale, by the use of his spectroheliograph, has seen to cover in 27 minutes the spot of July 15, 1892, faculæ which an hour later, had completely disappeared.‡

If the white glow of the prominences (and above all its intermittent character) can be considered as a striking verification of my theory, *their rose or peach-blossom color* is not unpredicted.

* Tacchini: *Rapp. d. eclisse del* 1870, tav. 5. Secchi: *le Soleil*, II. p. 78, I. pp. 373-376. F. Kemp: *Himmel u. Erde*, Oktober, 1889, p. 35, Clerke: *Hist. Astron.*, p. 248. Hastings: *Amer. Jour. Sci.*, Jan., 1891, Lockyer: *Chem. Sun* p. 407; Hale: *Astr. Nach.* 3053, Fényi: *Publ. Haynald Obs.* VI, 1892: *Über die im Spectroscop unsichtbare Protub. vom 19 Aug.* 1887, Tacchini: *Mem. Soc. Spet. Ital.* 1889, *Sull ecliss. totale di sole del 19 Agosto* 1887, Egoroff: *C. R.*, 109 p. 292, Arago: *Ann.* 1846, p. 460, Lockyer: *Solar Physics*, p. 108, Klein: *Die Sonnensystem* p. 36 etc. Stoney: *Phil. Mag.*, 1868, p. 450. Lias: *Fl'space celeste*, p. 55. The opacity of prominences seems to indicate that they are composed of matter in a sort of nebulous state. See Secchi: *le Soleil* II, p. 239. Hale: *Astronomy and Astro-Physics*, 1892, pp. 415, 431, 814, W. H. Pickering: *Ann. Harvard Coll. Obs.*, 18, 5, p. 100.

† Secchi: *le Soleil*, I, p. 104, figs. 53, 54, 55.

‡ *Nature* Sept. 8, 1892, Hale: *Astronomy and Astro-Physics*, 1892, p. 611; Hale: *loc. cit.*, Jan., 1893, Hale: *loc cit.* 1892, p. 431. Carrington: *Monthly Notices*, Nov. 1859.

For such a glow should result in the combination of the molecules A and B, and is in consequence a "chemical luminescence," precisely like that with which Wiedemann, E. v. Helmholtz, W. H. Julius and Pringsheim have made us familiar. It was previously believed that the brightness of a flame was principally due to its temperature. But it has been recently demonstrated that that brightness results rather from the chemical processes which take place in the flame. According to the very recent researches of Pringsheim and the earlier work of W. Siemens, the importance of those chemical processes would even be so great that without them the incandescent gases would not emit any light at all.* Now if the same gas at a constant temperature will or will not produce spectral lines, according as it is the seat of a certain chemical process or not, I recognize in this a beautiful confirmation of my theory of prominences, and the definite solution of a difficulty which I found it impossible to solve when, in 1888, I published my theory for the first time,—the difficulty of explaining *how it could be possible for the prominences to be more luminous than the surrounding parts of the solar atmosphere without being hotter.* Let us now explain why the prominences which are kindled in the region of the spots, have so often the *appearance of eruptions* and *enormous velocities.* These peculiarities are easily explained if we reflect that the spots emit less heat than does the photospheric matter which encompasses them and that, as a result, in those places above a spot, the radiation of the Sun will cause a greater loss of heat than elsewhere where that loss is more effectually prevented by the heat which is there radiated more abundantly from the undisturbed photosphere. Now as every prominence is the effect of a chemical combination, *which can be caused only by a loss of heat,* it is evident that all places above a spot will be, *ceteris paribus,* much more in position to form a prominence than neighboring localities. If then that prominence is produced it will have the deceptive appearance of an illuminated sheaf shooting out from a hole.

If the luminous displacements which these prominences exhibit often have great velocities, it is because they are suddenly caused by a cooling which is propagated with the enormous velocity of heat radiation. In the explosive mixtures which are burned in our laboratories† the chemical action is propogated in quite another manner. This propagation is relatively slow because all parts of the comparatively cold mixture must be successively

* See note of section 2.
† A. M. Clerke: *Knowledge,* Feb., 1893, p. 29.

kindled and heated by the contiguous parts already in combustion. In the prominences there is *absolutely* nothing of this kind. The chemical processes there do not require any considerable heating of a cold gas, but an infinitesimal cooling of a gas already highly heated. Now it is evident that, starting from a nascent spot this infinitesimal cooling should propogate itself with the velocity of radiant heat.

If, however, the displacements of the luminous state never attain that great velocity, it is because (as I have explained above) it is impossible, in a gaseous mixture as vaporous and as complicated as the solar gas, that the dissociated matter A + B can be always ready for combination at the least fall in temperature. It is the condensation of the molecules R, which in taking more or less time, so greatly diminishes the velocity of the luminous displacement and also causes in the prominences those queer accelerations and retardations so often observed.

There is no velocity, however small or great, which my theory does not provide for. It is only when this speed surpasses that of heat radiation that it becomes inexplicable.

It is clear, moreover, that in virtue of the difference in angular velocity between a spot and the atmosphere which covers it, new prominences of the same spot will be constantly produced in the changing atmospheric mass which, for the time being, overlies the spot. As the production of prominences requires some time, it is easily seen, 1st, *why spots often appear near an eruptive prominence** and 2d, why, above the spots observed near the limb, "*eruptive prominences are sometimes seen to renew themselves after intervals of 5 or 6 hours*, and to flame out on one side and be extinguished on the other."†

But, someone may say, if all the prominences are merely deceptive appearances, why is it then that we find *metals* at the bases of some of them and not at others? To this I reply that, by reason of the slight flattening of the solar stratified layers, the equatorial atmosphere ought to contain, at its base, metals which at higher latitudes will always remain buried in the depths of the photosphere. Now as *the prominences are always of the same material as the tranquil stratified atmosphere which they at times illuminate*, clearly those metals will be seen in them only when they rise in the deepest layers of the equatorial zones. And as it is only the eruptive prominences which rise from below, they are also the only ones to show us these metals. Their

* Secchi *le Soleil*, II p. 177.
† Secchi, *l. c.* p. 96.

height will in this region moreover count for nothing. Even at the least height the equatorial prominences will always be metallic. It is, according to Secchi, ordinarily small eruptions which are remarkable for their richness in metallic vapors.*

I do not wish to finish with the prominences without first saying a word about the displacement which their spectral lines often show. This displacement is always interpreted as if it certainly demonstrated that we are dealing with real motions and not with a simple change of position of luminous form. But that certainly does not exist. For it has never been proved that that interpretation generally adopted is the only one possible. It is not that I doubt in the least the entire truth of Doppler's principle. That truth is definitely established; for Huggins, Zollner, Vogel, Hastings, Young, Langley, Cornu, Crew and Dunér have clearly proved that the rotation of the Sun really causes the required displacement of the lines.

But this displacement of lines so well established when luminous matter is displaced does not at all prove, and never can, that a similar displacement of lines cannot also be caused in an entirely different manner and in a way, for example, such as that which my theory calls for.

If the well established displacement of lines when luminous matter is displaced, were a phenomenon easy of description and explanation, one would hardly dare to offer any interpretation other than that ordinarily given. But that complete explanation has never been given, and in fact to give one a very clear idea of the phenomenon it is necessary to know, *first*, the motion in the source of light which produces the waves in the ether; *second*, what, on the other hand, is the influence of the waves of ether upon the vibratory movement of the luminous atoms, and, *third*, in what manner the individual vibrations of the atoms interfere to produce waves of determinate phase.

Now all these details are still very mysterious. So long as one does not understand them, he cannot have the least certainty that a displacement of the lines is not produced just as well by the luminous propagation of a chemical synthesis in the tranquil matter as by the transport of the luminous matter itself.

If, as a result, the study of prominences proves that it is impossible for their motions to be real, the displacement of their lines will not prove that the impossible is true nevertheless.† *Let*

* Secchi *l. c.*, p. 149; Fényi Publ. des Haynald obs. 1892, pp. 13, 22.

† My new interpretation of the displacement of the lines at once received a favorable review at the hands of Mr. Fowler, who, in *Nature*, 21st March, 1889, says expressly that he sees no reason to differ from me in this new explanation.

no one imagine, however, that in thus establishing my theory, I have had to sacrifice Doppler's principle."* It is not the principle itself which I attack, but only its common interpretation which has never been demonstrated. For if it is proved that a displacement of the luminous source causes a displacement of lines, it does not at all follow that, conversely, each displacement of lines is *necessarily* brought about in the same manner. Consequently I sacrifice only an unproved assertion. Now it seems to me that my theory, so full of plausible explanations, is well worth so insignificant a sacrifice.

§ 6.

Explanation of the Corona

The brilliant filaments, the rays, the streamers and the sheets of white light at the limb of the eclipsed Sun forming an aureole of light to which we have given the name corona, arise at those places in the gaseous envelope of the Sun where a continual loss of heat causes the condensation of a cloudy mist. This mist is incandescent and emits a white light with continuous spectrum, but as it reflects also the light of the photosphere, the resulting light is more or less polarized and shows the dark lines of Fraunhofer. The coronal gas where this mist is condensed is recognized by its bright lines even in the darkest clefts. It rotates on its axis just like the rest of the Sun.†

It is clear again that the misty matter will by preference form in the places where the heat is least intense. Such places are always found immediately over a spot, since the gas with which the cavity of the spot is filled, although at the same temperature as that of the photosphere, has vastly smaller emisssive power. It is then easy to understand why, during a maximum period of spots, the coronal rays take their rise especially from the zones where the spots most abound and thus give to the corona that characteristic form of a star with four rays. If these four rays seem to originate in a latitude greater than that of the spots, it is only because we see them indistinctly projected upon the surface of the sky. The latitude from which a ray appears to emanate is not necessarily that towards which this ray is really directed. The polar zone, for example, might be completely free

Quite recently Fizeau has given still another interpretation to the displacement of the lines His hypothesis resembles mine in that it does not require high velocities of the luminous matter. Fizeau *C. R.* 7th Sept., 1891; *Astronomy and Astro-Physics*, 1892, p. 126

* Koeber *Himmel u. Erde*, April, 1893, p. 345.

† See note sec. 3.

from coronal streamers, and yet one would still see there projected the rays produced in the region of the spots.

This explanation of the square, cruciform, corona [an explanation which I have already clearly outlined in 1888 in my first "*Essai d'une Théorie du Soleil*"]* is identical with that which Schaeberle proposed later on as a result of observations made during the eclipse of Dec. 22, 1889.† Schaberle has reached the conclusion—which is mine also—that the corona is made up of rays of luminous matter projected from the spot-zones. By sticking needles into the corresponding zones of a ball, Schaeberle has made a model which reproduces many of the diverse appearances presented by the corona. According to him‡ the corona of April 16 last corresponded perfectly to the requirements of his theory; it quite resembled the figure which he had predicted.§ But if the corona of April 16 perfectly agreed with the form predicted by Schaeberle, it agreed also with mine.

Since, according to Schaeberle, the corona is neither more nor less than matter ejected through the spots (holes in the photosphere) he expects to find the same coronal structure, both at the surface of the Sun and at hundreds of thousands of miles outside.

While at the time of a spot-maximum, the corona has a tendency to take the cruciform shape mentioned above, it presents at other times appearances which are quite different, appearances not so easily explained.

This capricious character is doubtless connected with the nebulous origin of the phenomena, possibly with their intermittent nature, and with their resemblance to solar prominences. In any event, we must not forget that the spots are not uniformly distributed in the "spot-zones," and that, in *all* zones, there are "pores," "veiled spots" and faculæ, each modifying the emissive power of its own neighborhood, and thus influencing local condensation. To these effects, the form of the corona is very sensitive. Since the spots have velocities which are somewhat abnormal it is clear that the condensation which is determined by the radius vector (prolonged) of the spot will continually invade new regions of the corona. Hence those rapid changes, sometimes observable within the space of twenty minutes, to-wit an apparent wavering of the streamers.‖

* *Essai d'une Théorie du Soleil et des Étoiles variables* (Delft, Dec., 1888), p. 44.

† Schaeberle. *Publ. Astr. Soc. Pac.*, No. 7; *Nature*, May 15, 1890.

‡ Schaeberle. *Astronomy and Astro-Physics*, May, 1893, p. 463.

§ Schaberle. *l. c.*, Jan. 1893. Plate II.

‖ Young. *The Sun*, p. 192. Lockyer. *Solar Physics*, p. 294, 377. *Nature*, Vol. 18, p. 457. Pritchett. *Rep. Wash. Univ. Eclipse Party*, Eclipse of Jan. 1, 1889. *Nature*, 24th, Dec. 1891.

From what has been said, it will be seen that the beautiful corona of the maximum-period teaches us very little as to the real form of the coronal layer. For its figure is determined very largely by the condition of the photospheric surface in very distant parts of the Sun. What this condition may happen to be at any time is for us, at present, a mere matter of chance.

The minimum corona will, of course, be more regular and less brilliant.* For the spots and faculæ are then fewer in number, and the emissivity of the solar surface does not vary sensibly from one zone to another. There is nothing, therefore, to determine a greater intensity of coronal light in one zone than in another.

It is then at minimum-period, if ever, that one may hope to see this extremely tenuous outer layer of the Sun in its true form. At such a time, above all others, photographs are important. The excellent plates which Mr. Barnard obtained on the 1st of January, 1889, are, in this respect, exceedingly valuable.

If my theory is true, the gaseous envelope of the Sun has a density much greater than has usually been assigned to it. The prevailing opinion is based upon the sharpness of the Fraunhofer† lines and upon the vanishingly small resistance which the corona offers to the passage of comets.‡ But I have shown in my "*Memoire*" published by the Amsterdam Academy (pp. 87–115) that these arguments are by no means convincing. It matters little that my discussion is too lengthy to produce here, for my views have recently received the most thorough support in Deslandre's observations during the eclipse of the 16th of April, 1893.§ His measures of the equatorial velocity of the corona at a distance from the Sun amounting to two-thirds of a solar diameter show that the corona rotates just like the rest of the Sun. Accordingly the coronal envelope is one immense atmosphere exerting pressure upon the photosphere. For it is quite impossible that an atmosphere so tremendous should have a negligibly small density from its highest, all the way down, to its lowest portions.

DELFT, 16th July, 1893.

* Lockyer: *Chem. of Sun*, p. 443. Clerke: *Hist. of Astron.*, p. 233. Pickering: *Astronomy and Astro-Physics*, May, 1893.

† Scheiner: *Spectral-analyse d. Gestirne*, (1890), p. 206.

‡ Scheiner: *l. c.*, p. 207. Clerke: *History of Astronomy*, p. 249. Young: *Elements of Astronomy*, p. 147. Fényi: *Mem. Soc. Spet. Ital.*, 21, 1892. *Note sur une Protubérance excessivement grande observée le 3 Oct., 1892.*

§ Deslandres: *C. R.* 15th, May, 1893. *Nature*: 25th May, 1893.

ASTRO-PHYSICAL NOTES.

All articles and correspondence relating to spectroscopy and other subjects, properly included in ASTRO-PHYSICS, should be addressed to George E. Hale, Kenwood Observatory of the University of Chicago, Chicago, U. S. A. Authors of papers are requested to refer to last page for information in regard to illustrations, reprint copies, etc.

Is there Oxygen in the Atmosphere of the Sun?—M. J. Janssen makes the following remarks on the note of M. Dunér printed in our present number.

"In this note the learned Director of the Upsala Observatory seeks to show that the complex question of the presence of oxygen in the Sun's gaseous envelopes may be regarded as solved by the observations which he has made in the course of his researches on the rotation of the Sun.

I wish to say here that unfortunately I do not share this opinion. I have in fact been led by long and profound study of the question, to recognize that the method of displacement of lines, admirable as it is in other respects, is insufficient to give a solution of the problem. If the question of the presence or absence of the metalloid oxygen in the solar atmosphere could be solved by the simple experiment of causing the lines to oscillate, it would be useless to devote one's self to long and laborious observations at elevated stations, or with tubes full of oxygen at different pressures at the surface of the Earth.

The discrimination between the solar and the terrestrial origin of a spectral line, on the basis of changes of refrangibility due to the Sun's rotation, was made for the first time by Thollon. It constituted one of the most decisive proofs of the reality of the principle which was first enunciated by Doppler, but which, in the hands of M. Fizeau, has become an admirable method of confirming and measuring celestial motions.* In 1884, M. Cornu, in order to distinguish a line of solar from one of terrestrial origin, gave to this method an extremely elegant form by an arrangement which gives a kind of oscillatory motion to the solar lines, and permits them to be immediately distinguished from the fixed lines of terrestrial origin.

M. Dunér, in his important researches on the rotation of the Sun, which were made much later than these investigations, has not certainly done more than confirm them.

But it should now be remarked, that if the distinction between a dark line of exclusively solar origin and one due entirely to the terrestrial atmosphere can in fact be made with certainty, because the phenomenon is there presented in all its simplicity, the case is not the same if the line is due at the same time to both the solar and terrestrial atmospheres. One might then fear that the displacement of the solar part of the line would be masked by the breadth of the terrestrial part, all the more, in the case of oxygen, because we know to-day that the action of the terrestrial atmosphere gives a very great intensity to the oxygen lines, and that therefore, a solar action, if it exists, can be only a very feeble one.

But the phenomenon is still more complex, for the question of solar oxygen necessarily involves the coronal atmosphere, and we know that this atmosphere is the seat of violent movements, about which, however, very little is known. But if dark lines originate in that atmosphere, the phenomena of change of refrangibility due to motions of the source would necessarily depend upon these

* Some observations have also been made outside of France.—*Tr.*

movements, and might in an unknown measure modify the effect of the probable rotation of the coronal atmosphere.

Thus it may be seen how complex the question is, and how inadequate to its solution is the method based on the displacement of lines. It is these considerations which have caused me to attack the question from another side.

I have thought that it would first be necessary to determine the part played by the Earth's atmosphere, and only undertake the solar phenomena after this first part of the question had been elucidated.

The instrumental equipment of the Observatory of Meudon allows an absolutely decisive experiment to be made on the origin of the groups of oxygen lines in the solar spectrum. Oxygen at a pressure of 28 atmospheres has been introduced into a steel tube 60 metres long, lined with red copper. This quantity of oxygen represents that which is contained in the terrestrial atmosphere, with respect to a ray traversing it in the direction of the zenith, and it has been proved that with a powerful electric light as luminous source, the groups, notably A and B, have an intensity comparable with that of the same groups in the solar spectrum during the summer when the Sun is very high. A phenomenon is thus presented in which it is only necessary to judge of the equality of two effects, constituting the most favorable condition that can be realized in researches of this nature.

The same experiment was made under another form in 1889 with the aid of a powerful electric light placed on the summit of the Eiffel tower. The stratum of air between the tower and the Observatory of Meudon represents quite accurately in its absorptive action, the Earth's atmosphere, and in this case also it is possible to estimate the practical equality of the effect with that of the solar spectrum.

In 1888, at the Grands Mulets, I attacked the question under another form. My intention was to study the decrease in the intensity of the groups in question due to the altitude of the station.

These observations, even then conclusive, were repeated in 1890 at the Bosses-du-Dromadaire, and finally on the summit of Mont Blanc, with a large grating spectroscope defining the elementary lines of the groups A, B and α with great perfection; the results are known.

I believe that these researches show that the terrestrial atmosphere may be regarded as the sole cause of the presence of the oxygen groups in the solar spectrum, and that this first part of the question can only be solved by the study of the terrestrial atmosphere, where the phenomena presented are absolutely known to us. It may then be said that the gaseous envelopes of the Sun do not contain oxygen, at least in a state capable of producing the absorption phenomena which we see produced in our tubes and in the atmosphere of the Earth.

It is now necessary to attack the Sun considered by itself, and this I have already commenced to do by examining the spectrum of oxygen at a high temperature. I shall send an account of these new labors to the Academy.

To sum up the method which has served M. Dunér in his important researches on the rotation of the Sun, researches, however, in which the author did not have in view the question of the presence of oxygen in the Sun, cannot, in my eyes, be regarded as in itself capable of elucidating the complex question which is the subject of his note."—*Comptes Rendus*, cxviii, No. 2, (8 Janvier, 1894).

While we fully recognize the importance of M. Janssen's researches on the origin of the oxygen lines in the solar spectrum, we do not think

there is much in these remarks to weaken the force of M. Dunér's argument. It is not clear what the possible existence of oxygen in the corona has to do with the matter. As both authors point out, the observations which are considered relate to only one phase of the question, namely, the existence of solar lines coinciding with the lines of the terrestrial groups A, B and α. If such lines exist it follows that there is oxygen in the Sun. If they do not exist it does not follow that there is no oxygen in the Sun. Assuming the existence of a solar component, there is no doubt that the doubling of these oxygen lines at the east or west limb of the Sun would be certain to occur as pointed out by M. Dunér; the only question is as to the possibility of detecting it. If the difficulty of the observation lies in the faintness of the solar component, as suggested by M. Janssen, then we may point out that his own observations would be indecisive for the same reason, for on the summit of Mont Blanc more than half the mass of the Earth's atmosphere is still above the observer, and in the comparisons with tubes full of oxygen the slight additional intensity of the telluric lines in the solar spectrum, due to a true solar component, would be inappreciable.

An estimate of the difficulties arising from the closeness of the components can be formed with the aid of the following considerations. The maximum separation would be very nearly that due to the motion of the east or west limb of the Sun; at B it would amount to about 0.05 tenth meters. This is about the limit of resolution of Rowland's map, but in his memoir on the rotation of the Sun Professor Dunér has given a list of lines not recognizable as double on the map, but which he found to be double with his instrument. Hence it would seem that by careful observation of the sharpest lines, the duplicity in question should be detected.

Professor Schuster contributes a note on the same subject to the *Comptes Rendus* (Jan. 15, 1894), in which he points out that in 1877 he showed that several lines in one of the spectra of oxygen are perhaps represented by dark lines in the solar spectrum. Absolute coincidence of the lines cannot be asserted on account of small errors to which the observations were subject, but he thinks their possible identity should be considered in a discussion of the question.

Report of the Wolsingham Observatory for the year 1893.—In this very brief report Mr. Espin does little more than mention the various important observations made by him during the past year. They relate chiefly to the discovery of stars with remarkable spectra, and the results are well known to our readers. Four hundred and eighty-nine such stars were discovered besides fifteen nebulæ not included in the New General Catalogue.

The Observatory has sustained a severe loss in the death of Miss Brook, who was greatly interested in its work and generously defrayed many of the incidental expenses.

Large Rock-salt Prism and Lenses.—Mr. Brashear is making for the Smithsonian Institution a rock-salt prism and set of lenses which, when completed, will probably be the largest optical train yet constructed of this material. The 60° prism is seven inches high, and the width of each face is five inches. The lenses are seven inches in diameter. While the prism is not perfectly free from flaws, the loss of light from this cause will hardly be more than five per cent., and the lenses are even clearer than the prism. All three pieces were cut from a block of rock-salt which formed part of the Russian mining exhibit at Chicago.

Photographic Investigations of Captain Abney.—The relation between the brightness of a photographic image and the time of exposure necessary to produce a given density in the negative is a very important one, as it has a direct bearing on the methods of determining stellar magnitudes by photography. The simple relation that the product of the intensity and the time is a constant can only be assumed to hold within certain limits, and Captain Abney has always made this reservation in stating his belief in the principle. In a recent communication to the Royal Astronomical Society (*Monthly Notices*, LIV, Dec. 1893) he admits, however, that he was not prepared to find so wide a departure from the usually accepted law as that indicated by some of his experiments. With very feeble light the time of exposure had to be much more than proportionally increased.

Another important fact brought out by the investigation is that the sum of a large number of small exposures is not equal to the same exposure given at one time.

According to Captain Abney's results a large telescope has a greater advantage over a small one, in certain kinds of photographic work, than would be expected from a comparison of the areas of their objectives. The more the subject of photographic magnitudes is investigated the more numerous seem to be the difficulties with which it is beset.

Theory of Optical Instruments by Dr. Czapski.*—The ease with which all the fundamental laws of optics lend themselves to exact symbolic expression has made the subject a tempting one to mathematicians. One result has been a rather wide gap between theory and practice, between theory and technique, perhaps we should say, from which each possibly has suffered.

The book which Dr. Czapski has given us may fairly be called remarkable for the manner in which on every page he expresses his experimental facts in easily handled equations and, conversely, interprets his equations in terms of practical experiment. In this respect the author is quite in harmony with Mascart in his *Traité d'Optique* and Lord Rayleigh in his *Wave Theory of Light*, in each of which the constant appeal is from pure mathematics to experiment.

Another striking feature of the work is its excellent balance; a sense of proportion is seldom, if ever, lost; the special case is always exhibited in its relation to the general, at least in so far as the "universal dovetailedness of things will permit."

The first fifty pages are occupied by an exceedingly general and, of course, essentially Gaussian treatment of the subject. This, like the remainder of the book, is laid down on the lines of Abbe, and makes by far the most elegant presentation with which your reviewer is familiar.

Any optical system whatever is treated simply as a device for transforming a plane in one part of space into a corresponding plane in another part of space. For convenience, these two regions are called "object space" and "image space" respectively. The whole of what is ordinarily known as geometrical optics is based upon this single definition. The optical instrument thus defined is completely *described* by the fifteen constants of the following equations, which determine any point (x', y', z') in the "image space" in terms of its corresponding point (x, y, z) in the "object-space."

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* *Theorie der Optischen Instrumenten nach Abbe* von Dr. Siegfried Czapski. (Breslau 1893.) pp. 292.

$$x' = \frac{a_1x + b_1y + c_1z + d_1}{ax + by + cz + 1}$$

$$y' = \frac{a_2x + b_2y + c_2z + d_2}{ax + by + cz + 1}$$

$$z' = \frac{a_3x + b_3y + c_3z + d_3}{ax + by + cz + 1}$$

Planes of discontinuity in these two regions are recognized later as the two focal planes. The single case in which there is no discontinuity, *i. e.*, the case in which finite values of x, y, z, always yield the finite values of x', y', z', is later seen to be the case of the astronomical telescope when adjusted for a normal eye. In like manner are derived a number of other fundamental theorems.

The next problem is to reduce, without any essential sacrifice of generality, the fifteen constants to a workable number. This is accomplished, in a really elegant manner, by the successive introduction of the actual working conditions and by choice of convenient reference axes; so that only two constants, A and B, remain, giving the equations

$$x' = \frac{A}{x}$$

$$y' = \frac{By}{x}$$

$$z' = \frac{Bz}{x}$$

Of these, curiously enough, B happens to be the first focal length of the system, and A the product of the first and second focal lengths.

The first of these equations is, at once, *Newton's Rule*; the second and third give the magnifying power, as ordinarily understood; while the longitudinal magnification $\left(\frac{dx'}{dx}\right)$ comes from the first by direct differentiation; and so on. This rough sketch will give a general notion of the method pursued throughout.

The treatment of lenses and systems of lenses follows next, and is perhaps [redacted]

strongly insisted upon, for both incident and emergent beams. To those who will look through the book in vain for any discussion of the spectroscope, as such, these pages, together with those on the prism, are to be especially commended. On the other hand, disappointment must be confessed at not finding here a somewhat fuller discussion of diffraction effects determined by aperture. Reference is here intended especially to the distribution of light in the images of points and to the immediate dependence of resolving power upon this. The advantage of large objectives in offering increased light and diminished "spurious disc" is given, but no mention is made of the third advantage of these large glasses in giving a *steeper* intensity curve from the center to the edge of the "spurious disc." The proportion of the total incident light which an instrument is able to concentrate in the "spurious disc" is not touched upon. Possibly these and kindred subjects are reserved for the treatise which, in the preface, is promised from Professor Abbe.

The last hundred pages are devoted to the description of various optical instruments including the human eye, the photographic lens, the compound microscope, the astronomical telescope and the focometer. Although descriptive, these chapters are nowhere prolix, and the reader will find that he must keep himself mentally wide awake if he is to follow the author intelligently. For the student who has not followed the earlier pages as intelligently as he might wish, this later part of the volume is a helpful collection of special problems solved by the aid of the general principles laid down in the first part. To the microscopist and photographer the well known authority with which Dr. Czapski speaks makes this part of the work of high value.

Of the volume as a whole, it may be said that ordinary and uninteresting details have everywhere been omitted. Among *these* details, it is not our intention to include the index, which is not to be found. Will someone kindly explain the aversion which English and Continental writers have for indexes? In this case almost any other half dozen pages might have been omitted with less loss than those which would have sufficed for an index.

References to original memoirs abound in every chapter. The author's evident acquaintance with the work of Lord Rayleigh makes it all the more surprising that he should refer to Cotes' theorem regarding the magnifying power of any system as "*der Helmholtz-Lagrangesche Satz*. To Cotes and his theorem there appears to attach a peculiar fatality in virtue of which it has been necessary that he himself should be discovered successively by Newton* and Rayleigh, and that his theorem should be independently discovered three times in succession.

As to the author's style one word, at least, must be said. In the heaping of one dependent clause upon another to form gigantic sentences, he is possibly following only the genius of the German language. Any comparison with French or English must, of course be forborne. Into these long sentences are thrust interminable parentheses, often several in number necessitating as much brain labor on German syntax as is called for by the subject matter. These remarks might be quite out of place, were it not that recent scientific literature has shown the German language capable of better things.

To systematize and unify, as Dr. Czapski has done, so many original contributions to any subject is of course a valuable scientific service. HENRY CREW.

* It was Cotes of whom Newton made his classical remark, "If Mr. Cotes had lived, we should have known something."

Solar Observations at the Roman College in 1893.—The following is extracted from a letter recently received from Professor Tacchini:

On account of my trip to America, my communications are somewhat behind hand, and at present I am only able to send you an abstract of the solar observations made at the Royal Observatory of the Roman College in the second and third quarters of the year 1893.

1893	Days of Observation	Relative Frequency of Spots	Relative Frequency of Days without spots	Relative Size of Spots	Relative Size of Faculæ.	No. of Groups per day
April	26	29.11	0.00	110.5	90.6	7.3
May	25	23.16	0.00	95.2	89.8	7.0
June	27	28.74	0.00	101.3	63.9	6.9
July	30	26.30	0.00	138.1	94.2	7.3
August	31	44.84	0.00	227.5	111.0	10.2
September	30	30.77	0.00	141.1	134.0	6.7

During the second quarter of the year, the solar activity, with reference to the spots, showed an increase, and the frequency of the groups of spots remained nearly constant. As in the preceding quarter, veiled spots and pole faculæ were pretty frequent. In the third quarter the diurnal frequency of the true spots comes out rather small, but on account of the increase in the number of groups, the great number of large pores (trous), and the considerable size of the spots, the solar activity should be regarded as decidedly greater than in the preceding months, with a well-marked maximum in the month of August. The faculæ also were more numerous. Following are the results of observations of prominences

1893	Days of Observation	Average Number	Average Height ″	Average Breadth ′
April	26	11.58	39.3	1.8
May	21	6.52	40.1	1.9
June	26	5.81	38.8	1.9
July	26	6.23	37.4	1.7
August	29	8.73	36.3	1.8
September	26	6.77	36.5	1.7

During the second quarter the phenomena of the prominences were of about the same intensity as in the first months of the year, and the only point requiring notice is the secondary maximum in the month of April. In the third quarter the prominences continued to diminish in importance, while the spots showed a considerable increase, showing that the relation between the two phenomena is not so intimate as it was once supposed to be. It seems appropriate to remark, in connection with this subject, that as auroras and great magnetic disturbances have been much less frequent during the period in question, my former views have been confirmed, namely, that terrestrial phenomena are more closely related to the phenomena of the solar atmosphere than to the spots. The greatest heights reached by prominences were as follows:

	′		″
April	187	July	75
May	127	Aug.	121
June	90	Sept	121

P. TACCHINI

The following letter has been received from Professor Tacchini:

I take pleasure in sending you the results which I have obtained for the distribution in latitude of solar phenomena, from observations made at the Royal Observatory of the Roman College during the first half of the year 1893.

1898. Latitude.	Protuberances. First Quarter.		Faculæ. First Quarter.		Spots. First Quarter.		Eruptions. First Quarter.	
90 + 80	0.000							
80 + 70	0.005							
70 + 60	0.017							
60 + 50	0.017							
50 + 40	0.052	0.339	0.013		0.000			
40 + 30	0.055		0.048		0.076			
30 + 20	0.085		0.117	0.429	0.272	0.435		
20 + 10	0.068		0.160		0.087			
10 + 0	0.040		0.091					
0 − 10	0.046		0.147		0.109			
10 − 20	0.084		0.255		0.337			
20 − 30	0.095		0.143	0.571	0.109	0.565		
30 − 40	0.127		0.022		0.011			
40 − 50	0.127	0.661	0.004					
50 − 60	0.111							
60 − 70	0.066							
70 − 80	0.003							
80 − 90	0.002							
	Second Quarter.		Second Quarter.		Second Quarter.		Second Quarter.	
90 + 80	0.000							
80 + 70	0.000							
70 + 60	0.011							
60 + 50	0.011							
50 + 40	0.045	0.345	0.000					
40 + 30	0.092		0.025		0.000			
30 + 20	0.052		0.082	0.385	0.000	0.408		
20 + 10	0.062		0.155		0.229		0.500	
10 + 0	[illegible]		0.123		0.079		0.250	1.000
0 − 10	0.102		0.129		0.071		0.125	
10 − 20	0.113		0.234		0.314		0.125	
20 − 30	0.094		0.186	0.615	0.200	0.529		
30 − 40	0.102		0.060		0.007			
40 − 50	0.056	0.655	0.006					
50 − 60	0.075							
60 − 70	0.108							
70 − 80	0.005							
80 − 90	0.000							

The frequency of the protuberances is nearly twice as great in the southern zones as it is in the northern, and all the solar phenomena were more frequent in the southern hemisphere; the same circumstance is manifest in the data of each month. The absolute maxima by zones was also always found in this hemisphere. The maxima of faculæ and of spots occurred in the same zones (±10°, ± 20°), while those of the protuberances were situated in higher latitudes.

In the first quarter of the year no eruptions were observed.

P. TACCHINI.

PLANET NOTES FOR APRIL.

H. C. WILSON.

Mercury will be "morning star" during April, and will be at greatest elongation, west from the Sun 27° 40′, on the tenth of the month. Mercury will be in conjunction with the Moon April 3 at 5^h 37^m P. M. central time.

Venus is also "morning star" and is nearing greatest elongation west from the Sun. The greatest distance from the Sun, 46° 10', will be reached on the morning of April 27. This will be a favorable month, so far as position is concerned, for the study of the surface markings of Venus, although the fact that she is only visible in the morning will be a drawback to all but the most enthusiastic amateurs. On the morning of April 5 Venus will be near the star α Aquarii, conjunction in right ascension occurring at 2^h 17^m A. M. central time. Venus will then be 19' south of the star. The illuminated portion of her disc will increase during the month from one third to one half, while her brilliancy will decrease in the ratio of 195 to 139.

Mars improves a little in position during April, but it will not yet pay to spend much time in trying to observe this planet. He will move eastward and northward through the center of the constellation Capricornus. As he is brighter than any of the stars in the constellation it would not be difficult to identify him without the ruddy color which makes him so conspicuous. Mars will be in conjunction with the Moon April 29 at 1 A. M.

Jupiter will be pretty low in the west during the observing hours of April, but some satisfactory views may yet be obtained. He is moving slowly eastward south of the Pleiades. Jupiter will be in conjuction with the Moon, 5° south, April 9 at 5 A. M.

Saturn and *Spica* (α Virginis) make a fine pair in the south in the morning. They are nearly equal in brilliancy but differ a little in color, Saturn having a golden hue while Spica is bluish white. Saturn is retrograding, that is moving westward, and at the end of April will be almost directly north of Spica. He will be at opposition April 11 at noon. The moon will pass by Saturn, 4° to the south, April 10 at 9^h 28^m P. M.

Uranus is toward the southeast from Saturn in the constellation Libra. On the morning of the 27th at 7^h 11^m he will be in conjunction with the second magnitude star α Libræ, being only 4' north of the brighter component of that star which is a wide double. The motion of Uranus is so slow that he will be in the vicinity of the star for several days, so that this will be an excellent opportunity for the amateur to be sure that he has seen this planet. Note the green color and the visibility of a definite disc.

Neptune may be observed in the early evening but has past the most favorable position. He is about half way between ι and ϵ in the constellation Taurus.

Planet Tables for April.

[The times given are local time for Northfield. To obtain Standard Times for Place in approximately the same latitude, add the difference between Standard and Local Time if west of the Standard Meridian or subtract if east].

MERCURY.

Date. 1894.	R. A. h m	Decl. ° '	Rises. h m	Transits. h m	Sets. h m
Apr. 5	23 21.7	− 5 35	4 47 A. M.	10 25.9 A. M.	4 05 P. M.
15	23 58.8	− 2 52	4 31 "	10 23.5 "	4 16 "
25	0 49.0	+ 2 20	4 22 "	10 34.4 "	4 47 "

VENUS.

Date. 1894.	R. A. h m	Decl. ° '	Rises. h m	Transits. h m	Sets. h m
Apr. 5	22 12.1	− 8 36	3 47 A. M.	9 16.6 A. M.	2 46 P. M.
15	22 44.8	− 6 53	3 34 "	9 09.7 "	2 46 "
25	23 20.6	− 4 22	3 20 "	9 06.1 "	2 52 "

Date. 1894.	R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m
			MARS.		
Apr. 5	20 33.6	− 19 56	2 58 A. M.	7 38.3 A. M.	12 19 P. M.
15	21 02.7	− 18 14	2 40 "	7 28.0 "	12 16 "
25	21 31.1	− 16 18	2 20 "	7 17.1 "	12 14 "
			JUPITER.		
Apr. 5	3 54.5	+ 19 43	7 32 A. M.	2 58.0 P. M.	10 24 P. M.
15	4 02.9	+ 20 09	6 59 "	2 27.0 "	9 55 "
25	4 11.9	+ 20 35	[illegible] 26 "	1 56.5 "	9 27 "
			SATURN.		
Apr. 5	13 26.5	− 6 06	6 49 P. M.	12 28.3 A. M.	[illegible] 07 A. M.
15	13 23.6	− 5 48	6 05 "	11 46.1 P. M.	5 27 "
25	13 20.8	− 5 32	5 22 "	11 04.0 "	4 46 "
			URANUS.		
Apr. 5	14 48.4	− 15 47	[illegible] 51 P. M.	1 49.9 A. M.	6 49 A. M.
15	14 46.9	− 15 40	8 09 "	1 09.1 "	[illegible] 09 "
25	14 45.3	− 15 33	7 28 "	12 28.2 "	5 28 "
			NEPTUNE.		
Apr. 5	4 40.0	+ 20 41	8 12 A. M.	3 43.3 P. M.	11 14 P. M
15	4 41.0	+ 20 44	7 34 "	3 05.1 "	10 36 "
25	4 42.3	+ 20 46	6 56 "	2 27.0 "	9 58 "
			THE SUN.		
Apr. 5	0 58.7	+ 6 16	5 34 A. M.	12 02.6 P. M.	6 31 P. M.
15	1 35.5	+ 9 57	5 17 "	11 59.9 A. M.	6 43 "
25	2 12.8	+ 13 22	5 00 "	11 57.8 "	6 55 "
			THE MOON.		
Apr. 1	21 26.4	− 19 30	4 03 A. M.	8 46.6 A. M.	1 39 P. M.
3	23 03.0	− 8 40	4 35 "	10 15.2 "	[illegible] 08 "
5	0 38.8	+ 4 12	5 21 "	11 42.8 "	6 19 "
7	2 22.6	+ 16 52	6 02 "	1 18.4 P. M.	[illegible] 51 "
9	4 22.8	+ 26 06	7 01 "	3 10.5 "	11 33 "
11	6 36.6	+ 28 28	8 43 "	5 16.0 "	1 45 A. M.
13	8 45.5	+ 22 53	11 12 "	7 16.6 "	[illegible] 06 "
15	10 37.5	+ 11 51	1 50 P. M.	9 00.5 "	3 54 "
17	12 17.5	− 1 19	4 19 "	10 32.4 "	4 32 "
19	13 55.1	− 13 51	6 42 "	12 01.9 A. M.	5 10 "
21	15 38.2	− 23 27	9 04 "	1 36.8 "	[illegible] 02 "
23	17 28.7	− 28 16	11 16 "	3 19.1 "	7 20 "
26	19 20.3	− 27 22	12 57 A. M.	5 02.6 "	9 11 "
28	21 05.0	− 21 18	2 04 "	6 39.2 "	11 23 "
30	22 41.6	− 11 17	2 48 "	8 07.6 "	1 38 P. M.

Approximate Central Standard Times when the Great Red Spot will cross the Central Meridian of Jupiter.

		h m			h m			h m
Apr.	1	7 26 P. M.	Apr.	11	5 40 P. M.	Apr.	21	3 54 P. M.
	[illegible]	3 17 "		12	11 27 "		22	9 41 "
	3	9 04 "		13	7 18 "		23	5 32 "
	4	4 55 "		14	3 09 "		24	11 19 "
	5	10 42 "		15	8 56 "		25	7 10 "
	6	6 33 "		16	4 47 "		26	3 02 "
	7	2 24 "		17	10 34 "		27	8 48 "
	8	8 11 "		18	6 25 "		28	4 40 "
	9	4 02 "		19	2 16 "		29	10 26 "
	10	9 49 "		20	8 03 "		30	6 18 "

Jupiter's Satellites for April.

Phases of the Eclipses of the Satellites for an Inverting Telescope.

I. 

III. 

II. 

IV.  No Eclipse.

Configuration at 7^h for an Inverting Telescope.

Day.	West.		East.
1	O1· 4· ·3 ·2	O	
2	4·	·O3	·1 ·2
3	·4 1·	O	2· ·3
4	·4 2·	O	·1 3·
5	·4 ·1	O	3· ·2●
6	3· ·4	O	·1 2·
7	3· 2· ·1	O	·4
8	·3 ·2	O	1· ·4
9	·3	O	·2 ·4 ·1●
10	1·	O	2· ·3 ·4
11	2·	O	·1 ·3 4·
12	1·	O	3· 4· ·2●
13	3·	O	1· 2· 4·
14	3· 2· ·1	O	4·

Phenomena of Jupiter's Satellites.

Central Time.

	h	m			
Apr. 1	5	54	P. M.	I	Tr. In.
	6	54	"	I	*Sh. In.
	8	08	"	I	*Tr. Eg.
	9	07	"	I	Sh. Eg.
2	2	01	A. M.	II	Oc. Dis.
	6	21	"	II	Ec. Re.
	3	16	P. M.	I	Oc. Dis.
	6	26	"	I	*Ec. Re.
	7	24	"	III	*Oc. Dis.
	9	37	"	III	Oc. Re.
	11	32	"	III	Ec. Dis.
3	1	24	A. M.	III	Ec. Re.
	12	25	P. M.	I	Tr. In.
	1	23	"	I	Sh. In.
	2	38	"	I	Tr. Eg.
	3	36	"	I	Sh. Eg.
	9	14	"	II	Tr. In.
	11	10	"	II	Sh. In.
	11	40	"	II	Tr. Eg.
4	1	34	A. M.	II	Sh. Eg.
	9	46	"	I	Oc. Dis.
	12	54	P. M.	I	Ec. Re.
5	6	55	A. M.	I	Tr. In.
	7	51	"	I	Sh. In.
	9	08	"	I	Tr. Eg.
	10	05	"	I	Sh. Eg.
	3	25	P. M.	II	Oc. Dis.
	7	39	"	II	*Ec. Re.
6	4	16	A. M.	I	Oc. Dis.
	7	23	"	I	Ec. Re.
	9	41	"	III	Tr. In.
	11	54	"	III	Tr. Eg.
	1	31	P. M.	III	Sh. In.
	3	36	"	III	Sh. Eg.
7	1	25	A. M.	I	Tr. In.
	2	20	"	I	Sh. In.
	3	38	"	I	Tr. Eg.
	4	34	"	I	Sh. Eg.
	10	38	"	II	Tr. In.
	12	29	P. M.	II	Sh. In.
	1	04	"	II	Tr. Eg.
	2	53	"	II	Sh. Eg.
	10	46	"	I	Oc. Dis.
8	1	52	A. M.	I	Ec. Re.
	7	55	P. M.	I	*Tr. In.
	8	49	"	I	Sh. In.
	10	09	"	I	Tr. Eg.
	11	03	"	I	Sh. Eg.
9	4	49	A. M.	II	Oc. Dis.
	8	58	"	II	Ec. Re.
	5	16	P. M.	I	Oc. Dis.
	8	21	"	I	*Ec. Re.
	11	48	"	III	Oc. Dis.
10	2	02	A. M.	III	Oc. Re.
	3	33	"	III	Ec. Dis.
	5	26	"	III	Ec. Re.
	2	25	P. M.	I	Tr. In.
	3	18	"	I	Sh. In.
	4	39	"	I	Tr. Eg.
	5	31	"	I	Sh. Eg.
11	12	02	A. M.	II	Tr. In.
Apr. 11	1	47	A. M.	II	Sh. In.
	2	28	"	II	Tr. Eg.
	4	11	"	II	Sh. Eg.
	11	47	"	I	Oc. Dis.
	2	50	P. M.	I	Ec. Re.
12	8	56	A. M.	I	Tr. In.
	9	47	"	I	Sh. In.
	11	09	"	I	Tr. Eg.
	12	00	M.	I	Sh. Eg.
	6	13	P. M.	II	*Oc. Dis.
	10	17	"	II	Ec. Re.
13	6	17	A. M.	I	Oc. Dis.
	9	19	"	I	Ec. Re.
	2	05	P. M.	III	Tr. In.
	4	19	"	III	Tr. Eg
	5	31	"	III	Sh. In.
	7	38	"	III	*Sh. Eg
14	3	26	A. M.	I	Tr. In.
	4	16	"	I	Sh. In.
	5	39	"	I	Tr. Eg.
	6	29	"	I	Sh. Eg.
	1	26	P. M.	II	Tr. In.
	3	05	"	II	Sh. In.
	3	52	"	II	Tr. Eg.
	5	30	"	II	Sh. Eg.
15	12	47	A. M.	I	Oc. Dis.
	3	47	"	I	Ec. Re.
	9	56	P. M.	I	Tr. In.
	10	44	"	I	Sh. In.
16	12	10	A. M.	I	Tr. Eg.
	12	58	"	I	Sh. Eg.
	7	37	"	II	Oc. Dis.
	11	35	"	II	Ec. Re.
	7	18	P. M.	I	*Oc. Dis.
	10	16	"	I	Ec. Re.
17	4	13	A. M.	III	Oc. Dis.
	6	28	"	III	Oc. Re.
	7	34	"	III	Ec. Dis.
	9	28	"	III	Ec. Re.
	4	26	P. M.	I	Tr. In.
	5	13	"	I	Sh. In.
	6	40	"	I	*Tr. Eg.
	7	27	"	I	*Sh. Eg.
18	2	50	A. M.	II	Tr. In.
	4	24	"	II	Sh. In.
	5	17	"	II	Tr. Eg.
	6	48	"	II	Sh. Eg.
	1	48	P. M.	I	Oc. Dis.
	4	45	"	I	Ec. Re.
19	10	57	A. M.	I	Tr. In.
	11	42	"	I	Sh. In.
	1	11	P. M.	I	Tr. Eg.
	1	55	"	I	Sh. Eg.
	9	02	"	II	Oc. Dis.
20	12	54	A. M.	II	Ec. Re.
	8	18	"	I	Oc. Dis.
	11	14	"	I	Ec. Re.
	6	32	P. M.	III	*Tr. In.
	8	48	"	III	Tr. Eg.
	9	34	"	III	Sh. In.
	11	41	"	III	Sh. Eg.

Apr. 21	5	27	A. M.	I	Tr. In.
	6	11	"	I	Sh. In.
	7	41	"	I	Tr. Eg.
	[illegible]	24	"	I	Sh. Eg.
	4	15	P. M.	II	Tr. In.
	5	42	"	II	Sh. In.
	6	41	"	II	*Tr. Eg.
	8	07	"	II	Sh. Eg.
22	2	49	A. M.	I	Oc. Dis.
	5	43	"	I	Ec. Re.
	11	57	P. M.	I	Tr. In.
23	12	39	A. M.	I	Sh. In.
	2	11	"	I	Tr. Eg.
	2	53	"	I	Sh. Eg.
	10	27	"	II	Oc. Dis.
	2	13	P. M.	II	Ec. Re.
	9	19	"	I	Oc. Dis.
24	12	11	A. M.	I	Ec. Re.
	8	40	"	III	Oc. Dis.
	10	55	"	III	Oc. Re.
	11	34	"	III	Ec. Dis.
	1	30	P. M.	III	Ec. Re.
	6	28	"	I	*Tr. In.
	7	08	"	I	*Sh. In.
	8	42	"	I	Tr. Eg.
	9	22	"	I	Sh. Eg.
25	5	39	A. M.	II	Tr. In.
	7	00	"	II	Sh. In.
	8	06	"	II	Tr. Eg.
	9	25	"	II	Sh. Eg.
	[illegible]	49	P. M.	I	Oc. Dis.
Apr. 25	26	40	P. M.	I	*Ec. Re.
26	12	58	"	I	Tr. In.
	1	37	"	I	Sh. In.
	3	12	"	I	Tr. Eg.
	3	50	"	I	Sh. Eg.
	11	51	"	II	Oc. Dis.
27	3	31	A. M	II	Ec. Re.
	10	20	"	I	Oc. Dis.
	1	09	P. M.	I	Ec. Re.
	11	00	"	III	Tr. In.
28	1	16	A. M.	III	Tr. Eg.
	1	34	"	III	Sh. In.
	3	43	"	III	Sh. Eg.
	7	29	"	I	Tr. In.
	8	06	"	I	Sh. In.
	9	43	"	I	Tr. Eg.
	10	19	"	I	Sh. Eg.
	7	04	P. M.	II	*Tr. In.
	8	18	"	II	Sh. In.
	9	31	"	II	Tr. Eg.
	10	43	"	II	Sh. Eg.
29	4	50	A. M.	I	Oc. Dis.
	7	38	"	I	Ec. Re.
30	1	59	"	I	Tr. In.
	2	34	"	I	Sh. In.
	4	13	"	I	Tr. Eg
	4	48	"	I	Sh. Eg.
	1	17	P. M.	II	Oc. Dis.
	4	50	"	II	Ec. Re.
	11	21	"	I	Oc. Dis.
May 1	2	06	A. M.	I	Ec. Re.

NOTE.—In. denotes ingress; Eg., egress; Dis., disappearance; Re., reappearance; Ec., eclipse. Oc. denotes occultation; Tr., transit of the satellite; Sh., transit of the shadow; * Visible at Washington.

Annular Eclipse of the Sun, April 5, 1894.—This will not be visible in the United States. The path of the annular eclipse passes from a point in the Persian Gulf, across Hindostan and China, along the east coast of Siberia, ending in Alaska. It will be visible as a partial eclipse throughout Asia, north-east-

ANNULAR ECLIPSE OF APRIL [illegible]

Elongations of the Satellites of Saturn.

[In the diagram the points marked 0 are those of eastern elongation of the several satellites. Their positions at intervals of one day after eastern elongation are indicated by the symbols 1*d*, 2*d*, etc.]

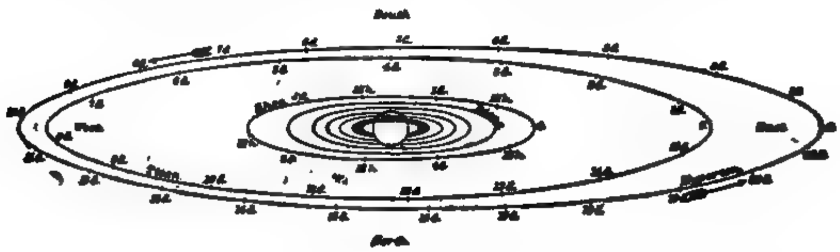

DIAGRAM OF THE APPARENT ORBITS OF SATURN'S SATELLITES.

MIMAS.

		h		
Apr.	4	2.7	A. M.	E
	5	1.3	"	E
	5	11.9	P. M.	E
	6	10.5	"	E
	7	9.1	"	E
	8	7.7	"	E
	9	6.3	"	E
	10	4.9	"	E
	11	3.5	"	E
	12	2.9	A. M.	W
	13	1.5	"	W
	14	12.1	"	W
	14	10.8	P. M.	W
	15	9.4	"	W
	16	8.0	"	W
	17	6.6	"	W
	18	5.2	"	W
	19	3.8	"	W

ENCELADUS CONT.

		h		
Apr.	9	8.7	A. M.	E
	10	5.5	P. M.	E
	12	2.4	A. M.	E
	13	11.3	"	E
	14	8.2	"	E
	16	5.1	A. M.	E
	17	1.9	P. M.	E
	18	10.8	"	E
	20	7.7	A. M.	E
	21	4.6	P. M.	E
	23	1.5	A. M.	E
	24	10.3	"	E
	25	7.2	P. M.	E
	27	4.1	A. M.	E
	28	1.0	P. M.	E
	29	[illegible]	"	E

TETHYS.

Apr.	1	4.7	P. M.	E
	3	2.0	"	E

DIONE CONT.

		h		
Apr.	11	12.9	A. M.	E
	13	6.6	P. M.	E
	16	12.3	"	E
	19	6.0	A. M.	E
	21	11.6	P. M.	E
	24	5.3	"	E
	27	11.0	A. M.	E
	30	4.7	"	E

RHEA.

Apr.	3	12.6	A. M.	E
	7	1.0	P. M.	E
	12	1.4	A. M.	E
	16	1.7	P. M.	E
	21	2.1	A. M.	E
	25	2.5	P. M.	E
	30	2.9	A. M.	E

TITAN.

Apr.	1	5.4	A. M.
	5	8.4	"

Minima of Variable Stars of the Algol Type.

[Given to the nearest hour in Central Standard Time.]

U CEPHEI.

R. A. .... $0^h\ 52^m\ 32^s$
Decl. .... $+81°\ 17'$
Period .... $2d\ 11^h\ 50^m$

Apr. 2	5 A. M.
4	4 P. M.
7	4 A. M.
9	4 P. M.
12	4 A. M.
14	4 P. M.
17	3 A. M.
19	3 P. M.
22	3 A. M.
24	3 P. M.
27	3 A. M.
29	3 P. M.

ALGOL.

R. A. .... $3^h\ 1^m\ 1^s$
Decl. .... $+40°\ 32'$
Period .... $2d\ 20^h\ 49^m$

Apr. 3	8 P. M.
6	5 "
9	2 "
12	11 A. M.
15	8 "
18	4 "
21	1 "
23	10 P. M.
26	7 "

R CANIS MAJORIS.

R. A. .... $7^h\ 14^m\ 30^s$
Decl. .... $-16°\ 11'$
Period .... $1d\ 3^h\ 16^m$

Apr. 1	2 P. M.
2	5 "
3	9 "
4	12 midn.
6	3 A. M.
7	6 "
8	10 "
9	1 P. M.
10	4 "
11	8 "
12	11 "
14	2 A. M.
15	5 "
16	8 "
17	12 noon.
18	3 P. M.
19	6 "
20	10 "
22	1 A. M.
23	4 "
24	7 "
25	11 "
26	2 P. M.
27	5 "
28	8 "
29	12 midn.

S CANCRI.

R. A. .... $8^h\ 37^m\ 39^s$
Decl. .... $+19°\ 26'$
Period .... $9d\ 11^h\ 38^m$

Apr. 3	12 midn.
13	12 noon.
23	12 midn.

S ANTLIÆ.

(Every third minimum.)

R. A. .... $9^h\ 27^m\ 30^s$
Decl. .... $-28°\ 09'$
Period .... $0d\ 7^h\ 47^m$

Apr. 1	8 P. M.
2	7 "
3	7 "
4	6 "
5	5 "
6	4 "
7	4 "
8	3 "
9	2 "
10	2 "
11	1 "
12	12 noon.
13	12 noon.
14	10 A. M.
15	4 "
16	10 "
17	9 "
18	9 "
19	8 "
20	7 "
21	7 "
22	6 "
23	5 "
24	5 "
25	4 "
26	3 "
27	3 "
28	2 "
29	1 "
30	1 "

δ LIBRÆ.

R. A. .... $14^h\ 55^m\ 06^s$
Decl. .... $-\ 8°\ 05'$
Period .... $2d\ 07^h\ 51^m$

Apr. 2	7 P. M.
5	3 A. M.
7	11 "
9	7 P. M.
12	3 A. M.
14	10 "
16	6 P. M.
19	2 A. M.
21	10 "
23	6 P. M.
26	2 A. M.
28	10 "
30	6 P. M.

U CORONÆ.

R. A. .... $15^h\ 13^m\ 43^s$
Decl. .... $+32°\ 03'$
Period .... $3d\ 10^h\ 51^m$

Apr. 4	3 A. M.
7	2 P. M.
11	1 A. M.
14	12 noon
17	10 P. M.
21	9 A. M.
24	8 P. M.
28	7 A. M.

U OPHIUCHI.

R. A. .... $17^h\ 10^m\ 56^s$
Decl. .... $+\ 1°\ 20'$
Period .... $0d\ 20^h\ 08^m$

Apr. 1	6 A. M.
2	2 "
2	10 P. M.
3	6 "
4	1 "
5	11 A. M.
6	7 "
7	3 "
7	11 P. M.
8	8 "
9	4 "
10	12 noon
11	8 A. M.
12	4 "
12	12 midn.
13	8 P. M.
14	4 "
15	1 "
16	9 A. M.
17	5 "
18	1 "
18	9 P. M.
19	5 "
20	1 "
21	9 A. M.
21	6 "
23	2 "
23	10 P. M.
24	6 "
25	2 "
26	10 A. M.
27	6 "
28	2 "
28	11 P. M.
29	7 "
30	3 "

Y CYGNI.

R. A. .... $20^h\ 47^m\ 40^s$
Decl. .... $+34°\ 15'$
Period .... $1d\ 11^h\ 57^m$

Apr. 1	1 P. M.
3	1 A. M.
4	1 P. M.

Y CYGNI CONT.		Y CYGNI CONT.		Y CYGNI CONT.	
Apr. 6	1 A. M.	Apr. 15	1 A. M.	Apr. 24	1 A. M.
7	1 P. M.	16	1 P. M.	25	1 P. M.
9	1 A. M.	18	1 A. M.	27	1 A. M.
10	1 P. M.	19	1 P. M.	28	1 P. M.
12	1 A. M.	21	1 A. M.	30	1 A. M.
13	1 P. M.	22	1 P. M.		

Phases and Aspects of the Moon.

	Central Time.				
		d	h	m	
New Moon	Apr.	5	10	00	P. M.
Perigee	"	10	9	40	P. M.
First Quarter	"	12	6	32	P. M.
Full Moon	"	19	9	02	P. M.
Apogee	"	26	1	55	A. M.
Last Quarter	"	27	9	21	P. M.

Occultations Visible at Washington.

Date 1894.	Star's Name.	Magnitude.	IMMERSION Washington M. T.		IMMERSION Angle f'm N pt.	EMERSION Washington M. T.		EMERSION Angle f'm N pt.	Duration.	
			h	m	°	h	m	°	h	m
Apr. 8	9 Tauri	7	8	17	74	9	12	266	0	55
10	136 Tauri	5	12	35	160	12	55	209	0	20
12	ω^2 Cancri	6	12	57	65	13	22	333	0	25

Maxima and Minima of Variable Stars.

MAXIMA			MINIMA		
April	1	R Herculis	April	5	X Bootis
	2	U Virginis		5	W Scorpii
	10	R Arietis		6	S Cygni
	11	V Leonis		13	R Canum Ven.
	12	X Scorpii		15	U Monocerotis
	14	V Capricorni		15	U Bootis
	16	L^2 Puppis		18	S Ursæ Maj.
	18	V Capricorni		19	R Lyræ
	20	U Capricorni		23	R Ursæ Maj.

[redacted]

cal with (89) Julia. I took every precaution in order to avoid similar errors in computing the dates of oppositions for 1894, but it is impossible to vouch for the exactness of all of 240 ephemerides, only 60 of which are computed in duplicate.

"The later planets of 1893 receive the following numbers.

		Discovered by	
1893	AJ = 373	Charlois	Sept. 15
	AK = 374	"	Sept. 18
	AL = 375	"	Sept. 18
	AM = 376	"	Sept. 18
	AN = 377	"	Sept. 20
	AP = 378	"	Dec. 6

"The name 'Chicago,' chosen at the astronomical Congress for a minor planet, has been conferred by Professor M. Wolf upon the planet (334). This planet presents a good means of determining the mass of Jupiter. It undergoes very remarkable perturbations by that great planet. The planets (153) Hilda, (190) Ismene, (279) Thule and (361) are also exposed to considerable perturbations, but only in future times. A conjunction of Hilda and Jupiter some years ago coincided with the perihelion passage of Hilda so that the distance of the two bodies has been at a maximum.

"I find that the Watson planet (175) Andromache, rediscovered in 1893 by Charlois and photographed in 1892 by Wolf, was in proximity to Jupiter in 1886-87, being near aphelion and moving very slowly. As the period of (175) is nearly one-half of that of Jupiter that conjunction will be repeated every 11-12 years, in about the same position. The effect will be a retardation of the mean motion, probably of large amount. I think the moderately distant planets are of greater value for the determination of Jupiter's mass than those very remote, like Hilda, since the conjunctions with Jupiter are much more frequent. Thus in one century we may observe 8 conjunctions of Andromache but only 4 of Hilda, and scarcely 3 of Thule, with Jupiter."

Ephemeris of Comet *c* 1893 (Brooks).—From Dr. Krueger's elements I have computed the following ephemeris of Comet Brooks.

G. M. T.		App. R. A.			App. Dec.		Log r	Log Δ
1894		h	m	s	°	′		
Mar	1.5	0	57	0	+55	19	0.4318	0.4648
	2.5	0	58	48	55	11		
	3.5	1	0	30	55	3		
	4.5	1	2	10	54	54		
	5.5	1	3	49	54	46	0.4398	0.4795
	6.5	1	[illegible]	26	54	38		
	7.5	1	7	3	54	31		
	8.5	1	8	39	54	24		
	9.5	1	10	14	54	17	0.4477	0.4936
	10.5	1	11	47	54	11		
	11.5	1	13	20	54	6		
	12.5	1	14	53	54	0		
	13.5	1	16	26	53	54	0.4551	0.5066
	14.5	1	18	2	53	50		
	15.5	1	19	37	53	46		
	16.5	1	21	8	53	42		
	17.5	1	22	38	53	38	0.4600	0.5108
	18.5	1	24	2	53	29		
	19.5	1	25	22	53	21		
	20.5	1	26	44	[illegible]	13		

G. M. T.	App. R. A.			App. Dec.		Log r	Log Δ
1894	h	m	s	°	′		
21.5	1	28	6	53	5	0.4695	0.5290
22.5	1	29	24	52	55		
23.5	1	30	43	52	42		
24.5	1	32	11	52	31		
25.5	1	33	38	52	19	0.4765	0.5348
26.5	1	35	0	52	9		
27.5	1	36	21	51	58		
28.5	1	37	43	51	48		
29.5	1	39	2	51	37	0.4834	0.5403
30.5	1	40	21	51	27		
31.5	1	41	39	+51	17		

O. C. WENDELL.

Harvard College Observatory, Feb. 14, 1894.

Brilliant Meteor.—A large ball of fire supposed to be a meteor was seen here February 16th, 7:45 P. M., standard time. The strange sight was witnessed by a great many people all agreeing that themeteor appeared to start in the western heavens, near the horizon and traveled in a northwesterly direction until it disappeared.

Some observers claim that the ball after travelling some distance broke, one piece going toward the ground, the other continuing in a straight course for several seconds. I was not fortunate enough myself to witness the display, consequently cannot vouch for the direction and duration of flight, but must be content with the testimony of others. CHAS. E. MYERS.

Canton, Ohio, Feb. 17th, 1894.

NEWS AND NOTES.

Dr. See's Researches on the Orbit of α Centauri.—The *Monthly Notices* of the Royal Astronomical Society for December contains a new and important investigation of the orbit of α Centauri by Dr. T. J. J. See, who has collected from original sources all the observations from the earliest times to the present date,

APPARENT ORBIT.

Length of major axis	$= 32''.50$
Length of minor axis	$= 6\ .16$
Angle of major axis	$= 27^\circ.2$
Angle of periastron	$= 38\ .0$
Distance of star from centre	$= 5''.94$

A beautiful illustration of the apparent orbit accompanies the paper, and the author also gives a comparison of the computed and observed places, which shows a very good agreement. Dr. See finds by means of his elements and the parallax of Gill and Elkin ($0''.75$) that the semi major axis of the orbit of α Centauri is 23.592 astronomical units, so that the companion moves in an orbit which is about a mean between those of the planets Uranus and Neptune, but the eccentricity is so high that in periastron the distance (11.3) but little surpasses that of Saturn, while in apastron it considerably surpasses the distance of Neptune from the Sun, becoming 36.0 astronomical units. The author finds the combined mass of the two components of α Centauri to be 1.998 times the mass of the Sun and Earth, and calls attention to the desirability of determining the relative masses of the two components, for the light the result would throw upon the cosmogony of double-stars.

Dr. See expresses his warm thanks to the observers in the Southern hemisphere—to Messrs. Tebbutt, Pickering, Douglass, Russell, Sellors, Gill and Finlay—for securing sets of measures expressly for the present research; also to Herr Lundendorff of Berlin, and to Professor Burnham and Mr. Foley of Chicago for kindly assistance in the investigation. In conclusion he refers to the recent elements by Mr. A. W. Roberts in the *Astronomische Nachrichten*, No. 3175, which are:

$$
\begin{aligned}
T &= 1875.715 \\
P &= 81.185 \text{ years} \\
e &= 0.52865 \\
\lambda &= 52^\circ\ 0'\ 58'' \\
i &= 79\ \ 21\ \ 36 \\
\omega &= 25\ \ \ 5\ \ 50\ (1900) \\
a &= 17''.71
\end{aligned}
$$

It is thus seen that Dr. See and Mr. Roberts, working independently, by different methods, have arrived at elements which are very nearly the same, and hence there can hardly be a doubt but that the orbit of α Centauri, which has been hitherto uncertain (the periods obtained ranging from 77 to 88 years), is now the best determined of any known double star. If any correction to Dr. See's elements should in the course of time be found desirable, it seems that they would of necessity be very small.

The Solar Image Reflected in the Seas of Mars.—M. Flammarion (La Planete Mars, p. 220) speaks of the possibility of seeing the image of the Sun reflected in the Seas of Mars. He estimates that in favorable circumstances it might appear like a star of the 3rd magnitude, but he adds that this implies that the surface of the sea should be as calm as a mirror. I have gone over M. Flammarion's calculations. It seems to me that the allowance he has made for absorption of light in its double passage through the Martian atmosphere, is probably insufficient, and that it would more likely be of the 4th magnitude.

What he says with regard to the calmness of the sea, is of course true, but the area of sea concerned in forming the image would only be a circle of about 7 miles diameter, and remembering that the winds of Mars are probably much weaker than ours, it does not seem so unlikely that this extent may be sometimes calm.

It seems to me that the reason this image has not hitherto been observed, is not improbably because it has not been systematically and properly looked for. We must remember that at a given instant it can only be formed at a particular point of the disc, and if at this instant this point is occupied by a portion of the surface incapable of specular reflection, no image can be formed.

Now only a very small portion of the surface appears to be so capable. The land, of course, is not; and if Professor Pickering is right, the majority of the so-called seas are land. I think it is more likely that the areas of greenish tint, generally called sea, but regarded as land by Professor Pickering, are really sea, but very shallow and choked up with rocks and small islands. In this case also they would be unfit to form an image.

So, I think, we could only expect it in the two areas regarded as sea by Professor Pickering, viz., Herschel II Strait, and part of the Hour-glass Sea.

The time when an image could be expected in these seas could be easily calculated beforehand, and it might then be looked for.

To prevent the brilliancy of the rest of the disc from overpowering the light, I would suggest that a diaphragm with a small hole in it be employed.

Obviously the point of the disc where it should be formed, is exactly midway between the centre of the disc and the centre of the illuminated hemisphere. In itself, this observation is not very important, but it would definitely settle the question whether the bright or dark markings are seas.

Of course, if the sea was *perfectly* calm, this image should be small, regular and starlike. And, in that case, it could hardly have escaped notice, even if considerably below the 4th magnitude; but evidently *perfect* calmness must be unusual. With waves following one another regularly, it would be elongated into a streak little more than 7 miles wide, and 100 or more miles long. With irregular ripples, it would appear nebulous, but even then it might be visible if properly looked for.

I have made a rough calculation, with the result that I doubt if this can be effectively tested at the next opposition. Throughout this, the Earth, as seen from Mars, will have a high southerly declination, and the Sun being far south, also, the spot on the disc where the image could be formed will be much too far south for either of the seas in question. It is not till the Sun nears the equator, that it can fall even in Herschel II Strait, and this will not be till nearly the end of January, when the planet's distance from us will be considerable and the image if formed, correspondingly feeble.

During the two months before and after the opposition, the locus will traverse a belt crossing Maraldi, Hooke and Terby Seas.

It might, perhaps, be worth while to look for the reflection in those Seas, on the chance that they *may* be water. J. R. HOLT.

6 Harrington St., Dublin, Ireland.

The Constant of Aberration.—In Bulletin No. 28 of the U. S. C. and G. Survey, Mr. Preston gives a detailed discussion of the observations made at Waikiki, H. I., by which a correction is sought for the commonly accepted values of the constant of aberration (20″.445). Mr. Preston says in closing that the defini-

tive result of the constant of aberration from the latitude observations of 1891-1892, made at Waikiki, Hawaiian Islands, on the part of the United States Coast and Geodetic Survey, is therefore

$$\text{Constant of aberration} = 20''.433 \pm 0''.034.$$

This value of the aberration constant, combined with the latest determinations of the velocity of light (V = 186,330 miles) and Clarke's value for the Earth's radius (R = 3963.30 miles), gives the Sun's distance and equatorial horizontal parallax as follows:

$$\text{Distance} = 92,709,000 \text{ miles}.$$
$$\text{Parallax} = 8''.82.$$

H. A. S. *in March Am. Jour. Science.*

The Dawn of Astronomy.—Mr. J. Norman Lockyer has just written a book entitled, *The Dawn of Astronomy.* It is published by Messrs. Macmillan & Company, London and New York, in neat and attractive form, with large page, clean type, wide margin, very heavy paper, 121 fine illustrations and a contents of 425 pages.

During the last three years the author has been industriously gathering facts and making a study of them that pertain especially to the temple worship and the mythology of the early Egyptians, in order to learn as much as possible about their astronomical views. He was led to this study, by an incident while on a visit to the ruins of the Parthenon in 1890. The curious direction in which the Parthenon was built and also the many changes in direction in the foundations at Eleusis revealed by French excavations were facts that awakened interest and suggested the query, whether or not, the choice of these various directions in which ancient temples should face, had its origin in important astronomical facts. If so what were the directions and what the celestial objects to which they were dedicated?

Upon inquiry it was learned that but scanty information could be found to answer such questions in relation to the ancient temples, for scarcely anything of the kind was known even in regard to churches outside of England and Germany.

However later it was learned that Professor Nissen of Germany had published some papers on the orientation of the ancient Egyptian temples, and these with needed information given by archeological friends opened the way for future work of an important kind. These ancient temple sites should be accurately surveyed; astronomers should furnish tables of the places of many stars reaching back 7,000 B. C., and the Egyptologist should read inscriptions for suggestions. In these three different lines of study there seemed to be in the outset much of promise and considerable has already been realized as is shown in the contents of this book.

The author begins the first chapter with the worship of the Sun and the Dawn by the first civilizations which archeologists place in the Nile valley and adjacent countries in western Asia. Two other civilizations of later time and different type were India and China, with paper records but no monuments of high antiquity, for, on the authority of Max Müller we must believe that the known temples of India are relatively of modern origin.

If we can go back in China's and India's history 4000 years and by Babylonian tablets 5000 years, those of Egypt, as estimated by various authors, will carry us back 6000 or 7000 years. So it is well, if possible, to get first glimpses of Egyptian Astronomy and these are shown by the Rosetta Stone, the temples of Edfû and Denderah with its circular zodiac, and the Egyptian Pantheon and

the tablet of kings at Abydos, and through these and numerous others like them to learn of the early worship and the study of the stars accompanying it. Chapters four, five and six explain, in popular way, elemental ideas of astronomy, and chapter seven speaks of the methods of determining the orientation of temples, giving illustrations of instruments used, maps of magnetic variation. The earliest solar shrines of Egypt are fully illustrated and the principal ones are those at Abydos, Ann, Collossi on the plains of Thebes, Gizeh and one near the Sphinx. Other similar shrines elsewhere are also described, such as the Pekin Sun temple, the temple at Jerusalem, St. Peter's at Rome and that at Karnak.

Next is considered the rising and setting of the stars, the Egyptian heavens, and more fully the zodiacs of Denderah, the circumpolar constellations and the myth of Horus.

In the fifteenth chapter the temples dedicated to the stars are presented, and further inquiries made concerning them and the inscriptions found in connection with the ruins, followed by a special study made of the star temple at Karnak. Later chapters of special interest are those concerning the Egyptian year and the Nile, the Euphrates gods and the Nile, the vague and Syrian years, the calendar and its revision, the fixed year and festival calendars, the mythology of Isis and Osiris, the early temple and great pyramid builders, north and south star temples, the origin of Egyptian astronomy under the name of the northern schools, and also under the Thebes school, general conclusions as to the north and south races, the Egyptian and Babylonian ecliptic constellations, and the influence of Egypt upon temple orientation in Greece.

This important book opens a new field for work in the early history of astronomy.

Photographic Chart of the Sky.—Vol. II, part II, of the "*Bulletin du Comite international permanent*," contains important memoirs by Messrs. Kapteyn and Loewy, the former on "Systematic difference between the photographic and visual magnitudes of stars in different regions of the sky," the latter a second paper on "Construction of the catalogue based upon the plates for the chart of the sky." There is also a description by Prosper Henry of the method of measurement and reduction of photographs at the Paris Observatory.

In the "correspondence" we notice several complaints in regard to lack of permanence in the *reseau*. It is liable to scratches and pin-holes in a very short time. Several observers suggested that the réseau be omitted from the plates for the chart although it should be retained upon the plates for the catalogue.

As to the progress of the work, the following report is given of the number of plates which have been obtained for the catalogue and for the chart:

	For Catalogue.	For Chart.
Paris	397	137
M. Baillaud	140	173
" Rayet	164	21
" Trépied	570	64
" Denza	49	20
" Russell	300	
" Donner	120	

H. C. W.

The Rings of Saturn.—It is not often that we hear of a woman receiving the degree of Doctor of Sciences, especially in the line of mathematics. That degree has recently been conferred by the University of Paris upon Miss Dorothea

Klumpke, a young American woman, of San Francisco, who a few years ago obtained admission to the Observatory of Paris, and has since been engaged there in study and astronomical investigation. She was for a time in charge of the great reflector, but is now in charge of the Bureau of Measurements, which has for its work the measurement of the positions of stars upon the photographic plates, which are being taken for the purpose of making a catalogue of all stars down to the 11th magnitude.

For her thesis Miss Klumpke chose the subject of the form of Saturn's rings. She gives a brief history of the discovery of the rings, theories as to their constitution and study of their equilibrium. She herself takes up the problem of the figure of a fluid ring, or a solid ring covered with fluid, in equilibrium around Saturn. This problem was first treated by La Place, later by Mdme. Kowalewski and M. Tisserand. Mdme Kowalewski considered the figure of the ring to be generated by the revolution of a plane curve about an axis passing through the center of Saturn and perpendicular to the plane of the curve's motion. Miss Klumpke carries on her investigation by including terms of a higher order and shows that their effect is very slight. She obtains for the equations of the cross-section of the ring

$$y = -\cos t, \quad x = 0.385 \sin t - 0.188\ \sigma\ \sin 2t$$

in which t is a variable parameter and σ is the ratio of the semi-major axis of the cross-section to the distance of its center from the centre of Saturn.

In the second part she investigates the simpler hypothetical case, in which the mass of Saturn is considered to be zero, the rings being subject only to the centrifugal force of its motion and the mutual attraction of its molecules. A first approximation including only the second powers of σ gives the figure of cross-section of the ring as a circle. Including terms in σ^3 the figure becomes an ellipse, and when the terms involving σ^4 are considered the cross-section becomes egg shaped with the small end inward. H. C. W.

Bibliography of Astronomy.—Works published during January, 1894. Compiled by W. Wesley and Son, 28 Essex Street, Strand, London, England, who will also supply any of the works, if desired.

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Guyon (E.) et H. Willotte. Cours elementaire d'astronomie, with 170 engravings and 2 plates. 8vo. Paris, 1894. $2.25.

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Lockyer (J. N.) The dawn of astronomy, a study of the temple-worship and mythology of the ancient Egyptians, 121 plates and engravings. Royal 8vo., cloth. 1894. $5.25.

Mayer (T). Sternverzeichniss. Nach den Beobachtungen auf der Goettinger Sternwarte, 1756-60. Neu bearbeitet von A. Auwers. 4to. Leipzig, 1894, $5.75.

Poynting (J. H.) The mean density of the Earth, 7 plates and engravings. 8vo., cloth. 1894. $3.15.

Proctor (R. A). The expanse of Heaven: a series of essays on the wonders of the firmament. New edition, post 8vo., cloth. 1894. 90 cts.

Astronomical and Physical Society of Toronto, Canada.—Meeting of Jan 23, 1894, chair, Dr. Larratt W. Smith, Q. C., vice president. He thanked the members for his recent re-election.

Mr C H. Collison, B A., of Upper Canada College, was elected an active member. Honorary membership was conferred upon Sir Robert S Ball, LL. D., Lowndean Professor of Astronomy, King's College, Cambridge, and director of the University Observatory; upon Geo. H. Darwin, M. A., Plumian Professor of Astronomy, Trinity College, Cambridge; upon Professor H. C. Vogel, director of the Astro-Physical Observatory, Potsdam, Germany. Professors C. H McLeod, Ma E., etc., Director of McGill College Observatory, Montreal; W. A. Rogers, Colby University, Waterville, Me , Edward W. Maunder, F. R A S., First-Class Physical Assistant, Greenwich Observatory, Rev. T E Espin, F R A S, Director of Tow Law Observatory, Darlington, Eng., were elected corresponding members.

Librarian G. G Pursey reported receipt of publications including those of the Royal Society and one presented by Mr. A Aaronsberg, Toronto.

Observations on the Sun, on the planets and on the zodiacal light were reported by several members. Dr. A. D. Watson referred to Saturn as being especially beautiful, his rings having opened out farther than has been observable for years. With but a three-inch telescope, he had seen Cassini's division.

Most of the evening was occupied reading and discussing these five-minute papers: "Jupiter's Atmosphere," Mr R. B. Ellis; "The distances of Jupiter's Satellites" (with diagram), Mr. A. Elvins, "The Ellipticity of the Orbits of Jupiter's Satellites," Mr. A. Harvey, "Certain Phenomena Connected with Satellites I and II." Dr. A. D. Watson; "The Mythology of Jupiter," Mr. W. B. Musson; "The Density of Jupiter," Mr. T. Lindsay. None of the papers presumed to be original, but all were clever.

Meeting of Feb. 6. Mr. Arthur Harvey presided. Honorary membership conferred upon M. Otto Struve of St. Petersburg; corresponding membership conferred upon M. Paul Henry, elder brother of the Henry brothers of Paris Observatory, France.

Upon the subject of changing astronomical time to begin at midnight instead of at noon, a letter was read from Mr. Sanford Fleming, C M. G., recently returned to Ottawa after a 24,000-miles trip on the sea. He had conferred with captains and other officers, and all were agreed that the change should be made. In answer to the circulars sent out there had been received two hundred answers, 152 of which were for the change and 48 in opposition to it.

Mr. Andrew Elvins reported some excellent observations of Venus. She showed a thin crescent, gapped along the terminator. Mr. Elvins had also seen the belts of Jupiter very plainly. It was stated that an observer in Barbados had seen the dark portion of the disc of Venus, similar to the phenomenon which appears at a new moon.

Mr. Joseph W. Allen, father of Grant Allen, the famous novelist and scientist, read the paper of the evening, which was a defence of his son's book, "Force and Energy."

JOHN A COPLAND

The Chicago Academy of Sciences, Section of Mathematics and Astronomy, Feb. 6th.—Professor G. W. Hough, President, in the chair. The Recorder announced that there was no business before the Section requiring special action, and the meeting at once proceeded to the program of the the evening. Professor G. W. Hough read the first paper on the "*Evolution of the telescope.*" In tracing the

history of lenses from Roman times to the present date, the speaker remarked that the earliest mention of anything that could be called a lens, is that of a "burning sphere" by Aristophanes, 424 B. C., and he considered the next record that of the astronomer Cleomedes who observed that a bottle magnifies objects seen through it. For the next 1000 years very little was done in optics, but the Arabian astronomers had given some attention to the subject about the time of Ibn Jounis, 1000 A. D.

In modern times the study of lenses begins with Roger Bacon, who used them for spectacles. When lenses were in general use for aiding the vision, of course a very slight step would give a magnifying glass. In 1608 Lippershey, the Dutch spectacle-maker, constructed a telescope which was applied to terrestrial objects, and Galileo soon afterwards invented the astronomical telescope, which created an epoch in the history of science. The speaker gave an interesting account of Galileo's observations of the mountains on the Moon, the spots on the Sun, the Satellites of Jupiter, and the Rings of Saturn; and after sketching the history of the "long" telescopes of the next century, and describing the method of using a telescope without a tube, he came down to the reflector invented by Gregory in 1663, and preferred by Newton on account of the imperfect definition of refractors arising from chromatic aberration. Professor Hough pointed out the importance of Dolland's services in using two kinds of glass with different refractive indices, and called attention to the problem of securing large homogeneous pieces of glass, which was finally solved by Frauenhofer, but which long retarded the use of the refractor.

The speaker then projected on the screen illustrations of most of the celebrated modern telescopes, beginning with the great reflector of Sir Wm. Herschel and concluding with the Yerkes 40-inch refractor. In reviewing recent progress in telescope making the speaker pointed out the unexampled services of Alvan Clark & Sons, whose first large telescope was the 18 inch Dearborn refractor at Evanston, with which he and Professor Burnham have discovered and measured so many close double stars. In conclusion Professor Hough remarked that refractors were preferred by American astronomers, but that for some purposes the reflectors have their advantages, and they are still used in England and France. A general discussion followed, in which all the observers present took part.

Dr. Henry Crew of Northwestern University read the second paper of the evening on "*Refraction at the Eye-end of the telescope*." Professor Crew defined the telescope as an instrument for transforming a plane wave surface into a spherical one. He then interpreted Airy's equation for the distribution of light in the field, and pointed out the limit which the finite value of the light waves sets to the defining power of any aperture. Various methods were shown for improving the definition, but in all cases a practical difficulty was encountered in loss of light.

Attention was called to Professor Michelson's extended definition of "defining power" and to some of the possibilities of the refractometer in astronomical work. At the close of the session a number of diffraction patterns resulting from apertures of various forms were shown by means of a small telescope and an artificial star. Professor Burnham, Dr. See and several others took part in the discussion of Dr. Crew's interesting paper, after which the Section adjourned.

T. J. J. SEE, Recorder.

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James E. Keeler, Observatory, Allegheny, Pa.; Henry Crew, Northwestern University, Evanston, Ill., Jos. S. Ames, Johns Hopkins University, Baltimore, Md.

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CONTENTS FOR FEBRUARY, 1894.

APPARATUS FOR ELECTRICAL CONTROL OF TELESCOPE.

Astronomy and Astro-Physics.

VOL. XIII, No. 4. APRIL, 1894 WHOLE No. 124.

ELECTRIC CONTROLS AND GOVERNORS FOR ASTRONOMICAL INSTRUMENTS.*

F. L. O. WADSWORTH.

In the January number of ASTRONOMY AND ASTRO-PHYSICS, in an interesting article on Telescope Mountings and Domes, Professor Pickering suggests the use of reversible electric motors for actuating each of the slow motion screws of the telescope. This plan has been used over a year in the control of the large siderostat of the Astro-Physical Observatory, and it has proved so thoroughly convenient and satisfactory, that a brief description of the mechanical and electrical features may be of interest.

The instrument in question has already been described in some detail by the maker.†

Briefly it is of the Foucault type similar to the one used by Professor Langley at Allegheny, but of much larger size, being intended to carry a mirror 20 inches in diameter. In order to overcome as far as possible the difficulties inherent in this form of instrument, all of the driving parts were made very heavy and stiff and the clock-work is unusually powerful.

The governor is of the usual Grubb type and was originally supplemented by a very ingenious but elaborate electrical device for controlling the rate of rotation of the driving shaft, by a free seconds pendulum, through the medium of two mouse controls, one arranged to slowly accelerate, the other to slowly retard the motion of the shaft in question. In addition to this, another mouse control, on the same shaft was arranged to be operated, by the observer, to further accelerate or retard the motion, of the clock at will. Slow motions were also provided for the motion in declination and in altitude and azimuth, all of these slow motions (eight in all) being actuated mechanically by means of cords or long rods running from the instrument to the observer who was inside the Observatory and nearly fifty feet from the instrument.

This system of mechanical controls had always been unsatisfactory because of the number of cords and rods involved and the

* Communicated by the author.

† See article by Grubb in Engineering, Vol. XLIX, p. 592.

limitations imposed by their use on the position of the observer while making these adjustments.

Various plans had been, from time to time, proposed by Professor Langley, Dr. Hallock and others connected with the Observatory, for replacing them with a form of electric control which, by pressing a key on a small moveable switch-board, would enable any one of the slow motion mechanisms to be operated in either direction from an electric motor. The advantages of this method of control are obvious but the mechanism necessary to carry out the proposed plans was so complex, and the expense and liability to get out of order so great, that they had never been practically adopted. When, however, the idea of using a single motor was abandoned and the plan referred to at the beginning of this article, *i. e.*, that of driving each slow motion independently by means of a small reversible motor, which in the case of the altitude and declination slow motions could be placed directly on the moving parts to which the screws for these motions were attached, was proposed by the author, the mechanical difficulties at once disappeared and the electrical features were considerably simplified.

The motors required were all so small that the additional expense involved by the use of four instead of one was inconsiderable, and more than compensated for, in the simplification in the mechanical work alone. It was found also that this plan involved the use of a smaller number of wires than any before proposed, the whole eight motions, *viz.*, forward and backward in right ascension, declination, altitude and azimuth requiring but five wires running from the switch-board by the observer to the instrument.[*]

The general arrangement of the electrical connections is that shown in Figure 1, where *s* is the switch-board (here considerably enlarged in relation to the other parts for the sake of clearness, but in reality so small and compact, that it may readily be carried in the observer's hand if desired), provided with four reversing keys, each of which controls one of the four double motions.

They are so arranged that the motions of the contact lever handle in either direction from the center, sends the reflected beams from the siderostat in the same direction, so that they may be manipulated readily in the dark without confusion or error. Any ordinary form of reversing key may be used of

† The electric control supplied by the maker required 10 wires for the control in right ascension alone.

course, but the one shown is particularly simple and easy of construction.

It consists simply, of two brass springs, *a*, *b*, screwed to an insulating handle pivoted midway between them, and four brass contact buttons, 1, 2, 3, 4, diagonally opposite buttons being attached to the two battery and motor terminals.

The motors could be reversed either by reversing the polarity of the fields, or by reversing the current in the armature, but for certain practical reasons the latter plan is preferable. The polarity of the field could be most easily kept constant, as the motors are all very small, by using permanent steel magnets, but for reasons of economy commercial motors with electro magnetic fields were used, and their field coils connected in series with an independent battery circuit, which was closed automatically by a relay R in the main circuit,—which dispenses with the necessity of running two additional wires to the switch-board.

In the first, of the two plates, which accompanies this article, the two motors which operate the mouse controls in right ascension and declination are shown. As will be seen they are directly connected by belts to two mouse controls, one on the driving shaft of the clock; the other on a shaft which moves the declination axis.* Those which control the altitude and azimuth slow motion have not yet been put in place, as these adjustments are only used for special work.

The electric control, supplied by the maker, a portion of which is shown at the right of the lower motor, has now been entirely discarded, as too complicated and liable to derangement.

Its place is fully supplied by the use of the motor, whose speed of rotation and therefore of regulation, can be adjusted to a nicety by means of the rheostat M, in the motor circuit.

This makes it possible to either keep the image fixed on the cross wires or to give it any desired rate of motion forward or backward or up and down in the field without altering the rate of the clock, an advantage of considerable importance in certain kinds of spectroscopic work.

Other advantages are the smoothness and regularity of the motion, which can be instantly checked, even when very rapid, by a momentary reversal of the motor, and the ease with which the motion can be controlled from parts of the laboratory to which it would be difficult if not impossible to carry the mechanical controls either because of the distance or on account of intervening apparatus.

* I agree with Professor Pickering in considering the mouse control decidedly superior to the usual worm and worm-wheel, or slow motion screw.

The only trouble which was at first experienced was the effect of the motor circuits on the delicate galvanometer used in the Observatory, but this has been overcome, by careful twisting of the wires together and the relocation of the batteries and rheostat.

The use of a motor in place of the usual clock as the driving power for not only equatorials, but also for siderostats, chronographs, and in fact all instruments where uniformity of rotation is desired, is something which the writer has long advocated and adopted whenever possible.

In these days when storage batteries form an almost necessary part of the equipment of every laboratory, mechanical driving clocks could be replaced with advantage in almost every instance by suitably designed electric motors, but on account of the unfamiliarity of most of our leading instrument-makers with the correct principles of design, and the unsuitability of most of the commercial motors in the market, it will probably be a long time before we can hope to see this change made.

The points which have to be considered in the general design of an electric motor which it is desired to have run with the greatest possible regularity and constancy are:

1. The motor should be much larger than is actually required to do the work, so that the load upon it may be at all times very light, and its counter electro-motive force therefore very nearly equal to the difference of potential between the terminals.

2. The motor should be compound wound with a powerful field worked considerably below the saturation point to secure a sensitive regulation, both for changes of load and slight variations on *E M F* at the terminals.

3. The armature should be of the drum type, with wires either buried in slots and covered with a wrapping of fine iron binding wire, or better, passed through holes near the outer circumference of the armature as in the Wenstrom dynamo type. It should of course be very perfectly balanced, and if not of considerable weight itself should have a heavy flywheel. The commutator should be of large size, with many segments, and together with the brushes should be kept scrupulously clean and free from oil and dust.

4. The electromotive force should be as high as possible both to minimize the effects due to changes in the individual cells of the battery and to eliminate as far as possible the effects due to momentary bad contact of the brushes, etc.

If these conditions are fulfilled and a good storage battery is

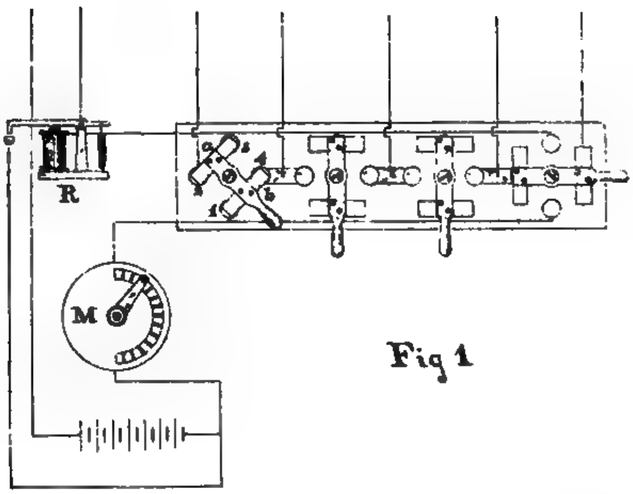


Fig 1

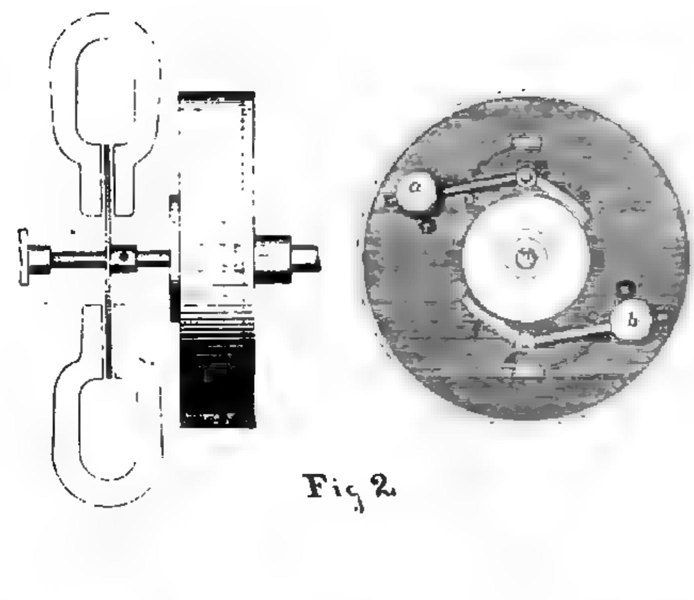


Fig 2.

used to furnish the current, there is no need for a special regulator unless the most extreme accuracy of rotation is desired.

Then the method of governing from an electric fork, which was first suggested and applied by Lord Rayleigh,* furnishes an almost ideal means of securing and maintaining any desired speed. The form of motor used by Lord Rayleigh, however, is only one of many that may be used for the purpose. When working with the revolving mirror in 1891, I considered many plans for driving it in perfect synchronism with a tuning fork by an alternating electric motor, whose circuit was interrupted by the tuning fork, using another motor such as the ordinary air turbine, or a special continuous current motor of the type just described, to furnish the main part of the power. Any form of synchronous motor can be used for the alternating or governing motor but those in which both armature and field coils are stationary are preferable for this speed work.†

It will be noted that the number of revolutions of the motor may be made any sub-multiple of the number of vibrations of the fork by simply increasing the number of poles and armature coils and any desired speed thus obtained without the use of gearing.

There are however other methods of mechanically regulating the speed of electric motors, almost as sensitive as that by the use of the tuning fork.

Any form of clock or chronograph governor can be used to regulate the speed, not by the clumsy method of absorbing an excess of power by the introduction of intermittent frictional resistance, but by more rationally supplying power in proportion to the work done, either by cutting in, or throwing out sections of the field coil, as the speed rises or falls, or even better, by moving a third brush over the commutator (three brush method of regulation).

As regards the mechanical construction of the governor itself, the field has been so thoroughly exploited that there would seem little more to be done. Nevertheless I will venture to describe a form of governor which I have recently designed, and which possesses, I think, one very important advantage in that the action resulting from a change of speed is instantaneous, whereas all forms of governors which govern by centrifugal force alone, require a certain small time to act. Figure 2 is an end view showing the general principle of action. The two governor balls *a*, *b*,

* Philosophical Transactions, Vol. 174, p. 317.

† Such a form together with other special forms of dynamos and motors, was described by the author in *Electrical World*, Vol. XVI, p. 183, "Some New Forms of Dynamos."

PLATE VIII.

APPARATUS FOR ELECTRICAL CONTROL AT EYE-END OF TELESCOPE.

are carried on levers pivoted on pins *c*, *d*, which are behind the balls as regards the direction of rotation, and in such a position that the arc of motion is inclined at a considerable angle to the radial line drawn through any part of the path.

The centrifugal force is counteracted by any form of spring in which the extension of the spring and therefore its restraining force is proportioned to the distance *mb ma* of the ball from the axes of rotation, under which conditions the governor will be strictly isochronous, *i. e.*, the centrifugal force and the restraining force of the spring will be in exact equilibrium for any position of the balls at some definite speed which will be determined by the relation between the weight of the balls and the strength of the spring. This form of governor, which in engineering practice is designated as a shaft governor, is now used almost to the exclusion of the old gravity forms, similar to those still used on chronographs, because of the readiness with which this isochronism can be obtained, for any definite speed and the more powerful governing action secured. The particular point to which it is desired to call attention, however, is the arrangement of pivots with relation to the balls and the direction of revolution, by means of which the inertia of the balls themselves comes into play on the instant of a change of speed and causes them to assume the position to which the centrifugal force alone would bring them only after the change of speed had become well established.

This principal of governing by inertia does not seem to be generally understood and it is surprising that a large proportion of the engine governors now in use are exactly wrong · *viz.*, the ball is placed behind the pivot, instead of in front, thereby causing the centrifugal and inertial forces to at first oppose one another, and still farther delaying the regulation of the speed.

The motion of the balls and levers may be used in any desired way to effect the desired regulation, for instance, if regulation by frictional resistance is required a very simple way is to provide the levers with break shoes *e*, *f*, which will be thrown inward against the dotted drum, which revolves with some degree of friction on a bearing concentric with the axis of the governor wheel. But a better arrangement than any depending on mechanical friction is secured by attaching to *n** a copper disc or drum *n* placed between the poles of two or more powerful magnets. When the drum *n* and the attached disc revolve, strong Foucault currents are set up in the latter, and exert a retarding force on the motion which is nearly proportioned to the speed.

* The *n* was omitted in the copy of Fig. 2.

When used in conjunction with the electric motor these governors would be mounted directly on the shaft and arranged to vary the current in the field or armature (best in the former) in the way previously suggested.

ASTRO-PHYSICAL LABORATORY, Washington, D. C
Smithsonian Institution, January, 1894.

ON THE FORMS OF THE DISCS OF THE SATELLITES OF JUPITER AS SEEN WITH THE 36-INCH EQUATORIAL OF THE LICK OBSERVATORY.*

E E BARNARD

In ASTRONOMY AND ASTRO-PHYSICS for March, May and June 1893, Professor W. H Pickering has given a series of very interesting papers on the forms of the satellites of Jupiter as seen at Arequipa

As the observations contained therein are of a remarkable nature and one of such vital importance to our knowledge of the physical condition of the satellite system of Jupiter, they should be verified before being finally accepted. But coming as they do from a station where the atmospheric conditions are said to be very perfect, they should be treated with respectful consideration pending their complete verification

I have naturally been very much interested in the subject, and though my previous observations of these moons lead me to accept these statements with caution, yet in consideration of the favorable location of the Observatory at Arequipa, it was deemed best to make a careful re-examination of the forms of the satellites under the best atmospheric conditions with our great telescope, before deciding in my own mind as to the reality of the Arequipa results.

Since my return to the Lick Observatory, during the past fall and winter, I have taken occasion when the opportunity afforded to examine the forms of the discs of the four bright moons of Jupiter.

These satellites—at least three of them, I, III and IV—often undergo singular transformations of apparent form during certain stages of their transits across the face of Jupiter These anomalies, as is well known, are in the main due to surface markings on the satellites. Though I had often seen these peculiarities yet I had never seen any of these moons other than round when off the disk of the planet.

* Communicated by the author

In dealing with the Arequipa observations, let us assume that the atmospheric conditions at that station are perfect, as has been claimed. This gives a wonderful advantage in examining the surface markings of a planet or satellite.

But the main question in this case, or at least the one to which I shall devote my criticism, is the forms of the discs of the satellites. It seems to me that the ease with which any malformation could be detected is mainly a function of the aperture of the telescope—assuming only that the definition is good enough to permit the use of proper magnifying power. Therefore the 36-inch refractor of the Lick Observatory is unquestionably better adapted to settle a question of this kind than a 13-inch—even though the 13 inch may have a better atmosphere to work in, and I am not sure that it has.

It would be absurd to claim that the separating power of a 13-inch is equal to that of a 36-inch—both being good glasses by the same maker, and working under nearly equal atmospheric conditions—yet it seems to me this is essentially what the Arequipa observations must claim, for the 36-inch does not verify them.

No one knows better than I that there are certain circumstances where a five or six inch telescope may show an object that cannot be seen with a 36-inch—such for instance as diffuse nebulous and cometary matter. As a matter of fact there are objects in the heavens that the naked eye shows better than any telescope—such as the zodiacal light and gegenschein. The subject in question, however, is not that of diffused and hazy light, but compact and clearly defined bodies with clear cut discs, where the bigger and more powerful the telescope the better they must be seen.

Understanding this, it must be clear that the Lick telescope should be a criterion in this case and by its verdict, if rightly interpreted by me, the Arequipa observations of the forms of these satellites must stand or fall.

Let it be understood, however, that my observations and criticisms have no reference to the observations of the surface markings or the theories of the physical conditions of the satellites, as contained in the papers under criticism.

From my observations with the 36-inch I have collected the following notes on the forms of these satellites when seen off the disc of Jupiter. A power of 1000 diameters has been uniformly used, as recommended by Professor Pickering, several times however when the conditions permitted, a still higher power was used.

To show the conditions of seeing or steadiness of the image I have assumed five as representing the best definition. For convenience of future reference the Standard Pacific Time (8 hours slow of Greenwich) is given.

From a desire to explain the anomalous appearance of the Ist. satellite when in transit Sept. 8, 1890 (see M. N. for February, 1894), I had previously examined this satellite with the 36-inch with Mr. Burnham several times in hope of getting some explanation of its apparent duplicity at the transit mentioned. 1891, Oct. 8, Mr Burnham and I carefully examined the form of the first satellite near elongation. Seeing 4 to 5; power 350 to 1500. Jupiter on the meridian.

The satellite was perfectly round. Mr. Burnham examined it carefully and was positive that it was perfectly round. We also examined II, III and IV, and they were perfectly round.

There was a white spot at the south limb of III, observation from $9^h\ 11^m$ to $9^h\ 40^m$. Later we examined I again, seeing 3 to 4, from $10^h\ 36^m$ to $10^h\ 56^m$. During glimpses of good seeing the satellite was perfectly round to us both.

1891, Oct. 16. From $9^h\ 35^m$ to $9^h\ 50^m$ we again examined the first satellite. Seeing from 2 to 3. During moments of best definition I was perfectly round, as also was II in field with it.

Everyone familiar with Mr. Burnham's extraordinary keenness of vision, and his wonderful power of detecting any defect in the roundness of the image of a star, will agree at once that this satellite must have been round on these occasions.

Since then I have examined the satellites with the 36-inch, the observations bearing directly upon the verification of those made at Arequipa. Some of these observations I quote here, with 1,000 diameters unless otherwise stated.

1893, August	27.	$16^h\ 10^m$ to $16^h\ 37^m$.		Seeing = 4. All four of the satellites perfectly round
August	28	14^h	30^m	All four are round
Sept.	3	13	0	All four are round.
		14	45	Satellite I is perfectly round. Seeing = 4.
		17	0	Seeing = 4, each of the satellites is round.
Sept.	17.	17	25	Seeing = 5. All four are round.
Sept.	24.	12	48	III is perfectly round.
		13	15	All four are round
		17	32	III perfectly round.
Sept.	25.	14	50	I is perfectly round.
		15	0	All four are round and clearly defined.
Oct.	1.	13	30	III with 1,500 perfectly round.
		14	50	I, II, III perfectly round.
		16	40	I and III perfectly round, seeing = 4.

Oct.	1.	17^h	30^m	Seeing = 4. I, II, III are round.
	29.	12	30	Seeing = 5. All the satellites are round
Nov.	6.	11	54	III is beautifully round.
		14	40	III is perfectly round. Seeing = 4.
Nov.	12.	10	16	III is perfectly round.
Dec.	3			I is near transit, following, it appears slightly elongated towards Jupiter.
Dec.	10	9	25	All four are round. Seeing = 3. North Pole of III is white.
		12	5	All are round. The south Pole of IV is light. The N. Pole of III is light. Seeing = 4.
Dec.	11.	9	18	I, II, III each is round. IV seems a little deficient on following side as if a slight phase existed. Bright spot at south Pole of IV. Bright spot at north Pole of III.
1894. Jan.	28	6^h 50^m to 6^h 55^m,		I, II, III are round. IV is slightly deficient on following side as if a slight phase or a dark area existed on it.

In *Monthly Notices* of the R. A. S. for February, 1894, I have given an account of the discovery of a bright belt on satellite I. It seems to me that with a very high magnifying power and not the best seeing, the duskiness of the poles of this satellite and its white equatorial belt might give the appearance of elongation towards the planet as noted by me Dec. 3. But Professor Pickering would not be deceived by this.

From the drawings given in ASTRONOMY AND ASTRO-PHYSICS for June, 1893, the distortions of the satellite discs are so conspicuous, that it would seem impossible for them to escape detection with our great telescope, even with the most casual observation, and for such to elude a careful and conscientious inspection would be very remarkable indeed.

It seems to me that with a very high magnifying power on a small telescope, the surface markings of the satellites themselves, when near the edges of their discs, might readily cause an apparent distortion of the satellites, as these same markings certainly do when the satellite is seen in transit.

APPEARANCES OF THE IIIRD SATELLITE IN TRANSIT.

In reference to the observations of the IIIrd satellite on September 17-24 and Nov. 6, 1893, perhaps a few more details may be of interest.

On September 17th, a dusky belt on this satellite was quite a noticeable feature. It was possible to determine the position angle of this with considerable precision. 13^h 40^m (St. P. T.) from four settings, the position angle was 108°.3. Mr. Garrett P.

Serviss of Brooklyn, New York, who was in the dome with me at the time, also saw the belt clearly. One setting of the wires by this gentlemen gave the position angle 114°. We both agreed that there was a diffusion of this belt towards the south near the following limb of the satellite. The last contact of III with Jupiter at emergence from transit was at $13^h\ 53^m$ St P. T.

Sept 24. The satellite was very much deformed during transit; the south preceding part of the disc being deficient, giving it the appearance shown in the sketch There was evidently a dark spot on the *s. p.* portion of the satellite, this being about the same shade as that portion of Jupiter, caused the south preceding portion of III to be undistinguishable from the surface of the planet. A fragment of a dusky belt was also seen cutting across the lower visible portion of the satellite. After emerging from the face of Jupiter, III was perfectly round, and the dusky belt could be seen as on Sept. 17. At mid-transit the south limb of III was tangent with the south limb of Jupiter.

Following are observations of III when entering transit:

First contact satellite and planet	16^h	$4^m.5$	St. P. T.
Satellite ½ on	16	19 .5	" "
Satellite completely inside the disc	16	30 .0	" "

Nov. 6. The belt and dusky spot were seen again. At $11^h\ 45^m$ two settings of the wires gave 111°.3 as the position angle of the belt. The phenomena of the transit of Sept. 24. was repeated except that the defective part was smaller—the satellite presenting a slightly different face to us. The transit was observed with 1000 diameters and the seeing = 4. The two drawings of this date are self explanatory. The one before the satellite entered in transit, shows a dusky region on its north preceding side, and this being of the same depth of shade as the planet on which it was later projected, gave the appearance of a defect in the symmetry of the satellite when in transit, as shown in the second drawing.

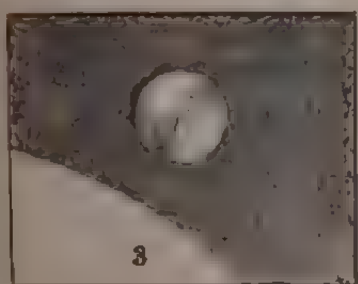
Comparing the first of these two drawings with that of September 17 it will be seen that, if the dusky shading is the same in both cases, and I think it is, the satellite period of rotation is different from that of its revolution about Jupiter. From this marking, if it is permanent, the rotation of the satellite can be easily determined, and I hope, if circumstances permit, to decide this question at the next opposition of Jupiter.

In these cases the satellite off the disc was perfectly round, but while on the face of the planet was defective, and this was obviously caused by the dusky marking.

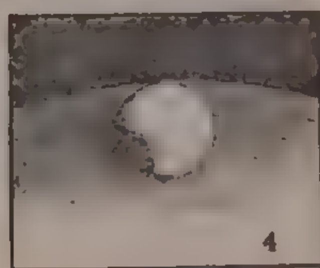
PLATE VIII. a

1893, Sept. 17, $14^h 16^m$.

1893, Sept. 24, $16^h 58^m$.
In Transit.



1893, Nov. 6, $11^h 54^m$.
Before Transit.



1893, Nov. 6, $13^h 35^m$.
In Transit.

Phenomena of Jupiter's IIId Satellite.
36-inch Equatorial.

TRANSPARENCY OF THE LIMB OF JUPITER.

Among the many questionable phenomena of Jupiter and his moons there is one that seems to have gained a respectable footing in astronomical literature. It has been more than once claimed that the satellites have been seen through the limb of Jupiter when undergoing occultation

In my mind this has been due to either poor seeing, a poor telescope or "an excited observer."

For nearly fifteen years I have observed Jupiter and his satellites, and with telescopes all the way from 5 inches up to 36 inches have tried to see this phenomenon. I have often watched the satellites under first class seeing with the 12-inch here at occultation, but have never seen one of them through the limb of Jupiter though that phenomenon was specially looked for

I have also made the following notes on this subject with the 36-inch.

1891, Oct. 22d, Mr. Burnham and I watched satellite II at occultation. Seeing = 4. The limb of Jupiter seen distinctly cutting the satellite. When last seen there was only the tiniest speck of II protruding—this quickly disappeared. Neither of us, taking turns, could see the slightest trace of the satellite through the planet. It passed behind a part of the limb where one of the dark belts should join it.

1892, Aug. 12. $16^h\ 6^m\ 48^s$ Standard Pacific Time. Satellite II bisected in coming out from occultation. Carefully watched for any trace of it through Jupiter. It was sharply cut by the limb of the planet which was sharply defined. The satellite was strongly contrasted with the limb but no trace of it could be seen through the planet. Seeing = 5. First class observation.

1892, Sept. 14. $14^h\ 17^m\ 17^s$ Standard Pacific Time. Satellite I bisected at emergence from occultation. Sharply cut by the limb, no trace of it through the planet. Seeing = 5.

1893, Sept 3. $14^h\ 39^m\ 23^s$ St. P. Time. Satellite I bisected coming out from occultation. Beautifully cut by the limb. No trace of it through Jupiter. It came out at the end of a heavy narrow belt north. Satellite very white compared with the limb —a strong contrast.

These observations have been made with care, with the most powerful telescope that to-day can be applied to such observations, and the limb of Jupiter has appeared perfectly opaque—as at all previous observations with the smaller telescopes.

I think it is high time that astronomers reject the idea that the

satellites of Jupiter can be seen through his limb at occultation. When the seeing is bad there is a spurious limb to Jupiter that well might give the appearance of transparency at the the occultation of a satellite. But under first class conditions the limb of Jupiter is perfectly opaque. It is quibbling and begging the question altogether to say that the phenomenon of transparency may be a rare one and so have escaped my observation. Has any one said yet that the Moon was transparent when a star has been seen projected on it when it ought to have been behind?

MT. HAMILTON, 1894, March 8.

MECHANICAL CAUSES OF THE FORMATION, MOTION AND PERIODICITY OF THE SUN SPOTS.*

J. M. SCHAEBERLE.

In previous papers bearing more directly upon the form of the Sun's corona, I have repeatedly stated that the observed data obtained during solar eclipses indicated that the corona was the visible representation of ejective effects produced by forces acting mainly in the Sun's spot zones. No attempt was made to assign the reason why the solar forces should be most active in particular regions; the observed fact was simply made use of to explain certain other phenomena.

The object of the present paper is to point out that according to mechanical principles every incandescent rotating liquid or gaseous body undergoes periodical surface changes in the form of zonal waves of varying surface strength accompanied by changes of internal pressure.

Any non-rotating incandescent liquid or gaseous mass in space will, through the action of gravity and heat radiation, tend to assume a spherical form, the temperature at the surface being lower than the interior. For any given element the increase of temperature and pressure with the depth below the surface places each particle in a state of unstable equilibrium, so that any disturbance of this condition at once results in a series of ascending heated currents and descending cooled currents of matter, such that at any given instant the density, temperature and radial velocity at the surface of any given spherical shell within the body will be the same at every point of the shell. If now a motion of rotation is given to the sphere, the homogeneity of the surface of any given shell is completely destroyed.

* Communicated by the author.

The actual conditions relating to variation in temperature, velocity, density, etc., are unknown, as is also the form of the complete equations for determining the relative motions of the rotating mass. The general effect of these internal forces can, however, be traced with considerable certainty, as will be shown in this paper.

As the direct cause of the non-homogeneity of any spherical shell is the angular motion, we shall only consider those relative motions which are due to axial rotation.

In the following table relative values are given for the rudely approximate conditions existing for particles enclosed between the two bounding surfaces of any thin spherical shell and the two conical bounding surfaces of an intersecting spherical sector having a small constant breadth in latitude, the axis of the sector coinciding with the axis of rotation.

Latitude	Mass	Relative Velocity		Relative Momentum	
		Radial	Tangential	Radial	Tangential
0°	350	1000	0	350	0
10	344	970	171	334	59
20	328	883	321	290	105
30	302	750	433	226	131
40	268	587	443	157	132
50	224	413	493	93	110
60	174	250	[illegible]	43	75
70	110	117	321	14	38
80	61	30	171	2	10
90	2	0	0	0	0

The last column of the above table shows that at the beginning of rotation the tendency of the surface matter to move toward the equator is greatest in the middle latitudes, the kinetic energy of the mass moving towards the equator is also greatest in the same latitudes.

For the sake of brevity four figures have been drawn which give a graphical representation of what would require too much space to describe in full.

In Fig. (1) the length of a normal ordinate to the boundaries of the shaded areas Q, Q, represents the relative magnitude of mass-motion towards the equator, *at the beginning of rotation*, for any given latitude. The curved full lines drawn tangent to the dotted radial lines indicate, in a general way, the paths of particles in different latitudes, moving from the interior towards

satellites of Jupiter can be seen through his limb at occultation. When the seeing is bad there is a spurious limb to Jupiter that well might give the appearance of transparency at the the occultation of a satellite. But under first class conditions the limb of Jupiter is perfectly opaque. It is quibbling and begging the question altogether to say that the phenomenon of transparency may be a rare one and so have escaped my observation. Has any one said yet that the Moon was transparent when a star has been seen projected on it when it ought to have been behind?

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Any non-rotating incandescent liquid or gaseous mass in space will, through the action of gravity and heat radiation, tend to assume a spherical form, the temperature at the surface being lower than the interior. For any given element the increase of temperature and pressure with the depth below the surface places each particle in a state of unstable equilibrium, so that any disturbance of this condition at once results in a series of ascending heated currents and descending cooled currents of matter, such that at any given instant the density, temperature and radial velocity at the surface of any given spherical shell within the body will be the same at every point of the shell. If now a motion of rotation is given to the sphere, the homogeneity of the surface of any given shell is completely destroyed.

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As the direct cause of the non-homogeneity of any spherical shell is the angular motion, we shall only consider those relative motions which are due to axial rotation.

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70	119	117	321	14	38
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The last column of the above table shows that at the beginning of rotation the tendency of the surface matter to move toward the equator is greatest in the middle latitudes, the kinetic energy of the mass moving towards the equator is also greatest in the same latitudes.

For the sake of brevity four figures have been drawn which give a graphical representation of what would require too much space to describe in full.

In Fig. (1) the length of a normal ordinate to the boundaries of the shaded areas Q, Q, represents the relative magnitude of mass-motion towards the equator, *at the beginning of rotation*, for any given latitude. The curved full lines drawn tangent to the dotted radial lines indicate, in a general way, the paths of particles in different latitudes, moving from the interior towards

the surface. The rate of curvature of these paths increases with an increase in the velocity of rotation.

Now submerged particles in a zone of given latitude, come to the surface in a zone of less latitude, consequently the resulting angular surface velocity will in general be less than the equatorial taken as a standard as has already been pointed out by Faye. The retardation from this cause will be greatest in the middle latitudes.

In Fig. (1) for instance, a series of submerged particles on radii cutting the surface at the points K′, R′, T′ all on the same meridian, come to the surface at the points K, R, T all in less latitudes, with linear velocities corresponding to greater latitudes a consequent retardation of angular velocity results from this change in latitude.

The retardation from this curve is zero at the equator and at the poles and a maximum in middle latitudes.

The ordinates to the black radial area are roughly proportional to the amount of retardation per unit of time for different latitudes.

Where the surface flow is greatest the density *at the beginning of rotation* will be least, as the heated matter from the interior is allowed easiest access to the surface, consequently the thickness or density of the cooled surface will be at a maximum at each pole and at the equator, while the minimum density will be in the middle latitudes. The normal ordinates from the circumference to the curve H, H, roughly represent the variation of density of the surface due to longer exposure to the cold of space.

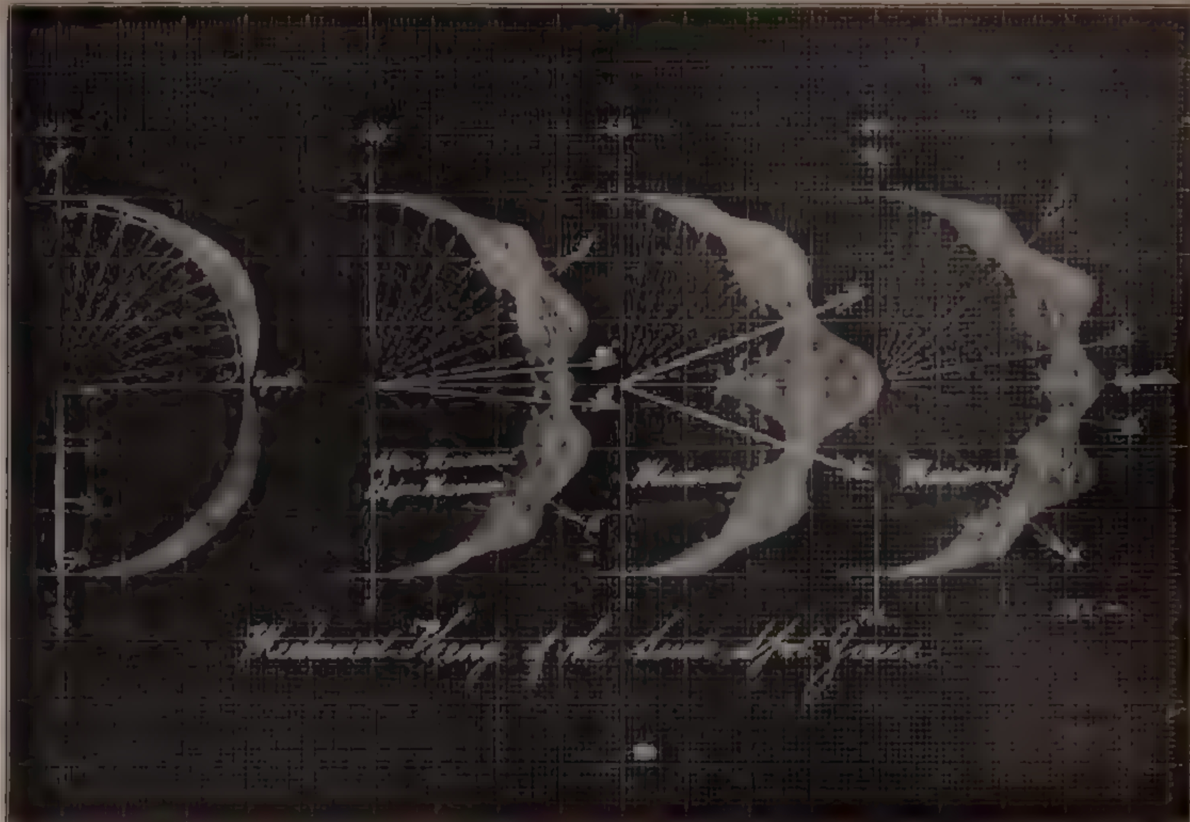
The above described conditions refer to the forces in operation immediately after an originally non-rotating incandescent liquid or gaseous body has been forced to have a motion of rotation about a central axis.

The ordinates to the shaded wave-like areas in Fig. (2) roughly represent the relative surface strength which must exist in the now spheroidal mass at some subsequent instant of time. As the surface flow towards the equator is greater in the middle latitudes than it is in lower latitudes, cooled matter must evidently, for a time, pile up as at AA, and also at the equator where the flow is from opposite directions.

Fig. (3) represents a still later phase in which the masses previously at AA have reached the equator forming the great mass c, while those of BB have moved on to DD.

Fig. (4) represents the conditions in which the waves have mean values. The changing surface conditions illustrated in

PLATE IX.



Figs. (2), (3), (4), (2), etc., will follow each other periodically, in the order given, so long as the rotating body retains its liquid or gaseous state.

The following laws based on purely mechanical principles are probably true throughout the whole universe.

Every incandescent rotating liquid or gaseous body in space which has assumed the spheroidal form undergoes periodical changes both of its surface and of internal pressure. The zones of greatest and least surface strength are parallel to and continually move towards the equator of the rotating body. The surface density, in the polar regions is always comparatively great.

In all such bodies the angular velocity of the surface will be greatest at the equator.

Applying this new principle of zonal-wave motions to the theory of our Sun a very simple explanation of the hitherto puzzling phenomena of the periodicity and motions of solar disturbances is obtained.

Without reference to the actual condition of matter at the Sun's surface (whether liquid or gaseous) it is safe to say that the temperature is lower than the interior, and that the greater the amount of moving surface matter in a given zone the greater will be the depression of this matter below the general surface of the spheroid, and the greater will be the amount of energy developed by the expansion of that portion of the cooled matter of which is forced to flow inward. The forces developed will always be such that there is a constant tendency to drive adjacent matter away from the crest of a zonal wave; so that the general flow towards the equator will, for a time, be retarded in latitudes greater than that of the zonal crest, and accelerated for lower latitudes.

In Figs. (2), (3) and (4) the long radial lines or arrows $\alpha, \alpha', \alpha''$, β, β', β'' drawn through the depressions in the shaded areas, indicate the positions of the zones of least surface strength. In Fig. (2) the zones α'', α'' moving towards the equator are about to be reinforced by the advancing masses A, A. The expression of inflowing matter due to the excess of pressure at A A, retards the velocity of adjacent matter in higher latitudes, causing secondary crests as at BB, and intermediate zones of weakness β'', β''. These systems of strong and weak surface zones follow each other and have a general motion towards the equator. The internal pressure will evidently reach a maximum when the masses A, A, from both hemispheres unite at the equator, as in Fig. (3), where they are forced to flow inwards; the expansion of this inflowing cooled matter causes an increase of internal pres-

sure which will be greatest in a submerged equatorial zone. The zones of weakness α'', α'', no longer exist while β'', β'', have now moved on to the positions α, α. After the greater portion of the cooler mass C has been forced into the interior the excess of pressure due to its expansion becomes less, and the standard masses D, D, are again accelerated. New zones of strength and weakness form and travel towards the equator in endless succession in the order represented by the surface waves of Figs. (3), (4), (2), (3), etc. If for any reason two crests or two depressions do not reach the equator at the same time differences of phase will result in the two hemispheres.

For a perfectly symmetrical distribution of the forces and motions it might also happen that after long intervals of time such final conditions of things might result that the zones of strength and weakness (crests and depressions) would for periods remain fixed in latitude. Unbalanced forces due to a lack of perfect homogeneity of the mass would, however, constantly tend to recreate zonal waves moving towards the equator.

The interval of time between two successive similar phases in a given zone will depend upon the fluidity of the mass and the velocity of rotation. For a mean surface velocity of three miles per hour (towards the equator) the periodic time will be about eleven years in the case of a body as large as the Sun.

Starting from a mean condition of the solar surface as represented by Fig. (4), the principle zones of weakness are at α', α', β', β', and eruptions on the surface will be mostly confined to these zones. Owing to the mass motion the eruptions at α'', α'', will gradually disappear as the zone approaches the equator at the same time those in the zone β'', β'', will increase in number both on account of the stoppage of the rents in the zone α'', α'', and because of the diminishing distance of the zones β'', β'', from submerged equatorial region of greatest pressure. This pressure will evidently reach a maximum for the conditions represented in Fig. (3), in which the submerged equatorial zone of cooler matter attains its greatest volume. The neighboring zones of weakness α, α, Fig. (3) most readily give way to the excess of pressure, and evidently the direction of the streams of ejected matter which form the corona may at times deviate very largely from the normal; the magnitude of this deviation depending upon the relative positions of the center of greatest force and the surface of least strength at which an eruption takes place. Gaps or rifts in the corona will, in general, be found opposite the zones of greatest strength.

As to the causes which produce the phenomena observed in and around solar spots the limits of this paper prevent any extended remarks. The spectroscopic observations of Hale, Lockyer, Secchi, Young and others seem to demonstrate that the spots are caused by cooled matter coming from higher regions, and that the faculæ are probably the more highly heated masses rising from a lower level. The views of Secchi, that the spots are caused by streams of ejected matter falling back to the Sun near but not at the place of ejection, seem to offer the simplest explanation of the observed phenomena. Every spot once formed would represent a surface of great strength, thus offering greater resistance to eruptions from the interior than the more heated neighboring areas; and the indraft over such a spot would continually draw in cooled material from neighboring eruptions thus tending to perpetuate the spot long after the primary eruption has ceased to exist.

In agreement with the theory that the surface density at the poles of an incandescent rotating body is greater than at lower latitudes, observations show that in very high latitudes the eruptions are not only rare but when seen are found to be of comparatively short duration, so that conspicuous circumpolar spots would not be expected.

The application of the principle of zonal wave motions to the mechanics of our own atmosphere and also to that of Jupiter and its bearing upon the variability in the light of the fixed stars is reserved for a future paper.

Lick Observatory, March 7, 1894.

Address Delivered by the President, Captain W. de W. Abney, C.B., R.E., D.C.L., F.R.S., on Presenting the Gold Medal to Mr. S. W. Burnham.

The Gold Medal of the Royal Astronomical Society has been awarded by the Council to Mr. S. W. Burnham for his discovery and measurement of double stars; and following the custom of the Society, it is the duty of the President to lay before it the grounds on which the award has been made.

I can scarcely hope to do justice to the labors entailed in the extensive researches made by Mr. Burnham, and to his discoveries. I believe I am correct in stating that Mr. Burnham's first

astronomical communication was made to the *English Mechanic*. It is, however, with his star catalogues and other later communications to which your attention must be drawn. The catalogues of double stars which he has given us amount to no less than nineteen containing 1,274 new double stars, a number which far exceeds those discovered by any one observer.

Burnham's first catalogue of new double stars, published in 1873, consisted of 81 pairs, which were discovered with a 6-inch Alvan Clark refractor at Chicago, and occupied his time from 1870 to 1872. The distances of the doubles in this, as in several of his first catalogues, were estimations and not exact measurements, his telescope not being furnished with a micrometer. It may here be incidentally remarked that, during the discovery of some of these pairs, he was in communication with Dembowski, who measured many of them, whilst others were measured by our Fellow, Mr. Knott, and appear in his catalogue. The measures by Dembowski were not published till after this observer's death, and were, comparatively speaking, recently (1888) printed by the *Reale Accademia dei Lincei* at Rome. The first catalogue contains several pairs which are very difficult to see with a 6-inch, even when they are known to be doubles. For instance

Number in Catalogue.	Magnitudes.	Distances. ″
4	7 and $7\frac{1}{2}$	0.5
13	8 " 12	1.0
63	6 " 12	0.7

Burnham's second catalogue contained twenty-five new double stars, and was like the first published by this Society in its *Monthly Notices* and in the same year, viz., 1873. The 6-inch was still his instrument, and the class of star is about the same, as will be seen from the following examples:

Number in Catalogue.	Magnitudes.	Distances. ″
89	9 and 9	0.6
96	6 " 14	2.5
104	7 " 12	2.5

His third catalogue contained seventy-six new doubles, and was also published in 1873. In this Burnham began to impose restrictions on his observations, rejecting distances exceeding 5″ and faint pairs below the ninth magnitude when not connected with a brighter star. He thus early appears to have grasped the true idea of weeding out, instead of cataloguing, useless or uninteresting pairs. The four following stars are from this catalogue:

Number in Catalogue.	Magnitudes.	Distances.
120 ν Scorpii	4 and 8	0.3
138	7½ " 10	1.0
141	7½ " 8½	0.4
151 β Delphini	3½ " 5	0.7

This catalogue is important, as containing a class of double star peculiar to Burnham's catalogues. I refer to pairs where the principal star is a naked eye star, and the companion close and faint.

In the pair ν Scorpii, the principal star is one of the 4th magnitude and the companion of the 8th magnitude, at a distance of less than half a second; the discovery of this pair is a remarkable feat with a 6-inch, and the more so as another companion to ν Scorpii had been measured by many observers before, and the chief component must have been well scrutinized. Burnham says, "I examined it several times under the most favorable circumstances, but could not get rid of an apparent elongation of the principal star in a direction nearly north and south. I requested Professor C. A. Young to examine it with the splendid 9.4-inch Clark refractor of the Dartmouth College Observatory. He examined it several times, and at last when the air was very steady he was rather inclined to think it double, although he could not even notch it." This star was early known as a wide pair, and Jacob at Madras in 1847 found the companion was also double. The close pair was in 1874 measured with the 26-inch Washington refractor, and by Dembowski.

From what has been said, it must be evident that Burnham has a remarkable acuteness of vision, and an eye wonderfully free from defects such as astigmatism, which would render observations such as these impracticable.

The fourth catalogue was published in the *Monthly Notices*, June, 1874. The fifth catalogue has 71 more new pairs, and brings out a peculiar characteristic of your medallist. If a star disc deviated an almost infinitesimal quantity from the circular, his eye detected it at once. In 1874 at Washington, on the night of August 11, he scanned some of his old discoveries, with the result that he made an addition of 14 new pairs to his list. I give one instance. No. 291 in the catalogue had on some occasion offended his critical eye when looking at it through the 6-inch, so he turned the 26-inch on it and found it consisted of two 8½-magnitude stars separated by a distance of only 0″.2. Turning, by chance, the telescope on to 34 Pegasi its mystery also disappeared, for a faint companion 2″ south was discovered.

In 1870, when your medallist began his work, very little was

being done in discovering new doubles. Most observers were contented with the catalogues of the Struves and Herschel, and, so far as I can gather, he had no intention to add largely to these catalogues. His acute eye, however, rendered it impossible for him to stop. His small, though very perfect, instrument was the means of breaking through that resolution, if he had formed one. As already mentioned, Mr. Burnham has added a new class of double stars—viz., naked-eye stars with faint companions. The more difficult of these were discovered with the 36-inch Lick refractor, and have already become interesting. Out of the 1,274 new double stars which he has discovered, 197 are naked-eye stars, not previously known to be double. Of the 1,274 no fewer than 120 have been proved to be physically connected by later measures. He has found new components to 113 old pairs, as follows:

W. Struve (Mensura Micrometricæ)	47
O. Struve (Pulkova Catalogue)	14
W. Herschel	14
J. Herschel	22
South	9
South and Herschel	7

When Mr. Burnham had the use of the $15\frac{1}{2}$-inch* refractor of the Dearborn Observatory, his catalogue still showed that the maximum dividing power was what he sought, and a star from his eleventh* catalogue will exemplify this. In this catalogue was β Scorpii of the second magnitude, with a companion of the tenth magnitude, distant only 0.8″. Even *he* considered it a very difficult pair, and, up to that time, far beyond any close pair discovered, in the inequality of its components. No second-class instrument, however large, would show its duplicity.

In the same catalogue are

	Magnitudes.	Distances ″
η Cygni	4 5 and 13	7 2
52 Hydræ	5 0 " 11	4 0
54 Herculis	5 " 12 5	2.6

When the great 36-inch telescope was placed at his command, he determined to still further restrict the class of star to be measured, and in selecting stars for his working catalogue, he gave the preference to such as could not or would not be observed elsewhere, leaving the easier systems to others. Speaking generally, I am informed that the most difficult to observe are the most interesting, and but for the care he bestowed on the most difficult of the old systems, as well as those of his own discovery, a serious gap would have occurred in the measures.

* This should read $18\frac{1}{2}$-inch refractor, and eleventh should read thirteenth.—Ed

An idea of the recent catalogues may be derived from an analysis of his eighteenth, which contains the numbers β 1225 to β 1266, that is, 42 pairs.

	Distances.	Pairs.	
	"		
Under	1	15	42
Between	1 and 2	13	
"	2 " 3	7	
Above	3	7	

As specimens may be cited

Numbers.	Distances.	Magnitudes.
	"	
1226	0.40	8.5 and 10.5
1240	0.15	5.6 " 6.0
1241	0.53	5.9 " 10.0

The faintness of the companions and the small distances of the recent Burnham pairs, indicate interesting researches for the possessor of the most powerful telescopes.

In all, your medallist, as before said, has published nineteen catalogues, containing 1,274 pairs, and I believe I am correct in saying that still one more catalogue is in the press, for I learn that the proof sheets of some 250 pages have left his hands for publication. These, it may be presumed, contain observations made at the Lick Observatory.

Of the 1,274 stars already published,

			"	
123	pairs are	under	0.5	apart.
230	"	between	0.5 and 1	"
370	"	"	1 " 2	"
168	"	"	2 " 3	"
178	"	"	3 " 6	"
205	"	over	6	"

Thus, of all known pairs whose distance is under 1″, Mr. Burnham has added more than one half.

We must also note that, besides the measures of his new stars, we are indebted to him for many thousands of measures of previously known doubles.

Not only is Mr. Burnham an original observer, but he is a critic as well. He criticised the catalogue of Sir J. Herschel in a paper in the *Monthly Notices* in 1873.

Nor did the Bedford Catalogue escape his searching scrutiny. A paper in 1880, June, in the *Monthly Notices* dealt with it somewhat severely. But all his criticisms have had the object of correcting and pointing out errors. His paper on the Trapezium of Orion is an example of this. He shows that the minute stars which were said to lie within it and to be visible with small tele-

scopes are, to say the least, mythical. It has only been with the 36-inch Lick telescope that any minute stars have been found, and these would be invisible in any telescope less than a 30-inch.

Not less excellent work has been carried out by your medallist in calculating the orbits of binaries; and, as an examble of the original way in which he sets to work, one has only to refer to his paper in the *Monthly Notices* of April, 1891, on the Orbit of the Companion of Sirius. It may be mentioned that he was the last to obtain measures of the companion of Sirius in 1890. In 1891 he failed to detect it with the 36-inch Lick telescope. With these last observations before him, he re-computed the orbit, and found a period for it shorter than those of Gore or Howard, his period being 53 years, whilst theirs were 58.47 and 57.02 respectively. Should this orbit be correct, the companion should be again visible this year.

Of double stars discovered by Burnham which have short peroids, the following are some of the most remarkable:

	Period.	Period Determined by
κ Pegasi β 989	11.37	Burnham
β 883	16.35	Glasenapp
85 Pegasi = β 733	17.48	"
β Delphini = β 151	22.97	"
9 Argus = β 101	23.3	Burnham
β 416	24.7	"
20 Persei = β 524	27.7	"
β 612	30.0	Glasenapp

It appears that there are only two other binaries whose known periods are less than 25 years. I may interpolate here a word as to a second most valuable paper on invisible double stars, in the same number of the *Monthly Notices* in which his paper on Sirius appears, as it indicates well his critical capabilities. In it he treats of the irregularities in the measures of certain double stars. These irregularities have been ascribed to the presence of a dark body in the system, and in some suggestive diagrams Mr. Burnham indicates his opinions on the subject. The double-star orbits which he has found, besides those named, are:

Double-Star Orbits. S. W. Burnham.

Σ 1785 (*Astr. and Ast.-Phys.*, May, 1893).

20 Persei (β 524) in	*Astr. and*	*Ast.-Phys.*,	May 1893.
9 Argus, β 101	"	"	June 1893.
70 Ophiuchi	"	"	Aug. 1893.
OΣ 285	"	"	"
6 Eridani	"	"	"
37 Pegasi	"	"	Oct. 1893.
95 Ceti	"	"	"

It may here be noted that Burnham, in his astronomical car-ee

has used a variety of telescopes—the 6-inch his first, a 9.4-inch at Dartmouth, the 12-inch Lick, the 15½-inch Washburn, the 18½-inch Chicago, the 26-inch Washington, the 36-inch Lick. With him, increase of aperture available meant a further refinement in his researches, and a further power for interesting work.

The catalogues of the double stars and of orbits calculated by Burnham are a formidable work to have been accomplished by any one man; but when it is remembered that this is the more serious phase of his labors, and does not include much of what one might almost characterize as a lighter character which he has contributed to astronomy, it seems almost impossible to realize that it lay within the capacity of any one individual. The *English Mechanic*, the *Monthly Notices and Memoirs of the R. A. S.*, the *Astronomische Nachrichten*, the *American Journal of Science*, ASTRONOMY AND ASTRO-PHYSICS, *Knowledge*, the *Sidereal Messenger*, the *Observatory*, and various other papers have been enriched by his contributions. The line of work that he laid himself out to accomplish he has successfully carried through. It is not of that showy or dramatic order which attracts universal attention, or gives occasion for newspaper paragraphs. It is, however, as arduous as it is unpretending, and when more than twenty years have been devoted to it, and when the success which has attended it has been so remarkable, it does honor to the Society to recognize the high estimation in which it holds this work by awarding to its author the greatest distinction it can confer.

Mr. Burnham is an amateur in the true sense of the word. Born about 1840, as far as I can learn, he adopted the vocation of stenographer, and it was not till he had chosen his profession that his mind was fortunately directed to the study of astronomy. What his first toy telescope may have been I know not; but from the time when he secured his 6-inch Clark, he made the progress in the direction which he had determined to follow. By day he followed his regular calling, whilst by night he studied the heavens till (as an article in *The Century* informs us) "daylight drove him to bed." In 1874 he became a Fellow of this Society, being nominated by his friend the late Rev. T. W. Webb, an astronomer to whose well known book, apparently, Burnham was indebted for the turn which his astronomical labors were to take. In 1876 Burnham was appointed Director of the Chicago Observatory, a post which he held for a short time, though he subsequently had the use of the 18½-inch telescope at that observatory. In 1879 when the trustees of Lick Observatory had chosen Mt. Hamilton as the site on which to build their Observatory, he

was selected on the recommendation of Professor Newcomb to report on the atmospheric and other conditions of that locality, and subsequently observed the transit of *Mercury* from the same spot in conjunction with Professor Holden. This connection with Mt. Hamilton was not destined to cease, for he was appointed to a post in the Lick Observatory, where he turned the magnificent telescope of that institution to good account in his researches. Lately he retired from the position occupied there and resumed his work at Chicago, and holds the position of Professor of Practical Astronomy at that University. It is to be hoped that he will be only temporarily absent from an established Observatory; for, if rumor is to be believed, he is to be Astronomer to the Yerkes Observatory, where the great 40-inch telescope is to be erected. If this be so, the choice made by the trustees is an honor to him and to themselves.

I think, gentlemen, I have said enough to convince you that the medal has been worthily bestowed and well earned, and in handing it to our honored foriegn secretary, Dr. Huggins, to transmit to him, I would ask him at the same time to convey a message from the Royal Astronomical Society "in Annual Meeting assembled," wishing Mr. Burnham health and strength to continue his contributions to astronomical science, and expressing their gratitude to him for what he has done for it in the past.—*Monthly Notices.*

THE ORBIT OF 9 ARGUS.*

S W BURNHAM

In the issue of ASTRONOMY AND ASTRO-PHYSICS for June, 1893, I give the results of an investigation of the orbit of 9 Argus (β 101) and predicted that in the two years following the date of my last measures at Mt. Hamilton (1892.05) the angular motion of the companion would be about 180°, and that at the beginning of the present year the position-angle would be a little more than 270°.

In order to ascertain whether or not the apparent ellipse shown in the paper referred to, which depended entirely on my last measures with the 36-inch refractor, I requested Professor Barnard to make a set of measures with the same instrument,

* Communicated by the author.

and to carefully note the quadrant of the smaller component. I have received from him the following observations:

		°	″
	1893.920	278.9	0.52
	.939	285.8	0.40
	.961	281.6	0.40
	1894.153	282.8	0.37
	.189	280.8	0.48
	.192	282.4	0.42
Mean	1894.06	282.0	0.43

The smaller star was noted as being certainly on the preceding side. It will be seen that this position conforms very closely to the ellipse I have given. The angular motion is a little greater than that predicted, and consequently the distance is a trifle larger, but the difference is only 0″.07, and the agreement on the whole is entirely satisfactory.

In the paper referred to, I found a period of 23.3 years. The later measures appear to indicate that this time may be still further reduced, but as the observations of next year will give more accurate data for whatever correction may be necessary, it does not seem worth while at this time to obtain another provisional orbit.

THE SATELLITE OF NEPTUNE.*

F. TISSERAND.†

Less than a month after Galle had discovered Neptune‡ in the place assigned to it by the calculations of Le Verrier, the English astronomer Lassell suspected the existence of a small satellite, and verified it with certainty in 1847. This body gives little light, for it is only of the fourteenth magnitude, and it requires a very powerful telescope to render it visible. According to the photometric determinations of Pickering it would be, however, as large as our moon; but it is about 12,000 times more distant from us, so we understand why it is so faint.

When its orbit was calculated it was found that the satellite had a retrograde movement around the planet; this is more marked than that already known concerning the satellites of Uranus; from this point of view the two planets, the most distant of our system, present a striking contrast to the others.

* Translated from *L'Astronomie*, March, 1894.

† Director of the Observatory of Paris.

‡ Neptune was searched for and found by Galle from the calculations of Le Verrier, Sept. 27, 1846; the satellite was discovered by Lassell Oct. 10, following.

We know to-day that Mars has two satellites, Jupiter four, Saturn eight and Uranus four. One might suppose that Neptune would have more than one. Several times searches have been made with powerful telescopes, notably with that at Washington; but no new satellite has been found.

Lassell's satellite appears to be an unique body in the solar system, in this respect, that its very great distance from the Sun must protect it from perturbations having their origin on this side. On the other hand it is not troubled by neighboring satellites; it seems that it should present a movement of the greatest simplicity, realizing rigorously the geometric motion of Kepler. So some astronomers have proposed to make it a sort of touchstone to verify the uniformity of certain movements in the planetary system; it would constitute a time-keeper of great precision, to which no cause of derangement could be foreseen.

The accumulated observations, however, have shown that this is not true.

Mr. Marth, the English astronomer who occupies himself with the ephemerides of satellites, called attention some five or six years ago, to a singular fact: the observations from 1852 to 1883 show that the plane of the orbit of the satellite of Neptune is slowly shifting, in the same direction and by an appreciable amount, for, during these 31 years its inclination to the plane of Neptune's orbit has increased about 5°, and this difference is too great to be accounted for by errors of observation. On the other hand the observations made by Mr. H. Struve, with the great refractor at Pulkova, during the last ten years, confirms the direction and the amount of displacement of the orbit. What can be the cause of this trouble?

We have not hesitated to attribute it to the flattening of the planet. This flattening has escaped, up to the present time, direct measures, and it will escape without doubt for a long time yet. The reason is that the disc of Neptune subtends to us only a small angle of about two seconds, and if the flattening is small, $\frac{1}{100}$, for example, the ellipticity of the disc will be imperceptible.

But, in order to account for the derangement established by the observations, another thing is necessary. If, indeed, the plane of the orbit of the satellite coincided with the equator of the planet, there would be no reason for this coincidence not maintaining itself indefinitely. It is necessary, then, that the two planes make a considerable angle with each other and it is demonstrated that in this case the first of the two planes is displaced with reference to the second in such a manner, that the angle, which it makes, always retains the same value.

If we imagine on the celestial sphere the poles of these two planes, the first describes, with uniform motion, a small circle around the second, so that, when we shall have the observations of two or three centuries, we will be able to trace this circle quite exactly, and obtaining its pole will find the north pole of the planet, which direct observation has been incapable of doing. The data which are at our disposal to-day are insufficient; however, it seems to us probable that the angle of which we have spoken should be from 20 to 25 degrees, and the flattening less than $\frac{1}{85}$. Mr. Newcomb, without making detailed calculations, has assigned the same cause to the phenomenon.

The fifth satellite of Jupiter, discovered so unexpectedly by Barnard in 1893, should show a displacement produced by the same cause. It does not appear that the four large satellites of Galileo are appreciably disturbed in this way; here again it is necessary to have in mind the flattening of the planet, which is considerable in the case of Jupiter. But this flattening produces another effect; it cannot modify the position of the plane of the satellite's orbit, since this small body moves in the plane of the equator; but it can make this orbit turn in its plane, and the calculation shows that it should make a complete revolution of the orbit in about five months. If, then, the orbit is not rigorously circular, but is much or little excentric, it must happen that at a certain time the satellite will recede farther from the planet on the west side than on the east. This is what Barnard has already established. But we can say that 75 days later the inverse will occur; it will be nearest on the west side. I hope that the observations will confirm this prediction if the orbit is at all elliptic.

The effect of which we have spoken should also be produced for the satellite of Neptune, but it is a great deal less pronounced than the change of the plane of the orbit; nevertheless, it will not be long until it is established.

SUNSPOT OBSERVATIONS AT GOODSELL OBSERVATORY.*

H. C. WILSON.

Visual observations of the Sun were begun at Goodsell Observatory in May, 1889, and were continued with few interruptions, save from cloudy weather, until August 20, 1892, when the 8-inch equatorial which was employed for this work was dismounted

* Abstract of introduction to Publication No. 3 of Goodsell Observatory of Carleton College, Northfield, Minn.

for repairs and important modifications. When the remodeled instrument was mounted, in May, 1893, the photographs obtained with it were so excellent, that it seemed useless to continue the visual observations, since the same results could be more easily obtained from the photographs. It has been thought best therefore to publish this series of observations separately, in the hope that it may be of use to investigators in Solar Physics, extending as it does from about a minimum well on toward a maximum of the Sun-spot period.

The observations consisted in counting once each day, preferably near noon, the number of groups of spots, the number of spots in all the groups and the number of faculæ or groups of faculæ visible on the solar disc. The state of the seeing was indicated by noting whether or not the granulation of the solar surface was visible.

The instrument employed was usually the 8-inch equatorial by Alvan Clark & Sons. In the first part of the series a diagonal eyepiece with a neutral tint shade was used. Later, when photographs were being taken regularly, in order to avoid changing the adjustments, the image of the Sun was projected through the photographic combination of lenses, objective and enlarging, on a screen of white paper. This projected image was about 6 inches in diameter and the spots were easily seen and counted, but the faculæ were not quite so well seen as by the direct method.

Photographs of the Sun.

The 8-inch equatorial was provided with a third objective lens, correcting it for the photographic rays of light, in 1887, but it had no enlarging apparatus and no means of making the extremely short exposures necessary for a successful photograph of the Sun. In 1889 an enlarging apparatus was improvised out of an ordinary wide field negative eyepiece and the adapter provided for use in stellar photography. A very convenient and rapid little shutter was made by a local amateur photographer, Dr. H. L. Cruttenden. This was attached to the eyepiece where the emerging pencil of rays was about one-fourth of an inch in diameter. The slide of the shutter was made from a light piece of zinc, two inches long and three-fourths of an inch wide and had a run of one inch. A slit one-half an inch long and a fiftieth of an inch in width was cut across the center of the slide. Later the slit was formed by two pieces of black paper pasted upon the zinc. These were cut from the same piece and laid with the corresponding edges in juxtaposition, so that irregularities in one

edge were matched by opposite ones in the other, and the slit was everywhere of the same width, although the edges were not necessarily straight. The width of the slit, after many experiments, was put at one one-hundredth of an inch, greater width giving over-exposure. The slide was propelled by rubber bands capable of giving it the run of an inch in less than a tenth of a second, so that the exposure for each ray in the pencil of light was less than one thousandth part of a second. The slide was touched off by means of a rubber air bulb and piston. The whole apparatus was so light that no perceptible vibration was produced and it was in every way satisfactory except that the enlarged image was not in a plane and therefore could not all be brought into focus on the photographic plate. The average diameter of the solar image upon the plate was three and one-half inches, and of this the central three inches was in fairly good focus, while the edge was poorly defined.

With this apparatus photographs of the Sun were taken during 1889 and 1890 whenever spots of any considerable size were visible. After July, 1891, they were taken daily when possible. The time of each photograph was noted to the nearest minute.

The sensitive plates used at first were Seed No. 20. Later these could not be obtained and we used Seed No. 22. These also went out of market and toward the last Seed No. 23 were used. Recently we have been using Carbutt's Keystone A plates, sensitometer No. 10, with much more satisfactory results.

A line parallel to the equator was photographed upon each plate, by means of a thread stretched across the tube just in front of the plate. This thread was adjusted parallel to the equator and tested by allowing a sunspot to drift along it when near the meridian. This adjustment once made was changed only when necessary because the photographic attachments had been taken off in order to use the telescope for other purposes.

In 1892 we were able to purchase new enlarging lenses, made especially for the purpose by Messrs. Hastings and Brashear. In order to use these it was necessary to remodel the mounting of the telescope and therefore discontinue the photographs for several months.

The new apparatus gives images of the Sun either three and one-fourth or seven and one-half inches in diameter. The smaller sized image is very sharp in all parts and capable of very accurate measurement. The larger image is not quite plane but is only slightly out of focus toward the edges. Photographs of the smaller size are now being taken daily and we hope soon to have apparatus for measuring them accurately.

MEASURES OF THE PHOTOGRAPHS.

The measures, the results of which are contained in this volume, were made with the aid of a mica scale made by the writer. On a sheet of clear mica, 5 × 6 inches, a series of concentric circles were ruled with a pair of fine pointed steel compasses, the radial measurements being taken from a chronograph scale which was accurately ruled for us by Mr. G. N. Saegmuller. Radial lines were drawn at every tenth degree of the circumference. The radial scale was approximately 1 division = 1.5 mm. On the average 54 of these divisions covered the diameter of the solar image.

The measures were made by laying this scale, ruled side down, upon the film side of the negative, making the 90° — 270° diameter parallel to the equatorial line, centering the solar image within the circle nearest to its circumference, and reading from the scale the position angle and distance of each spot from the center. The scale reading for the edge of the solar disc was read at the same time, this being necessarily the same for all points of the limb except where distortion resulted from refraction. In all cases it was easy to get the average radius.

For the measures the negative was placed in a wooden frame, a little larger than the mica scale, so that the latter could be easily adjusted and held to it. This frame was then held toward a strong light and one person read off the positions and diameters of the spots, while another recorded the readings. This process is extremely rapid and surprisingly accurate for such a simple apparatus. The positions and dimensions of all the spots at all prominent on the most spotted plate can be read off in from five to ten minutes. The maximum error of the scale is one-tenth of a division or about 0°.2 of arc on the solar surface, at the center of the disc. The maximum error of reading the scale is about the same, so that the combined error of scale and reading may be as large as perhaps 0°.5 of longitude at the center of the disc. This error becomes 1°.0 at 65° distance from the center and at the edge of the disk might amount to as much as 5°.0 of longitude. The position angles were read to the nearest half degree so that the maximum error in these and the resulting latitudes at the edge of the disk is about 0°.5.

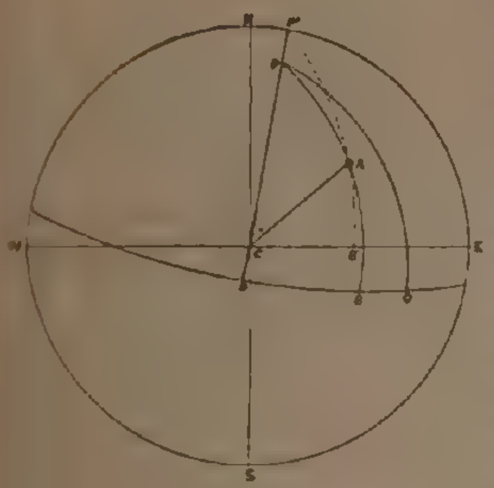
These limits of accuracy are amply sufficient for the study of the distribution of sunspots and for the study of their larger movements. For the great majority of spots, too, the changes in form from day to day are such that it is impossible to identify the same portions of them within the above limits.

For the areas of the spots the average diameters of the penumbræ were estimated. Allowance for foreshortening of spots near the limb was partly made by taking the diameter parallel to the limb.

Reduction of the Measures.

The method of reduction was made as nearly as possible consistent with the accuracy of the measures. The corrections for parallax and refraction were wholly neglected, the former being wholly insignificant, the latter amounting to 0°.1 only when the Sun was within 15° and to 0°.2 within 8° of the horizon, below which altitudes no photographs were taken. Auxiliary tables were prepared which rendered the reductions very easy and rapid, those for a single spot occupying a skilled computer from one to two minutes of time. These tables were prepared in the following manner.

Fig 1



In Fig. 1 let A be the position of a spot upon the solar disc, and C the center of the disc; N, E, S, W represent the north, east, south and west points of the solar image as seen upon the film side of the plate. Let the scale reading for the distance CA be r and that for the radius of the image CE, CN, CS or CW be R. Represent the arc on the solar surface corresponding to AC by s and the angle at the Earth subtended by the same line by s', then

$$AC = \frac{r}{R} = \sin\,(s + s')$$

and

$$s = \sin^{-1} \frac{r}{R} - s' \tag{1}$$

The maximum value of s' will be the apparent solar radius which averages 16′ or 0°.3, and

$$s' : 16' :: r : R \text{ or } s' = \frac{r\,0.^\circ 3}{R} \qquad (2)$$

A table was prepared from formulæ (1) and (2) giving s for every unit of r and for every tenth of unit of R between the extreme scale readings on the edge of the disc. For those values of r which fell between R and $R - 1.0$ the table was extended to tenths of units of r.

Again in Fig. 1 let P be the position of the north pole of rotation, PO the assumed zero meridian for the reckoning of longitudes, and DBO the solar equator. Represent the position-angles of the spot and the north pole of rotation from the north point of the disc by p and P, the heliographic longitude and latitude of the spot by l and d, and those of the center of the disc by L and D; then

$$\begin{aligned} p &= NCA \\ P &= NCP \\ L &= OPC \\ D &= \text{arc } CD \\ l &= OPA \\ d &= \text{arc } AB \end{aligned}$$

The quantities P, D and L were taken from the tables published annually in "The Companion to the Observatory" a publication edited by the astronomers at the *Royal Observatory, Greenwich*, from which we take also the following statement:

"In computing D the inclination of the Sun's axis to the ecliptic has been assumed to be 82° 45′, and the longitude of the ascending node to be 74° 20′. In computing L the Sun's period of rotation has been assumed to be 25.38 days, and the meridian which passed through the ascending node at the epoch 1854.0 has been taken as the zero meridian."

These tables were interpolated to noon of each day at this Observatory and were thus very convenient in use.

In order to find l and d it is necessary to solve the spherical triangle ACP for each spot. We have the following general equations:

$$\begin{aligned} \cos PA &= \cos PC \cos CA + \sin PC \sin CA \cos PCA \\ \sin PA \cos APC &= \sin PC \cos CA - \cos PC \sin CA \cos PCA \\ \sin PA \sin APC &= \sin CA \sin PCA \end{aligned}$$

By the proper substitutions these become

$$\begin{aligned} \sin d &= \sin D \cos s + \cos D \sin s \cos (p - P) \\ \cos (L - l) \cos d &= \cos D \cos s - \sin D \sin s \cos (p - P) \qquad (3) \\ \sin (L - l) \cos d &= \sin s \sin (p - P) \end{aligned}$$

To adapt these to logarithmic computation let

$$\begin{aligned} k \sin \psi &= \sin s \cos (p - P) \\ k \cos \psi &= \cos s \end{aligned} \tag{4}$$

then

$$\begin{aligned} \sin d &= k \sin (D + \psi) \\ \cos (L - l) \cos d &= k \cos (D + \psi) \\ \sin (L - l) \cos d &= \sin s \sin (p - P) \end{aligned} \tag{5}$$

The formulæ to be used in making an accurate reduction are (1), (2), (4) and (5), and in many cases we have used them in order to verify the tables. Ordinarily we have used tables prepared from formulæ (3) in the following manner.

As D is always a small angle, or arc, its maximum value being only a little over 7°, it may be treated as a differential quantity, and its effect upon the quantities d and l may be obtained by differentiating equations (3). This process gives

$$\begin{aligned} &\cos d\, \Delta d = \{\cos D \cos s - \sin D \sin s \cos (p - P)\} \Delta D \\ &\sin (L - l) \cos d\, \Delta l - \cos (L - l) \sin d\, \Delta d \\ &\qquad = - \{\sin D \cos s - \cos D \sin s \cos (p - P)\} \Delta D \\ &\cos (L - l) \cos d\, \Delta l + \sin (L - l) \sin d\, \Delta d = 0 \end{aligned} \tag{6}$$

If now in equations (3) we put $D = 0°$ they become

$$\begin{aligned} \sin d_0 &= \sin s \cos (p - P) \\ \cos (L - l_0) \cos d_0 &= \cos s \\ \sin (L - l_0) \cos d_0 &= \sin s \sin (p - P) \end{aligned} \tag{7}$$

This is equivalent to placing the north pole of the Sun at P' instead of P.

Putting $D = 0°$ in (6) we have

$$\begin{aligned} &\cos d_0\, \Delta d = \cos s\, \Delta D \\ &\sin (L - l_0) \cos d_0\, \Delta l - \cos (L - l_0) \sin d_0\, \Delta d \\ &\qquad = - \sin s \cos (p - P)\, \Delta D \\ &\cos (L - l_0) \cos d_0\, \Delta l + \sin (L - l_0) \sin d_0\, \Delta d = 0 \end{aligned} \tag{8}$$

Substituting from (7) the values of the right hand members of (8) and solving for Δd and Δl we obtain

$$\begin{aligned} \Delta d &= \cos (L - l_0)\, \Delta D \\ \Delta l &= - \sin (L - l_0) \tan d_0\, \Delta D \end{aligned} \tag{9}$$

Then for any variation of D from 0° we will have

$$\begin{aligned} \Delta D &= D - 0° = D \\ d &= d_0 + \Delta d \\ l &= L - (L - l_0) + \Delta l \end{aligned} \tag{10}$$

The quantities d_o and $(L - l_o)$ were computed by equations (7) for every 5° of s and $(p - P)$ and tabulated with the horizontal argument s and vertical argument $(p - P)$. The quantities computed for one quadrant serve for the others by the proper arrangement of the arguments $(p - P)$ and the signs of the tabular numbers.

The corrections Δd and Δl were tabulated by means of equations (9), the former for every 10° of $(L - l_o)$ and every degree of D, and the latter with the three arguments, $(L - l_o)$, and d_o to every 10° and D to every degree.

The areas of spots were taken from a table prepared by means of the formula

$$A = \frac{a^2}{8R^2}$$

in which a is the measured diameter of the spot and R the measured radius of the Sun. A is expressed in millionths of the visible hemisphere of the Sun.

By means of these tables the reduction of a single spot measure is reduced to the following steps:

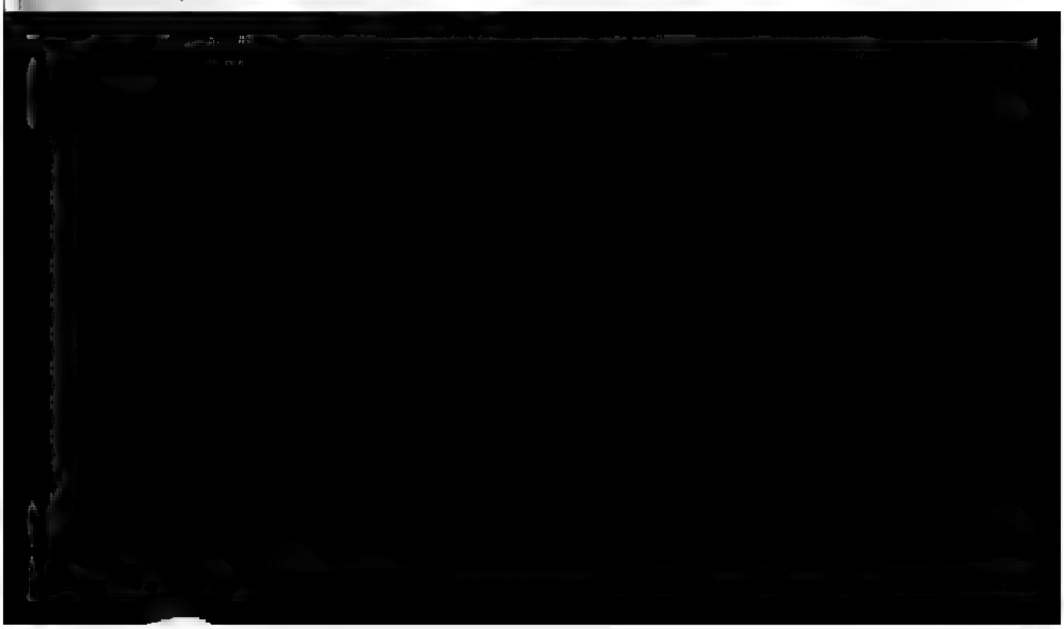
1. Take from Table I the quantities P, D and L.
2. With the measured radii r and R take s from Table II.
3. Subtract P from p.
4. With arguments s and $(p - P)$ take $(L - l_o)$ from Table III and d_o from Table IV.
5. With arguments $(L - l_o)$, d_o and D take Δl from Table V and Δd from Table VI.
6. Subtract $(L - l_o)$ from L.
7. Add Δl and Δd to l_o and d_o.
8. Take A from Table VII with arguments a and R.

More rigorous values of Δl and Δd may be obtained by performing step 5 a second time, using as arguments $(L - l_o - \frac{1}{2}\Delta l)$, $d_o + \frac{1}{2}\Delta d$ and D, but this second approximation will usually be unnecessary.

When there are many measures to be reduced the work is expedited by performing each step for a large number of measures at once.

Comparison with the Greenwich Results.

The latest volume of the Greenwich Spectroscopic and Photographic Results at hand is that for 1890. A comparison of the few measures made on common dates in that year is given in the following table, in which Δl and Δd are the corrections required



:educe our measures to those made at Greenwich. In several es the spots were very irregular and it is evident that in meas- ıg the photographs the observers did not take the same por- ıs of the spots. The average difference taken regardless of ıs, is 0°.91 for *l* and 0°.53 for *d*. This is larger than the aver- differences between our own measures of the same spots on :rent days and shows, probably, that the errors due to per- ality in locating the point for measurement in the spot are ater than those due to the method of measurement and reduc- ı.

Date.			Group.	$L-l$	l	d	Δl	Δd
				°	°	°	°	°
;90	Sept.	9	18	+ 13.0	264.6	+ 22.0	− 1.3	+ 0.7
		23	19	+ 44.0	48.2	+ 21.8	− 0.5	+ 0.5
		27	..	+ 1.1	39.3	+ 21.5	+ 0.2	0.0
			–	+ 4.1	36.3	+ 20.3	+ 0.1	+ 0.3
			..	+ 6.3	34.1	+ 21.5	− 0.8	+ 1.2
	Oct.	20	20	+ 62.8	35.4	− 22.2 }	− 1.3	− 0.1
			..	+ 67.8	30.0	− 23.1 }		
		21	..	+ 48.6	35.2	− 21.3	− 0.6	− 0.9
			–	+ 50.9	32.9	− 19.1	− 0.9	− 1.7
			..	+ 56.4	27.4	− 23.1	+ 1.2	− 0.5
		22	..	+ 33.8	35.4	− 20.0	+ 1.3	− 0.2
			..	+ 37.1	32.1	− 18.8		
			..	+ 42.0	27.2	− 22.7	+ 1.3	+ 0.5
		23	–	+ 21.6	35.9	− 21.4	− 0.9	− 0.9
			..	+ 24.6	32.9	− 19.6	− 1.1	− 0.8
			..	+ 30.2	27.3	− 23.8	0.0	− 0.7
			22	+ 30.0	27.5	− 4.9	− 1.7	− 0.5
	Nov.	26	23	+ 7.8	320.6	+ 18.8	− 0.9	+ 0.1
			..	+ 14.8	313.6	+ 17.9	− 0.6	+ 0.2
			..	+ 15.2	313.2	+ 20.8	− 2.1	0.0
			..	+ 23.2	305.2	+ 22.4	− 0.5	+ 0.2

Summary of the Observations.

n the following tables the columns are self explanatory, with · exception of the last. This column gives the average area per ŗ of all the spots, on the days when photographs were taken, 1 on the days when photographs were not taken because no ›ts were visible. The areas are expressed in millionths of the a of the visible hemisphere of the Sun.

'he progress of the average daily numbers and areas of spots ;hown graphically in Fig. 2, the dotted line representing the .ly number of spots, and the smooth line the areas. In order plot them to the same scale the daily numbers were multiplied 3 and the areas divided by 10. Although the fluctuations are ite marked, especially in July, 1891, and February and June, 92, there is a pretty steady increase in both the number and :a, during the period of two and a half years, which might be

quite fairly represented by a straight line. As a rule the [illegible] and area change together.

Year.	Month.	No. of Days of Visual Obs.	Average No. of Groups.	Average No. of Spots.	No. of days when no spots were seen.	[illegible]
1889	May	3	0.3	0.3	2	[illegible]
	June	9	0.6	3.0	4	[illegible]
	July	4	0.5	6.5	2	[illegible]
	Aug.	20	1.3	7.0	4	11
	Sept.	18	0.8	6.5	8	3
	Oct.	9	0.3	0.6	6	[illegible]
	Nov.	0				
	Dec.	6	1.5	12.7	2	[illegible]
1890	Jan.	13	0.7	2.2	7	[illegible]
	Feb.	14	0.3	0.5	9	[illegible]
	March	10	0.5	1.8	5	[illegible]
	April	15	0.4	2.1	[illegible]	[illegible]
	May	12	0.5	1.2	7	[illegible]
	J[illegible]	13	0.2	1.2	[illegible]	[illegible]
	July	14	1.1	6.6	3	[illegible]
	Aug.	19	1.0	10.0	6	[illegible]
	Sept.	13	1.2	6.0	3	[illegible]
	Oct.	10	1.2	8.4	3	3
	Nov.	15	0.5	3.4	7	3
	Dec.	15	1.0	4.5	3	[illegible]
1891	Jan.	13	1.0	4.1	5	2
	Feb.	4	1.2	6.2	0	4
	March	1	0.0	0.0	1	2
	April	13	2.2	16.1	0	3
	May	16	3.6	14.1	0	[illegible]
	June	4	3.8	19.0	0	4
	July	14	3.0	22.1	0	10
	Aug.	22	2.8	6.8	0	19
	Sept.	18	3.9	14.2	0	16
	Oct.	16	3.3	10.4	0	16
	Nov.	14	3.4	11.3	0	12
	Dec.	15	2.4	8.7	0	14
1892	Jan.	21	5.3	16.3	0	21
	Feb.	11	4.5	24.0	0	12
	Mar.	16	3.6	12.3	0	17
	April	15	5.1	16.7	0	16
	May	14	4.7	24.0	0	14
	June	20	5.1	16.4	0	19
	July	23	5.0	23.0	0	23
	Aug.	18	6.9	23.0	0	20

Distribution in Latitude of Sunspots.

Latitude.	Number of Groups.					Area of all Groups.				
	1889	1890	1891	[illegible]	Total	1889	1890	1891	[illegible]	Total
°										
+35	0	0	0	0	0	0	0	0	0	0
30	0	1	5	1	7	0	241	1149	331	1721
25	0	1	13	12	26	0	2	2234	1775	4011
20	0	4	30	15	49	0	2077	7055	1565	10697
15	0	1	17	15	33	0	204	4169	2570	6943
10	0	0	3	18	21	0	0	181	5408	5589
+5	0	1	0	2	3	0	2	0	6	8
0	0	0	0	0	0	0	0	0	0	0
−5	1	1	0	2	4	542	7	0	43	592
10	3	[illegible]	2	7	12	826	[illegible]	327	799	1952
15	0	0	2	15	17	0	[illegible]	1192	2073	3265
20	2	3	11	23	39	915	637	4090	3493	9135
25	2	1	2	16	[illegible]	357	2	1051	2158	3568
30	0	0	1	8	9	0	0	16	3854	3870
−35	0	0	0	0	0	0	0	0	0	[illegible]

In this table the latitudes include 2°.5 on each side of the number given. The area of each group was taken on the day when it was nearest the center of the disc except in a few cases when there was so great change that some other date would give more nearly its maximum area. The totals for the four years clearly indicate the maximum number and area of spots at about latitude 20° north and south. The secondary maximum at — 30° in 1892 was produced by the extraordinary group of February. In the same year a remarkably large number of groups occurred in latitude + 10°, producing maxima for the year in that latitude in both the number and total area of groups. The preponderance of the number of groups in the southern hemisphere over that in the northern hemisphere of the Sun in 1889 has been remarked by other observers. The results here given for that year are too meager to be of much weight. Summing the results in north and south latitudes we see that in 1890 the greater number and greater area of spots were on the northern hemisphere. The same was true in 1891 but in 1892 the spottedness of both hemispheres was about the same.

Year	No of Photographs.	No. of groups. North.	No. of groups. South.	Total Area. North.	Total Area. South.
1889	26	0	8	.000000	.002640
1890	27	8	5	.002526	.000646
1891	122	68	18	.014788	.006676
1892	142	63	62	.011655	.012420

Distribution in Longitude of Sunspots.

A similar study of the distribution of sun-spots in longitude[1] revealed no tendency toward the continued recurrence of groups in the same longitudes for periods of more than a few months, the maxima for one year often coinciding in longitude with the minima for the next. In this investigation the spots in north and south latitudes were considered separately and together. The great solar disturbances durirg the period 1889–1892 occurred in longitudes $30° - 40°$, $80° - 90°$, $150° - 160°$, $180°$, $200°$ and $220° - 230°$ in the northern hemisphere, and $30° - 40°$, $80°$, $160°$, $250° - 270°$, and $280°$ in the southern hemisphere.

The Great Group of February, 1892.

We have not attempted to study the movements of the spots, except in the case of the great group, No. 136, of February, 1892. This group was remarkable for the great extent of the disturbed area, it being the largest that has occurred for many years. Its passage across the central meridian of the Sun was accompanied on the Earth by a violent magnetic storm and a brilliant display of the aurora borealis. Measures of the photograph of Feb. 11 when the group was near the center of the Sun's disc give for the dimensions of the single large penumbra 72,000 by 33,000 miles, while the total disturbed area was 135,000 miles long by 80,000 miles wide.

The changes in the details of the group were so rapid that it is difficult to identify the same spots from day to day. There were two large and very black umbræ, *A* and *B*, surrounded by a large penumbra, which marked two notable centers of disturbance. Their apparent motions were quite different, as will appear from Fig. 3, in which the longitudes and latitudes on the different dates are plotted. The spot *A* moved due east, decreasing about $10°$ in longitude during the interval Feb. 5 to 16. The spot *B* in the same time described a looped curve, changing from a position due north of *A* to one almost due west of the latter. The great penumbra surrounding these two centers shared the rotary motion thus indicated. The spots *C* and *D* were separate from the great penumbra and did not share in its motion. Each divided into two parts between Feb. 9 and 11, or a new spot developed in the rear of each.

The cyclonic movement of the spot *B* is more clearly shown if we reduce the longitudes to a given date, applying corrections for the variation of the period of rotation with the latitude.

The longitudes were determined with Carrington's rotation period 25.38 days. Carrington, however, found that the period varied for spots in different latitudes, the apparent daily motions being represented approximately by the formula

$$\Delta l = 865' - 165' \sin^{\frac{7}{4}} d$$

in which d represents the latitude and Δl the daily motion in longitude from the center of the Sun's disc. Faye found from the same observations

$$\Delta l = 862' - 186' \sin^2 d.$$

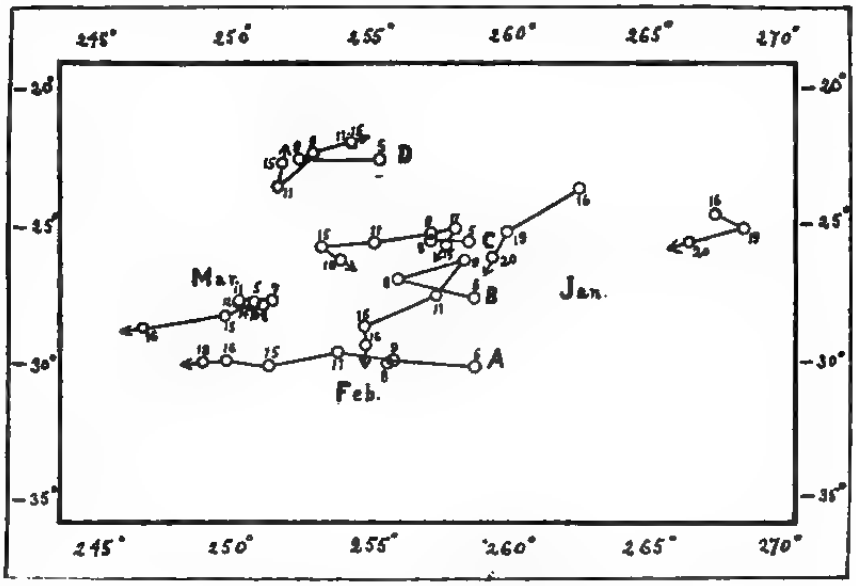


FIG. 3.—APPARENT MOVEMENTS OF THE PRINCIPAL UMBRÆ OF THE GREAT SUN-SPOT GROUP OF FEBRUARY 1892.

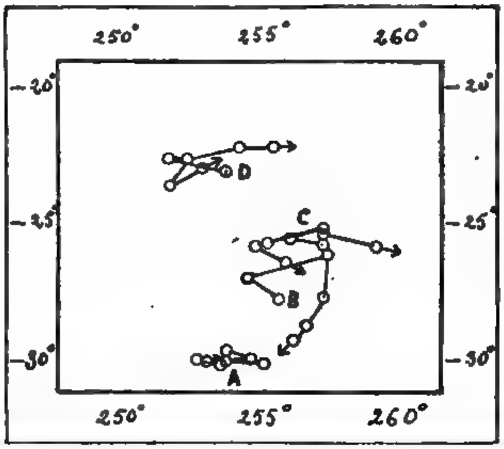
The period 25.38 days corresponds to a daily motion of 851′. The daily correction, therefore, required to reduce the longitude of a spot to the period corresponding to its own latitude is, according to Faye,

$$\Delta_1 l = 11' - 186' \sin^2 d.$$

Reducing the longitudes of the four spots A, B, C and D, to the date Feb. 11 by this formula, we have the following results, in which d is the latitude, l the longitude by the 25.38 day period and l_1 the longitude reduced to Feb. 11.

	A d	A l	A h	B d	B l	B h
	°	°	°	°	°	°
Feb. 5	− 30.2	259.1	255.6	− 27.7	259.1	256.1
8	− 30.0	256.0	254.2	− 27.0	256.4	254.9
9	− 29.9	256.3	255.1	− 26.2	258.8	257.8
11	− 29.6	254.2	254.2	− 27.6	257.7	257.7
15	− 30.1	251.7	254.0	− 28.7	255.1	257.1
16	− 29.9	250.1	253.0	− 29.3	255.2	256.7
18	− 30.0	249.3	253.4			

	C d	C l	C h	D d	D l	D h
	°	°	°	°	°	°
Feb. 5	− 25.7	260.0	257.6	− 23.1	255.9	254.2
8	− 25.5	257.7	256.5	− 22.6	252.8	252.0
9	− 25.2	258.5	257.7	− 22.5	253.3	252.7
11	− 25.4	257.7	257.7	− 22.2	254.7	254.7
	− 25.7	255.6	255.6	− 23.6	252.0	252.0
15	− 25.8	258.2	259.6	− 22.2	254.8	255.9
	− 25.8	255.3	255.3	− 22.9	252.2	253.3
16	− 26.4	254.4	256.4			



The positions thus corrected have been plotted in Fig. 4. The displacement of *A* has, by this process, been reduced to an amount which may, perhaps, be accounted for by the errors of measurement. The spot *B*, however, takes on a decided cyclonic curve. The large displacement between Feb. 8 and 9 is not a displacement of the

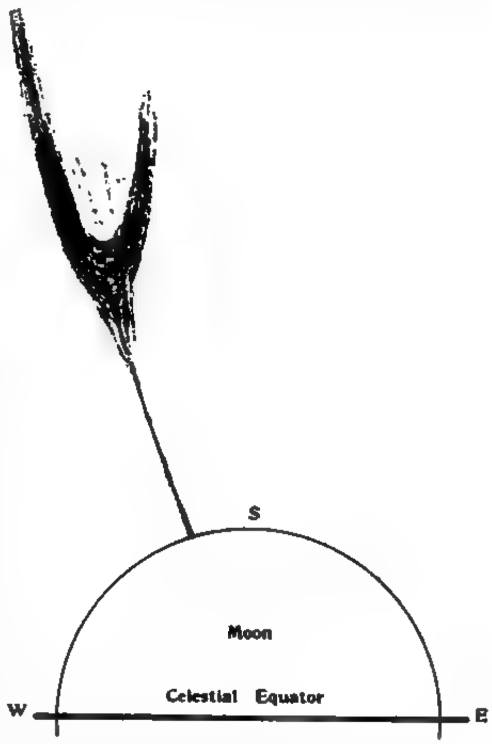
At its reappearance in March the group was much changed and none of its prominent spots can be identified with individual spots of the February group. The most prominent umbra was in the latitude of *B*. Its apparent course during the interval March 5 to 16 is shown in Fig. 3.

A COMETARY STRUCTURE IN THE CORONA OF APRIL 16, 1893.

J. M. SCHAEBERLE.

In the October number of this journal I called attention to a comet-like structure near the Sun during the total eclipse of last April, as shown on all the Lick Observatory photographs of the outer corona.

The form and position of this object is shown in accompanying sketch. The straight, slender, nearly radial streamer, from the Moon's outline to the structure in question, is conspicuously visible and distinctly isolated from the more inclined neighboring streamers not shown in the sketch. The drawing is made from an original negative taken with the 40 ft. telescope. On the Dallmeyer negatives the tail-end of the structure, which for the 40-ft. telescope falls outside of the limits of the 18 × 22 inch plate, can be traced for more than a degree from the nucleus or head of the object. Until I have seen copies of the results obtained at the other eclipse stations, I do not wish to express an opinion as to the true nature of this object. Copies of our own photographs were distributed more than eight months ago, but repeated requests for copies of the results obtained at the other stations have thus far been in vain.



LICK OBSERVATORY, March 16, 1893.

MELTING OF THE POLAR CAPS OF MARS.*

WILLIAM H. PICKERING

In ASTRONOMY AND ASTRO-PHYSICS for 1892, p. 668, is given an account of a series of conspicuous changes that were observed upon the surface of Mars at the time of the melting of its southern snow cap. These changes were so marked that many of them could be readily observed this year with any moderately large telescope, although the position of the planet at this time will be much more unfavorable than was the case in 1892. In that year upon July 12 a central branch made its appearance in the peak of the Y mark. This branch would lie just south of Noachis upon Schiaparelli's map. It was soon seen that this central branch formed a portion of a dark line connecting the great split in the southern snow cap with the Northern Sea. This sea lies in the northern portion of the Syrtis major, and is much darker than any of the surrounding regions. Immediately upon the formation of this connecting link, a series of striking changes occurred in the shape and color of the regions surrounding the Northern Sea. These changes are fully described in the article referred to above, and no further description of them is necessary in this connection. The apparent alterations from night to night were very marked, and the whole series of changes was completed inside of two weeks.

The point to which I wish to call the attention of astronomers at the present time is that upon May 30, 1894, Mars will reach the same portion of its orbit with regard to the Sun that it did upon July 12, 1892. It is therefore presumable that a similar series of changes will occur about that date. As Mars will be morning star at this period, rising about midnight, and will at the same time, be rather remote, it is not likely that there will be many observers watching it, and for this reason every available observation will be of much greater value than it would be under other circumstances. The center of the Northern Sea, longitude 290°, is central May 30d. $17^h.5$ E. S. T., therefore if the expected changes occur on time this year the eastern astronomers must look for them chiefly by daylight, while the western astronomers may look for them rather earlier in the morning. There is no reason, however, for expecting any meteorological phenomenon to occur on precisely the same date upon two successive years, and should the thaw begin earlier this year upon Mars than it did last, it will be to the advantage of the eastern astronomer.

CAMBRIDGE, MASS., March 22, 1894.

* Contributed by the author.

Astro-Physics

ON A COMBINATION OF PRISMS FOR STELLAR SPECTROSCOPE.*

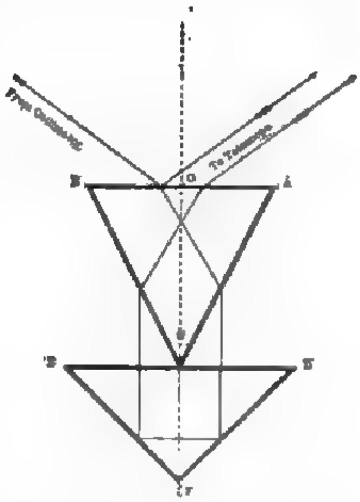
H. F. NEWALL.

The arrangement of prisms which is the subject of this note, has not, so far as I am aware, been described before, and its conveniences for astronomical purposes in particular are so numerous that I propose to give some details concerning it.

ABC is a strongly dispersing prism; all three faces are accurately worked; and the angles at A and B are equal.

DEF is an ordinary double total reflection prism.

These two prisms are relatively fixed as shown in the accompanying section with the faces A B and DE parallel (or approximately so; see final paragraph); the edges of the prisms are also parallel and further the edges C and F lie in a plane GCF which passes through the middle, G, of the face AB and is perpendicular to AB.



Prisms relatively fixed; the combination capable of rotation about G.

The combination thus formed is symmetrical about the plane GCF, and is made capable of being turned about an axis which is the intersection of the plane GCF with the face AB. Thus in the figure the section turns in the plane of the paper round G.

Light from a collimator falls on the face AB as indicated in the figure, (the *central* ray, however, falls on G), and the incidence is suitably adjusted by turning the combination about G; the usual telescope is adjusted to view the spectrum. The light from the collimator suffers deviation and dispersion in passing through the prism of refracting angle A; it is then twice reflected within the reflecting prism and again suffers deviation and dispersion in passing through the prism of refracting angle B.

The spectrum seen with the telescope is therefore similar to what would be seen in a two-prism spectroscope whose prisms

* (Read on 29 Jan., 1894, before the Cambridge Philosophical Society, England.) Communicated by the author.

are of the same material as that used for the prism ABC and
nd angles equal to A and B respectively.

The telescope receives, when it and the prism-combination are
properly adjusted, not only the light which has passed *through*
the combination, but also the light reflected from the face AB at
primitive incidence. The reflected light gives rise to a simple
image of the slit which appears to be superposed on the spectrum;
for ease in description and for an obvious reason this will be
called the 'pointer.' If the prism combination is turned through
a small angle $\delta\theta$, the pointer moves through the angle $2\delta\theta$, but
the spectrum moves through a much smaller angle, whose magnitude varies with the part of the spectrum considered. Hence
the pointer can be made to coincide with any line in the spectrum,
and its change of position is known in terms of the corresponding change of position of the prism-combination. If, therefore, a
suitable micrometer movement is used to move the prisms the
position of the pointer may be read off the micrometer.

The line in the spectrum which coincides with the pointer is
always that which is due to rays which have passed symetrically
through the prism combination. The movement of the prisms
gives symetrical passage to different rays in turn, and the pointer
indicates which ray has passed symetrically. The course of rays
which pass symetrically through the combination is shown in
the figure; such rays emerge from the face AC in a direction parallel to the plane of symmetry, GCF, and consequently the deviation in passing through the prism A is equal to the angle of primitive incidence:

$$D = \varphi = \varphi + \psi - A;$$

whence $A = \psi$, or the angle of emergence from the face AC is equal
to the angle A. The angle of emergence can be made under readily calculable conditions, equal to the angle of primitive incidence,
but generally only with rigid accuracy for rays of one refrangibility; hence the rays which pass symetrically will not in general
pass with absolute minimum deviation.

The pointer may be considered as being connected with the
prism and independent of the observing telescope. It is thus attached, so to speak, to the strongest part of the instrument
instead of the weakest, where the micrometer is usually placed,
namely the eye-end of the telescope. The telescope is used merely
as a magnifyer. The need for carefully worked surfaces for the
prisms forms perhaps the strongest objection to the use of this
combination; curvature in any one surface of either prism must

throw the pointer and spectrum into different focal planes in the observing telescope and so introduce parallax difficulties which can only be eliminated by reworking the faulty surface. Two prisms which I have in my possession and which were worked by Hilger, have been used to test the capabilities of the combination and give excellent results.

The observing telescope is pointed towards G and is mounted so as to turn about the same axis through G as that about which the prisms turn; this single motion is enough to bring every part of the spectrum in turn into view. Thus the advantages of a two-prism spectroscope are obtained without the disadvantages arising from the usual double adjustment necessary in directing the observing telescope. The combination which I describe may therefore replace a grating in a diffraction spectroscope.

If a bright star is observed, both spectrum and pointer are bright; for a faint star, the brightness of the pointer is appropriately subdued. The fact that the brightness of the pointer maintains a suitable proportion to the brightness of the spectrum to be investigated is a great convenience.

In astronomical work, the object glass of an equatorial is used to throw an image of the star, whose spectrum is to be studied on the slit of the spectroscope. If the slit is widened, the image of the star itself is seen in place of the pointer. This is a great convenience, inasmuch as in most cases the star may be thus identified. When the star is recognized amongst its neighbors, the slit is closed to a suitable width and the pointer then appears as a short narrow line in the spectrum. In practice it is preferable to have the pointer not actually superposed on the spectrum, but displaced so as to be a little above or below the spectrum. This end may be attained by slightly tilting the reflecting prism.

OBSERVATIONS OF THE NEW STAR IN NORMA.*

W. W. CAMPBELL.

Observations of the new star in Norma were first attempted here February 13th, 18^h. The star was found without difficulty, although its true altitude when on the meridian is less than 2½ degrees. Its light was estimated at one-fifth or one-sixth that of the 8 mag. star A. G. C. 10940. The Nova would therefore be of the 9½ or 10 magnitude.

* Communicated by the author.

Its spectrum consisted of an exceedingly faint continuous spectrum in the yellow and green, and four bright lines apparently identical in position and relative intensity with the bright lines 575, 501, 496, and 486 in the August, 1892, spectrum of Nova Aurigæ. Rough measures of the wave-lengths of the two brightest lines, made after daylight, gave 5013 and 4953.

The star was seen again for a few minutes between clouds on February 28th, 17^h. Its magnitude remained unchanged at about 9½. The faint line in the yellow was not seen with certainty this morning, possibly owing to light clouds. Two hasty settings of the micrometer wire upon each of the three bright lines in the green and blue gave the following intervals:

1st. (intensity 10) – 2nd. (intensity 3) = 49 tenth metres;
2nd. (intensity 3) – 3d. (intensity 1) = 100 " "

Keeler's intervals for the nebular lines are respectively 48.0 and 97.5 tenth-metres.

On March 2d, $16^h 30^m$, the star was seen, magnitude unchanged, but fogging of the object glass prevented measures. The transparency of our atmosphere is shown by the fact that neighboring stars were visible down to about the 9.5 magnitude in the 4-inch finder, though the Nova could not be seen with certainty.

March 6th, 16^h. A hazy sky made the spectrum very faint. Eight micrometer comparisons with the adjacent lead line gave the wave-length 5007.3 for the principal Nova line. These different settings on the second Nova line gave for it the wave-length 4957. The third line, $H\beta$, was too faint for observation. Magnitude of star unchanged.

There can be no doubt that the spectrum of Nova Normæ is nebular.

LICK OBSERVATORY, 1894, MARCH 7.

RESULTS OF SOLAR OBSERVATIONS MADE AT THE ROYAL ROMAN COLLEGE IN THE FOURTH QUARTER OF THE YEAR 1893.*

P. TACCHINI.

I have the honor to send you the results of our solar observations for the fourth quarter of the year 1893. In November the weather was not very favorable, but in October and December it was really splendid.

* Communicated by the author.

1893	Days of observation	Relative Frequency of Spots	Relative Frequency of days without spots	Relative Size of Spots	Relative Size of Faculæ	No. of groups per day
October	27	26.85	0.00	112.7	89.2	7.4
November	20	23.15	0.00	96.4	84.0	5.4
December	27	[illegible]	0.00	166.4	86.0	8.0

The phenomena of the spots, although still of considerable frequency, have suffered some diminution in comparison with the preceding quarter. It is well to note the secondary maximum in the month of December, for after the other maximum in the month of August, the spots continued to diminish until some time in November. In the period of maximum solar activity, the Sun has always appeared with spots and large pores (trous). Following are the results of the observations of protuberances:

1893	Days of observation	Protuberances Average number	Protuberances Average height	Protuberances Average breadth
			"	°
October	22	5.82	[illegible]	1.9
November	13	5.00	34.7	1.8
December	23	6.48	[illegible]	1.9

The observations show, therefore, that the phenomena of the protuberances have undergone a diminution. It should be noted, however, that a secondary maximum occurs in the month of December, as in the case of the spots, and that a similar coincidence occurred in the month of August. The only protuberance worthy of special note was observed on the 26th of December, in position angle 288°. Its variations in height are shown in the following table:

h	m		"
11	20	height =	87.8
11	41		133.0
11	47		141.1
11	54		79.8
12	9		66.5

The maximum apparent velocity of ascent was 26 kilometres per second; of descent, 109 kilometres. At 278° there was another protuberance, the height of which at $11^h\ 22^m$ was 109″, and which at $11^h\ 48^m$ had entirely disappeared. No metallic lines were observed in the spectrum of the limb of the Sun at the place of the two prominences; the maximum solar activity is therefore quite different in character from what it was before. The spots at the limbs of the Sun appear almost always in a state of quiescence, and it is probably for this reason that on the Earth we have had no strong magnetic disturbances or remarkable auroras.

ROME, Feb. 15, 1894.

ON THE VISUAL APPEARANCE OF NOVA AURIGAE.

WILLIAM HUGGINS

Some discussion has taken place as to the visual appearance of this object since it was re-observed in the autumn of 1892. At the Lick Observatory and also at Pulkowa, the Nova was seen to differ in appearance from a star of similar magnitude, and to have taken on the appearance of a small bright nebula consisting of a nucleus surrounded with a pretty bright and dense nebulosity 3″ in diameter (A. N. 3118, p. 408, and 3184, p. 263).

Mr. Newall, on the contrary, observing with the 25-inch Newall refractor at Cambridge on Sept. 14th, 1892, says: "With a power of 215 I, at first, thought that the Nova was diffuse and resembled a planetary nebula rather than a star; but on focussing more carefully I made out that the Nova was distinctly stellar; now, however, the neighbouring stars resembled planetary nebulæ. In fact the Nova and neighboring stars could not be focussed simultaneously. The Nova owes its visual magnitude nearly entirely to the light that gives rise to the three green lines, and it was possible to verify a conclusion drawn from this fact and from the nature of the chromatic dispersion of a refractor 29 feet focal length; the image of the Nova was distinctly more point-like than that of the neighbouring bright star when each in turn was focussed as carefully as possible." (*Nature*, vol. 46, p. 489).

Dr. Roberts photographed the star on Oct. 3d, 1892, with an exposure of 110 minutes; and on Dec. 25th, 1892, with an exposure of 20 minutes; the diameters of the photo-images being 21″ and 13″ respectively. Dr. Roberts concludes, "there is no indication of nebulosity round the Nova or in its vicinity. It appears as sharply defined as the other stars." (*Monthly Not.* vol. LIII, p. 123.)

Professor Vogel still maintains (A. N. 3198, p. 76) as an explanation of the nebulous appearance seen at the Lick Observatory and at Pulkowa, the view which he suggested in his paper "*Ueber den neuen Stern in Fuhrmann*" (*Abhandl. d. Kgl. Akad. d. Wissensch.* Berlin, 1893, p. 46): "*dass die beobachteten Hüllen um den Stern nichts anderes als die chromatischen Abweichungskreise gewesen sind.*"

It may therefore be of interest for me to state the results of a careful scrutiny of the image of the Nova in a reflecting telescope which gives very fine definition. This telescope has an aperture of 18 inches, with both mirrors of speculum metal arranged in

the Cassegrain form. Mrs. Huggins and myself on Jan. 12th, 1894, compared the image of the Nova with that of the small star 85″ *n. f.* which was only a little less bright, observing with a series of eye-pieces magnifying from 100 up to about 700 diameters. The Nova, which of course, came to focus absolutely with the star, presented always an appearance precisely similar. We remarked particularly that with the highest power the image was as small and as sharply defined a point as that of the star. On the night of Jan. 12th, definition was excellent here; and beyond all doubt under the conditions above described, the Nova appeared as a true star.—*Observatory* for March, p. 108.

LONDON, S. W., 90 Upper Tulse Hill, 1894, Jan. 18.

ASTRO-PHYSICAL NOTES.

All articles and correspondence relating to spectroscopy and other subjects, properly included in ASTRO-PHYSICS, should be addressed to George E. Hale, Kenwood Observatory of the University of Chicago, Chicago, U. S. A. Authors of papers are requested to refer to last page for information in regard to illustrations, reprint copies, etc.

Note on Nova Aurigæ.—I have just made the interesting discovery that Nova Aurigæ appears on a plate which I exposed Jan. 5, 1892, on the north coast of Norway, for the purpose of photographing the northern light. It has been my intention for a long time to look over my plates with this object in view, but on account of other affairs I have but just now been able to undertake the work.

Unfortunately, the Nova is near one edge of the plate; χ Aurigæ is not on it at all. The Nova is however fairly distinct, since stars below the eighth magnitude can sometimes be recognized without great difficulty. The photograph was taken on an orthochromatic plate, sensitive to yellowish green light. Martin Brendel, Greifswald, 1893, Dec. 9. [Translated from *A. N.* 3209].

Reproduction of Astronomical Photographs.—The *Observatory* and the *Journal of the British Astronomical Association* reproduce some of the photographs of the corona taken by Professor Schaeberle in South America on April 16, 1893, but any one who has seen Professor Schaeberle's beautiful negatives will hardly find these reproductions satisfactory, as the detail of the inner corona is wholly lost. The difficulty is one inherent in photographic processes. Strong contrasts can be successfully dealt with, but an attempt to reproduce photographs of nebulæ or of the corona, in which delicate gradations of light occur, generally results in failure. Professor Holden, writing on methods of representing the Milky Way, in No. 34 of the *Publications of the Astronomical Society of the Pacific*, says, "If drawings are reproduced by photography, the very first copy on a sensitive plate changes all the contrasts of the original design. It is usual to send this first negative to the person who is to make the process block for printing, and who must make another copy on a 'stripping plate,' or on something equivalent. These stripping plates are usually very slow, and the contrasts are again much

changed by the transfer. Finally, the block is made, and in the course of printing the impressions new changes of contrast come in, not to speak of great losses of definition. If the original is a negative, and not a drawing, difficulties of precisely the same sort are present. Definition is always lost and the contrasts are always changed, more or less. Our experience at the Lick Observatory has been considerable, and we have found reproductions by heliogravure (on copper) to be the most satisfactory. They are hardly more than twice as expensive as the best 'processes,' and they are very much superior."

As original negatives of astronomical subjects can find their way into the hands of comparatively few persons, the question of satisfactory methods of reproduction is one of very considerable importance. Mr. Ranyard is doing valuable service by publishing in nearly every number of *Knowledge* really beautiful plates representing the latest triumphs of astronomical photography.

An Astronomical Expedition to Arizona from Harvard College Observatory.*—A party in charge of Professor W. H. Pickering will soon set out from Harvard College Observatory, to establish an observing station somewhere in the state of Arizona, the principal object of the expedition being to observe Mars during the favorable opposition next summer. For the following details we are principally indebted to an article in the *Boston Herald*.

The success of the observing station at Arequipa has induced Professor Pickering to make the experiment of founding a similar station at a high altitude somewhere within the limits of the United States. Trial on Pike's Peak showed that the "seeing" was not good there, (in this respect agreeing with the experience of Professor Hale), and preference has been given to the dry climate and clear skies of Arizona. The exact location has not yet been decided upon, as it has been thought best to make some preliminary tests with a small instrument. Mr. A. E. Douglass will take a six inch telescope for this purpose to Prescott, Phoenix and Tucson, and the site chosen will depend upon his report.

The chief instrument of the expedition will be a fine eighteen inch refractor by Brashear, the objective of which was exhibited at Chicago. Mr. Brashear will also provide a serviceable mounting and many of the accessories.

It is stated that the funds for the expedition have been generously provided by Mr. Percival Lowell of Boston, a gentleman who is deeply interested in astronomy, and has contributed to its literature. Mr. Lowell will himself accompany the expedition as an observer.

Stonyhurst College Observatory.—The latest publication of this Observatory consists mainly of meteorological observations. We learn however, in the introduction, that the objective of the Father Perry Memorial telescope arrived in the beginning of November, and was placed in position on the 6th. It has a clear aperture of 15⅛ inches, was made by Sir Howard Grubb, and although the atmospheric conditions have not yet been sufficiently perfect for a complete test of its defining powers, is undoubtedly of the highest excellence. This telescope is mounted on the pier of the old eight-inch telescope which it replaced, the pier being so massive that it carries the new and much larger telescope without difficulty.

Since May, 1893, all clear nights have been devoted to a photographic study of the spectrum of β Lyræ. The results have been published in the *Monthly Notices* of the Royal Astronomical Society (Dec. 1893).

* This expedition will not be under the direction of Harvard College Observatory, but under that of private parties.—Ed.]

New Telescope for Greenwich Observatory. An editorial note in the *Observatory* says:—"We have reason to know that Sir Henry Thompson, the eminent surgeon has offered the magnificent sum of 5,000 pounds sterling to the nation, through the Astronomer Royal, for the purpose of buying a telescope for Greenwich Observatory. It is not often astronomy finds such a generous patron, on this side of the Atlantic at least, and moreover, one who can so well appreciate the exact needs of the science at the moment. For Sir H. Thompson foreseeing that the astronomy of the future is to be photographic, and feeling that England should be well equipped in this arm, makes it a condition of his gift that the telescope is to be expressly designed for photographic purposes. So far as the plans are made, and subject to the acceptance of the offer by the government, the instrument is to be of 26 inches aperture, just twice that of the telescope used for the Photographic Chart of the Heavens—in fact the instrument (which will probably be made by Sir H. Grubb) is to be made from the model of Astrographic Equatorial, but of exactly double the demensions in every particular. The guiding telescope for the new instrument will be the 12⅘-inch Merz refractor with a light tube, and the 9-inch photographic objective presented by Sir H. Thompson to the Royal Observatory some three years ago will also be carried on the same mounting for use as a photoheliograph as at present.

The new instrument, when completed, will be housed under the Lassell Dome, on the top of the central octagon of the new Physical Observatory, now being built in the south grounds of the Royal Observatory.

Anderson's Variable in Andromeda.—The variability of Anderson's star in Andromeda has been confirmed by Professor Pickering. The star appears on eight plates made at Harvard College Observatory and from these the maximum photographic brightness has been determined to be 9.0 while other plates show that the minimum is below 12 mag. The Harvard photographs and the observations quoted by Dr. Anderson as having been made at Bonn and Cambridge are satisfied by a period of 281 days. A maximum occurred on March 30, 1894.

A remarkable feature of this star, according to Professor Pickering, is the uniformity of the variation of its light. During the three months following the maximum the diminution in light expressed in magnitudes was perfectly uniform, and at the rate of one magnitude in 25 days. The increase during the three months preceding the maximum was also uniform, and at the rate of one magnitude in 26 days. The magnitude at minimum brightness, assuming these laws to hold throughout the period of variation, would be 14.5.

In a note following Professor Pickering's article in *A. N.* 3213, Dr. Hartwig gives a few observations which lead him to think that the period of the star may be 74.4 days, but they are not sufficiently numerous to justify a difinite conclusion.

Mr. Hadden's Solar Work.—From recent private letter we learn of Mr. David E. Hadden's recent work with the spectroscope. He says: "I am now in possession of a very good two-inch grating by Mr. Brashear, and am much interested in spectroscopic observation, especially in noting the reversals of the C line in the umbra of the large spot now on the Sun's surface (Feb. 23). I have also secured some fairly good photographs of the Sun showing the large group now visible with a photoheliograph of my own construction. I am using a 3 inch Jena glass visual objective (Brashear) and enlarging lens at focus, and secure photographs about two and one half inches in diameter."

CURRENT CELESTIAL PHENOMENA.

PLANET NOTES FOR MAY.

Mercury will be at superior conjunction, *i. e.*, behind the Sun, May 20 at 9^h 44^m central time. During May this planet will be wholly hidden to the eye by the glare of the Sun, although it is calculated to reach its greatest brilliancy on the 23d.

Venus will be in good position for observation about 4 o'clock in the morning during May. Her phase will increase from about half to two-thirds during the month, while her brilliancy will diminish in the ratio of 137 to 97 in the same time, because of her recession from the Earth. Venus and the waning Moon will be in conjunction May 1st at 5^h 07^m P. M. central time and again May 31 at 3^h P. M.

Mars is also to be observed in the morning. He is about 30° west and 14° south from Venus, in the constellation Capricorn and will move northeast into Aquarius during May. At the end of the month he will be found about half way between the first magnitude stars Fomalhaut (*α Piscis Austrini*) and Markab (*α Pegasi*). Mars will be in conjunction with the Moon March 28 at 2^h 18^m central time. Observers in Central and South America may see the planet occulted at this time.

Jupiter and *Neptune* will be too low in the west during the early evening hours for any satisfactory observations during this month. The tables of the satellites are therefore omitted. On Poole Bros. map for this month, however, the courses of Jupiter and Neptune among the stars are indicated for the six months from April 1 to Sept. 1.

APPARENT ORBITS OF THE SATELLITES OF URANUS IN 1894.

Saturn will be in best position for observation during May, crossing the meridian about 10 o'clock in the first half and 9 o'clock P. M. in the latter half of the month. The rings of Saturn are now pretty well widened out, so that the three parts can be distinguished readily and the Cassini division can be followed all the way around. The elevation of the Earth above the plane of the rings is about 12°. Saturn is in the constellation Virgo about 5° north of the first magnitude star Spica, with which he is almost equal in brightness. A conjunction of the Moon and Saturn occurs May 16 at 10^h 55^m A. M.

Uranus **is also in good position for observation, being at opposition May 3. We give this time a diagram showing the apparent courses of the four satellites about the planet. In the tables we give the times when each satellite will be at greatest elongation, that is, at the point 0 on the diagram. The black dots with the numerals beside them indicate the positions of the satellites on the successive dates after the northern elongation. For example, Umbriel will be at northern elongation, *i. e.*, at the point marked 0 on its orbit, May 3 at $8^h.6$ P. M. On May 4 at the same hour it will be at the point marked 1, May 5 at the same hour it will be at 2, etc.**

The four oldest of the minor planets, *Ceres, Pallas, Juno* and *Vesta*, all happen to be in the region of sky covered this month by Poole Bros'. map in *Popular Astronomy* and their apparent courses for the next six months are shown in red upon the map. Ceres, Pallas and Vesta have passed the best time for their observation but will all be bright enough to be found without much difficulty during the next three months. Ceres was at opposition March 13. Its brightness will be equal to that of a star of the 7.2 magnitude April 1, 7.5^m May 1, 7.9^m June 1, 8.2^m July 1, 8.5^m August 1, and 8.8^m September 1. Pallas was at opposition Feb. 7. Its brightness will be 7.0^m April 1, 7.6^m May 1, 8.1^m June 1, 8.5^m July 1, 8.8^m Aug. 1, and 9.1^m Sept. 1. Vesta was at opposition March 10. Its brightness will be 6.5^m April 1, 6.8^m May 1, 7.2^m June 1, 7.5^m July 1, 7.8^m Aug. 1 and 7.9^m Sept. 1. Juno is not so favorably situated. Although she comes to opposition May 7 she is so far from her perihelion, or point of nearest approach to the Sun, that she will at brightest be only of the tenth magnitude and will therefore probably not be seen by the amateur.

Planet Tables for May.

[The times given are local time for Northfield. To obtain Standard Times for Places in approximately the same latitude, add the difference between Standard and Local Time if west of the Standard Meridian or subtract if east].

Date.	R. A.	Decl.	Rises.	Transits.	Sets.
1894.	h m	° ′	h m	h m	h m
			MERCURY.		
May 5......	1 50.6	+ 9 16	4 15 A. M.	10 36.5 A. M.	5 38 P. M.
15......	3 06.3	+16 58	4 18 "	11 32.7 "	6 47 "
25......	4 35.7	+23 14	4 27 "	12 22.5 P. M.	8 08 "
			VENUS.		
May 5......	23 58.4	− 1 15	3 05 A. M.	9 04.5 A. M.	3 04 P. M.
15......	0 37.7	+ 2 17	2 51 "	9 04.4 "	3 18 "
25......	1 18.2	+ 6 03	2 37 "	9 05.4 "	3 34 "
			MARS.		
May 5......	21 58.9	− 14 11	1 58 A. M.	7 05.3 A. M.	12 12 P. M.
15......	22 26.0	− 11 55	1 36 "	6 53.1 "	12 10 "
25......	22 52.4	− 9 32	1 14 "	6 40.2 "	12 07 "
			JUPITER.		
May 5......	4 21.2	+20 59	5 53 A. M.	1 26.5 P. M.	9 00 P. M.
15......	4 30.8	+21 22	5 22 "	12 56.8 "	8 32 "
25......	4 40.6	+21 43	4 50 "	12 27.2 "	8 04 "
			SATURN.		
May 5......	13 18.2	− 5 18	4 39 P. M.	10 22.1 P. M.	4 06 A. M.
15......	13 15.9	− 5 05	3 56 "	9 40.5 "	3 25 "
25......	13 14.1	− 4 56	3 14 "	8 59.4 "	2 44 "
			URANUS.		
May 5......	14 43.6	− 15 25	6 45 P. M.	11 47.2 P. M.	4 49 A. M.
15......	14 41.9	− 15 18	6 04 "	11 06.3 "	4 09 "
25......	14 40.4	− 15 11	5 22 "	10 25.4 "	3 28 "

Date. 1894.	R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m
			NEPTUNE.		
May 5......	4 43.6	+20 50	6 16 A. M.	1 49.1 P. M.	9 22 P. M.
15......	4 45.2	+20 52	5 38 "	1 11.2 "	8 44 "
25......	4 46.7	+20 55	5 00 "	12 33.4 "	8 07 "
			THE SUN.		
May 5......	2 51.0	+16 24	4 44 A. M.	11 56.5 A. M.	7 08 P. M.
15......	3 30.0	+18 59	4 32 "	11 56.2 "	7 20 "
25......	4 10.0	+21 03	4 23 "	11 56.7 "	7 31 "
			THE MOON.		
May 2......	0 16.4	+ 1 02	3 24 A. M.	9 34.2 A. M.	3 56 P. M.
4......	1 58.0	+14 11	3 59 "	11 07.7 "	6 34 "
6......	3 56.9	+24 38	4 58 "	12 58.5 P. M.	9 12 "
8......	6 13.3	+28 33	6 30 "	3 06.6 "	11 36 "
10......	8 27.0	+24 03	9 01 "	5 12.0 "	1 08 A. M.
12......	10 21.9	+13 34	11 41 "	6 58.8 "	2 01 "
14......	12 02.0	+ 0 43	2 09 P. M.	8 30.7 "	2 38 "
16......	13 37.8	−11 49	4 30 "	9 58.4 "	3 15 "
18......	15 18.4	−21 56	6 50 "	11 30.9 "	4 03 "
20......	17 07.4	−27 42	9 05 "	1 11.6 A. M.	5 15 "
22......	18 59.6	−27 51	10 53 "	2 55.8 "	7 01 "
25......	20 45.6	−22 38	12 05 A. M.	4 33.6 "	9 10 "
27......	22 22.0	−13 24	12 52 "	6 02.1 "	11 23 "
29......	23 54.3	− 1 36	1 28 "	7 26.2 "	1 38 P. M.
31......	1 31.8	+11 11	2 04 "	8 55.4 "	4 03 "

Phases and Aspects of the Moon.

	Central Time. d h m
New Moon................................	May 5 8 42 A. M.
Perigee................................	" 7 10 24 P. M.
First Quarter................................	" 12 12 21 A. M.
Full Moon................................	" 19 10 43 A. M.
Apogee................................	" 23 6 20 P. M.
Last Quarter................................	" 27 2 04 P. M.

[redacted]

Elongations of the Satellites of Saturn.

MIMAS.

	h		
May 1	9.9	P. M.	W
2	8.5	"	W
3	7.1	"	W
4	5.7	"	W
5	4.3	"	W
6	2 9	"	W
8	12.9	A. M.	E
8	11.5	P. M.	E
9	10.1	"	E
10	8.8	"	E
11	7.4	"	E
12	6.0	"	E
13	4.6	"	E
14	3.2	"	E
16	1.2	A. M.	W
16	11.8	P. M.	W
17	10.4	"	W
18	9.0	"	W
19	7.6	"	W
20	6.2	"	W
21	4.8	"	W
22	3.4	"	W
24	1.4	A. M.	E
24	12.0	midn.	E
25	10.6	P. M.	E
26	9.3	"	E
27	7.9	"	E
28	6.5	"	E
29	5.1	"	E
30	3.7	"	E

ENCELADUS.

	h		
May 2	3.6	P. M.	E
4	12.5	A. M.	E
5	9.4	"	E
6	6.3	P. M.	E
8	3.1	A. M.	E
9	12.0	M.	E

ENCELADUS Cont.

	h		
May 10	8.9	P. M.	E
12	5.8	A. M.	E
13	2.7	P. M.	E
14	11.5	"	E
16	8.4	A. M.	E
17	5.3	P. M.	E
19	2.2	A. M.	E
20	11.1	"	E
21	7.9	P. M.	E
23	4.8	A. M.	E
24	1.7	P. M.	E
25	10.6	"	E
27	7.5	A. M.	E
28	4.3	P. M.	E
30	1.2	A. M.	E
31	10.1	A. M.	E

TETHYS.

	h		
May 1	9.4	P. M.	E
3	6.7	"	E
5	4.0	"	E
7	1.3	"	E
9	10.6	A. M.	E
11	7.9	"	E
13	5.2	"	E
15	2.5	"	E
16	11.8	P. M.	E
18	9.1	"	E
20	6.4	"	E
22	3.7	"	E
24	1.0	"	E
26	10.3	A. M.	E
28	7.6	"	E
30	4.9	"	E

DIONE.

	h		
May 2	10.3	P. M.	E
5	4.0	"	E
8	9.7	A. M.	E

DIONE Cont.

	h		
May 11	3.4	A. M.	E
13	9.0	P. M.	E
16	2.7	"	E
19	8.4	A. M.	E
22	2.0	"	E
24	7.7	P. M.	E
27	1.4	"	E
30	7.1	A. M.	E

RHEA.

	h		
May 4	3.3	P. M.	E
9	3.6	A. M.	E
13	4.0	P. M.	E
18	4.4	A. M.	E
22	4.8	P. M.	E
27	5.2	A. M.	E
31	5.5	P. M.	E

TITAN.

	h		
May 2	11.9	P. M.	I
7	3.0	A. M.	W
11	5.2	"	S
14	11.8	P. M.	E
18	9.4	"	I
23	12.3	A. M.	W
27	2.7	"	S
30	9.3	P. M.	E

HYPERION.

	h		
May 3	10.8	P. M.	W
8	8.3	A. M.	S
13	1.0	P. M.	E
19	8.7	"	I
25	3.4	A. M.	W
29	12 8	P. M.	S

IAPETUS.

	h		
Apr. 27	9.2	A. M.	I
May 17	4.3	"	W
June 6	3.7	"	S

Maxima and Minima of Variable Stars.

Maxima

May 2	R Trianguli
3	S Carini
3	U Monocerotis
4	R Lyræ
6	U Aurigæ
7	V Ophiuchi
10	V Virginis
11	S Virginis
13	R Tauri
17	U Geminorum
18	S Serpentis
19	T Pegasi
91	S Vulpeculæ
20	S Aquilæ
21	T Herculis
24	T Cassiopeiæ
27	R Aurigæ
29	W Tuari
31	S Hydræ
31	S Piscis Austrini

Minima

May 1	T Ursæ Maj.
1	T Libræ
1	R Scuti
3	Y Virginis
10	η Geminorum
16	S Orionis
18	S Leonis
18	R Camelopardi
21	W Cygni
22	V Tauri
22	R Serpentis
22	R Sagittæ
29	R Ceti
30	U Monocerotis

Minima of Variable Stars of the Algol Type.

[Given to the nearest hour in Central Standard Time.]

U CEPHEI.

May	2	2 A. M.
	4	2 P. M.
	7	2 A. M.
	9	2 P. M.
	12	2 A. M.
	14	2 P. M.
	17	1 A. M.
	19	1 P. M.
	22	1 A. M.
	24	1 P. M.
	27	1 A. M.
	29	1 P. M.

R CANIS MAJORIS.

May	1	3 A. M.
	2	6 "
	3	10 "
	4	1 P. M.
	5	4 "
	6	7 "
	7	11 "
	9	2 A. M.
	10	5 "
	11	8 "
	12	12 M.
	13	3 P. M.
	14	6 "
	15	9 "
	17	1 A. M.
	18	4 "
	19	7 "
	20	10 "
	21	2 P. M.
	22	5 "
	23	8 "
	24	11 "
	26	3 A. M.
	27	6 "

S ANTLIÆ CONT.

May	5	5 A. M.
	6	5 "
	7	4 "
	8	[illegible] "
	[illegible]	[illegible] "
	10	[illegible] "
	11	1 "
	12	1 "
	12	12 midn.
	13	11 P. M.
	14	11 "
	15	10 "
	16	9 "
	17	9 "
	18	8 "
	19	7 "
	20	7 "
	21	6 "
	22	5 "
	23	5 "
	24	4 "
	25	3 "
	26	3 "
	27	2 "
	28	1 "
	29	1 "
	30	12 M.
	31	11 A. M.

δ LIBRÆ.

May	3	1 A. M.
	5	[illegible] "
	7	5 P. M.
	10	1 A. M.
	12	9 "
	14	5 P. M.
	16	12 midn.
	19	8 A. M.
	21	4 P. M.

U OPHIUCHI CONT.

May	3	5 A. M.
	4	1 "
	4	9 P. M.
	5	5 "
	6	1 "
	7	9 A. M.
	8	6 "
	9	2 "
	9	10 P. M.
	10	6 "
	11	2 "
	12	10 A. M.
	13	6 "
	14	2 "
	14	11 P. M.
	15	7 "
	16	3 "
	17	11 A. M.
	18	7 "
	19	3 "
	19	11 P. M.
	20	7 "
	21	4 "
	22	12 M.
	23	8 A. M.
	24	4 "
	24	12 midn.
	25	8 P. M.
	26	4 "
	27	12 M.
	28	9 A. M.
	29	5 "
	30	1 "
	30	9 P. M.
	31	5 "

Y CYGNI.

May	1	12 M.
	2	12 midn.

New Asteroid 1894 AT, AU, AV, AW, AX, AY and AZ.—These were all discovered photographically. *AX* is the brightest that has been discovered for a long time. At one of the European observatories, however, it was observed visually on the night of March 3, and found to be not brighter than 9.5 or 9.6 magnitude.

		Discovered by	Greenwich M. T.		h	m	R. A. h	m	s	Decl. °	′	Daily Motion. s	′	Mag.
1894	*AT*	Charlois	Jan.	29	9	26	8	44	56	+19	24	−48	+2	12
	AU	Charlois	Jan.	20	9	26	8	54	12	+19	56	−48	+3	13
	AV	Courty	Feb.	11	10	45	9	58	24	+22	24	−68	+6	11
	AW	Wilson	Jan.	30	15	22	3	41	25	+24	50	+38	+1	12
	AX	Wolf	Mar.	1	9	05	11	04	36	+ 6	59	−76	0	8
	AY	Wolf	Mar.	1	9	05	11	14	00	+ 4	08	−48	+8	12
	AZ	Courty	Mar.	5	10	14	9	34	24	+23	08	−48	+9	10

New Comet, *a* 1894, discovered by Denning:—A faint comet was discovered by Mr. W. F. Denning at Bristol, England, on the night of March 26. Its position was:

March 26.396 Gr. M. T.; R. A. $9^h\ 55^m\ 00^s$; Decl. $+32°\ 15'$. The daily motion of the comet was 1° south following. The discovery was cabled to Mr. John Ritchie, Jr., of Boston and by him telegraphed to the different observers in the United States on March 28.

The comet was observed at Goodsell Observatory on the night of March 28. It was very small and faint in the five-inch finder but was quite conspicuous and easily observed with the 16-inch telescope. It had a well defined nucleus of the 11th magnitude, and a short tail about 2′ long and spreading to 1½′ width. The nebulosity about the nucleus was quite dense and faded rapidly toward the end of the tail. The position of the nucleus was determined by comparison with an eighth magnitude star north preceding it, resulting as follows:

March 28.6552 Gr. M. T. R. A. 10^h 03 01.58; Decl. $+ 30°\ 58'\ 02''.0$. There are not sufficient data yet at hand for the computation of an orbit.

NEWS AND NOTES.

Experience during the last month makes it very necessary to remind correspondents that proper names should be written very plainly to avoid mistakes in our mailing lists. This is especially true in regard to *foreign* correspondents.

Before his return from Germany, Professor George E. Hale will visit Mt. Etna to make experiments in photographing the corona of the Sun. He plans to reach the mountain about May 15. Mr. Hale's persistent endeavors to discover some way to photograph the corona in full sunlight will, we believe, be rewarded with success before long.

Considerable useful matter intended for the department of ASTRO-PHYSICS in this number has been unavoidably delayed beyond the time of publication. No one can be rightly blamed for this, for every reasonable effort by those in charge of this department was put forth to supply the matter in time without success.

Lantern Shades at Goodsell Observatory.—Since completing his course of special study at Goodsell Observatory, Mr. A. G. Silvashan has given some attention to the making of lantern slides of the photographs of suitable objects made at this Observatory. These consist of views of different phases of the Moon, Pleiades showing curiously intertwining nebulous masses, star trails about the North Pole to show its positions photographically, star clusters, views from the Milky Way, Whirlpool Nebula, minor planet trails, a number of views of the (Rordame?) comet of July last, enlarged views of Sun-spots and faculæ, and numerous other objects suitable for lantern projection, either for class illustration, or for popular lectures on astronomy. Professor William A. Rogers, of Colby University, Waterville, Me., has recently secured a number of these pictures, and he does us the credit to speak very favorably of them.

Professor Turner in the Savilian Chair at Oxford.—A kind note from Professor C. H. McLeod, of McGill University, Montreal, gave us first notice under date of February 9, of the complimentary dinner which was to be given to Professor H. H. Turner February 14, in honor of his appointment to succeed the late Professor Pritchard at Oxford. The dinner was given by the staff of the Royal Observatory, Greenwich, and other astronomical friends, the Royal Astronomer, Mr. Christie, presiding.

The March *Observatory* makes the following note of the occasion: "A congratulatory dinner was given to Professor Turner in celebration of his election to the Savilian Chair, at the First Avenue Hotel, on Wednesday, February 14, by the members of the Observatory staff and several astronomical friends. There were present besides the Observatory staff, Messrs. W. Airy, F. W. Dyson, Dr. A. A. Common, Messrs E. Dunkin, W. Ellis, W. C. Johnson, G. Knott, W. H. Maw, G. C. Pulsford, Dr. Spitta, Messrs S. Waters and W. H. Wesley. Letters and telegrams of congratulation and of regret for inability to attend were read from Capt. Abney, Mr. F. McClean, Mr. W. E. Plummer, Professor McLeod, of Montreal, and Messrs Dickenson and Wilmot of the Commercial Cable company.

Mr. G. E. Lumsden, of Montreal, has in the January number of the *Canadian Magazine* a very readable article on *Common Telescopes and What they will show*. We also notice with pleasure and profit, the articles that John A. Copland has been publishing in the *Globe* of Toronto. They are fully and very neatly *illustrated*.

A Search for Comet of Holmes.—The interest that seems to hover around the comet of Holmes led me to indulge in a thorough search for it during the opposition just past.

During December and January I was observing a star with the Prime Vertical instrument, that came in the early morning hours, and I utilized the time while waiting for the star to transit in making a search for the comet. I used the ephemeris computed from the elements by Professor Boss, as published in the *Astronomical Journal*, until those computed by Mr. Corrigan, and which had been corrected for perturbations, appeared, when I adopted the latter.

Recognizing that the object might appear in the form of an asteroid, each night before commencing the search, I platted all the stars within a radius of 20′ of the comet place. The positions I copied from Argelander's chart, and then compared that with the sky, filling in all the fainter stars that could be seen with our 10-inch refractor. The following night, if clear, or as soon afterwards as possible, I again examined the place to see if any star was missing.

The 10-inch should show stars as faint as the 12th magnitude, and upon each night I have been able to identify all the stars. After platting all the places I swept about two degrees each side of the comet place and although I came upon three faint nebulæ, I was able to identify them in existing catalogues.

If the comet is following its predicted place it is too faint for a 10-inch, and is not as bright as the 12th or 13th magnitude.

There is much in the physical form of comets that awaits our earnest attention. The proximity of comet of Holmes to the Earth at this opposition, in comparison with the distance at which it was when last observed, leads me to believe that we are very deficient in our knowledge of what condition forms what we call the brightness of a cometary mass, and we are compelled to admit that its brightness at a certain distance from the Sun and the Earth is no criterion of what it will be when it again arrives at that point.

In the first of January Professor Brown and I made a search for the comet upon a very clear night with the 26-inch refractor and a nebula of at least the 14th magnitude would not have escaped our attention. GEORGE A. HILL,

Naval Observatory, Washington, D. C. Asst. Astronomer.

Jan. 29, 1894.

Chandler on Observations of Variable Stars at Harvard College Observatory.—In a recent number of the *Astronomische Nachrichten* is a damaging article by S. C. Chandler of Cambridge, Mass., claiming that to some extent at least the observations of variable stars at Harvard College Observatory are not trustworthy. This article will be a surprise to astronomers generally, for the Harvard work has stood very high everywhere. In this guarded paper we are sorry to notice some traces of the former ill-feeling against the director of the Observatory. Doubtless Professor Pickering will have something to say in answer to these charges in due time.

Halley's Comet.—Professor Glasenapp announces that the computing bureau established by the Russian Astronomical Society has undertaken the calculation of the true path of Halley's Comet with a view to predicting the exact date of the next return. He hopes that astronomers acquainted with unpublished observations of the comet will communicate the information to the Society.

Untrodden Ground in Astronomy and Geology.—In the December number of *Science*, page 341, will be found an article under the title "The glacial period proved as a necessary consequence of the Earth's movements." It was written by J. C. Cowell of England, and is a review of General Drayson's book known by the title: "Untrodden Ground in Astronomy and Geology." We have not seen this book, and know of it only by the article before us. In the opening paragraph of that article Mr. Cowell claims than General Drayson has made a great discovery of a "second rotation of the Earth" (as he calls it) which supplies all the conditions necessary for the explanation of glacial epochs at regular known intervals, in all past or future geologic time.

This is comforting news for the astronomer, for now it seems as if there were some chance for him to agree with the geologist in regard to the data, in time, for past glaciation, in more ways than one. The astronomer has been much interested in late changes of view in regard to the length of past geologic time. Sir Charles Lyell thought the origin of life on the Earth extended back 500,000,000 years; Charles Darwin, in the first edition of his Origin of Species

reckoned 306,662,400 years "as a mere trifle" of that at command for establishing his theory of the origin of species through natural selection. George H. Darwin, professor of mathematics of Cambridge University, England, limits the geologists to 100,000,000, while Professors Tait and Newcomb have named as probable 10,000,000 or 12,000,000 years. Wallace thinks that 28,000,000 years gives time enough for the deposits of geological strata; and now Prestwich and Wright are only asking for 100,000 years as probably time sufficient to include the slow coming on of the glacial period and its rapid close, and that 25,000 years is ample time to allow for the reign of the glacial period. These very marked changes of belief of eminent men of science during the last 40 years are very significant.

But General Drayson gets his conclusions in a much easier way, viz.: by the so-called "Second rotation of the Earth." For the illustrations the copy of *Science* referred to should be consulted. We would not have referred to this article at so great length if we had not received several letters concerning it asking for information on certain points in the article. We reply generally and briefly.

There is no "second rotation of the Earth" in the sense indicated.

The article does not show any proofs of the so-called "second rotation."

The author does not at all seem to comprehend the problems of practical astronomy or the real nature of the data sought by them, or he would not expect to use simple trigonometry to solve them.

In the results given as a proof of his method he begs the question, because he tests his results by those obtained by astronomical methods which he condemns.

Astronomers very well know that the present uncertainty of knowledge of the mean distance of the Sun is such that it would be impossible for them to say certainly just what the eccentricity of the Earth's orbit was at any precise time in the remote past so as to predict for glaciation. The error of the solar parallax alone might make a difference of several times 25,000 years, and the astronomer might be in doubt whether to say polar or tropical conditions or neither reign supreme.

The Chicago Academy of Sciences Section of Mathematics and Astronomy. March 5.—The regular monthly meeting was held at the Chicago Athenæum; Professor G. W. Hough, President, in the Chair. Dr. T. J. J. See read the paper of the evening on "*Gauss' Method of Determining Secular Perturbations, with an Application to the action of Neptune on Uranus.*" The speaker began by pointing out the distinction between periodical and secular perturbations, and then sketched briefly the work of the great mathematicians on the secular inequalities of the planetary motions. After alluding to the work of Lagrange and Laplace, which depends upon analytical developments in series, expanded according to the powers and products of the eccentricities and inclinations of the planes of the orbits, the speaker came to the method of Gauss, which was first developed in a memoir, on the attraction of a certain form of elliptical ring, communicated to the Royal Society of Sciences of Gottingen in 1818. Since the secular perturbations depend only upon the mean action of the planets from age to age, Gauss conceived the idea of substituting a certain form of elliptical ring for the moving planet; the determination of the attraction of these rings involves the use of elliptic integrals of the first and second kinds.

The mass of the planet is imagined to be distributed around the orbit in such a way that equal areas described by the radius vector will include equal portions of the planet's mass. The author gave the principal step in the investigation for

finding the action of such elliptic rings, and called attention to the high importance of the memoirs of Dr. G. W. Hill and M. Callandreau, which not only develop the theory of Gauss' method, but give auxiliary tables for facilitating its numerical application.

Dr. See then gave the results of his computation on the secular perturbations of Uranus depending upon the action of Neptune, and showed that they agreed very well with Leverriers's values when the latter are corrected for the modern planetary masses. In the discussion of the paper, Professor Hough made some interesting remarks on the history of the discovery of Neptune; Professor Burnham and Dr. Crew also took part. In reply to the question as to the possibility of discovering a trans-Neptunian planet by means of its perturbations, Dr. See stated that if such a planet exists, it will certainly be possible to detect it in the course of time by means of the resulting irregularities in the motions of Neptune and Uranus.. Adjourned. T. J. J. SEE, Recorder.

Astronomical and Physical Society of Toronto, Canada.—Meeting of Feb. 19, 1894; Vice president John A. Paterson, M. A., presided.

Professor G. E. Hale of Chicago was elected a corresponding member.

Mr. G. G. Albery of Meaford, Ont., wrote that the Meaford Astronomical Society had been formed.

Dr. Larratt W. Smith sent word of a great meteor which had passed over Nevada and California on Feb. 5.

Mr. G. G. Pursey, librarian, reported receipt of numerous publications. A presentation copy of J. Ellard Gore's new work, "An Astronomical Glossary," was laid on the table, and was pronounced valuable.

A large group of interesting sun-spots was referred to by Messrs. Andrew Elvins, A. F. Miller and G. G. Pursey. A drawing made by Mr. Geo. Willings was handed in.

Papers were read by Messrs. Elvins, J. M. Collins, Chant, Gordon Hull and Thomas Lindsay on "Dynamics," as outlined in Mr. Grant Allen's book on force and energy.

Meeting of Feb. 6; Mr. John A. Paterson, M. A., Vice-president, presided.

Miss Annie Gentle was elected an active member.

Letters were read from Sir Robert Ball, F. R. S., Mr. J. Ellard Gore, F. R. A. S., Professor W. H. Pickering of Arequipa, Peru, Dr. Sanford Fleming, and others.

Professor Pickering was elected a corresponding member.

The society approved of the report that the Sir Adam Wilson telescope be set up at No. 23 Walmer road, the residence of Mr. J. A. Paterson.

Mr. C. P. Sparling laid upon the table the society's fourth annual report. It has 166 pages and contains a portrait of Mr. Andrew Elvins.

The greater part of the meeting was devoted to reporting and discussing observations made upon the Sun, Mercury, Venus, Jupiter, Saturn and a series of auroræ observed mostly on the night of the 23rd of February. The more active observers were:—Messrs. Arthur Harvey, T. Lindsay, A. Elvins, G. G. Pursey, J. R. Collins, S. Hollingworth, R. Dewar, J. A. Copland, W. B. Musson, Dr. J. C. Donaldson of Fergus, John Hollingworth of Beatrice, Muskoka, and G. E. Lumsden.

Mr. Harvey read a memorandum on the size and frequency of sun-spots.

Dr. Donaldson of Fergus referred to a series of difficult double-star observations he has been making, with a view to preparing a list of test-objects for telescopes of apertures of three and one-half inches and less. JOHN A. COPLAND.

PUBLISHER'S NOTICES.

The subscription price to ASTRONOMY AND ASTRO-PHYSICS in the United States and Canada is $4.00 per year in advance. For foreign countries it is £1 or 20.50 marks per year, in advance. Recent increase in price to foreign subscribers is due to increase of postage because of enlarged size during the year 1892. Messrs. Wesley & Son, 28 Essex Street, Strand, London, are authorized to receive subscriptions. Payment should be made in postal notes or orders or bank drafts.

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All communications pertaining to Astro-Physics or kindred branches of physics should be sent to George E. Hale, Kenwood Observatory, of the University of Chicago, Chicago, Ill.

For information of correspondents, the names and addresses of the associate editors of ASTRO-PHYSICS are given as follows:—

James E. Keeler, Observatory, Allegheny, Pa.; Henry Crew, Northwestern University, Evanston, Ill., Jos. S. Ames, Johns Hopkins University, Baltimore, Md.

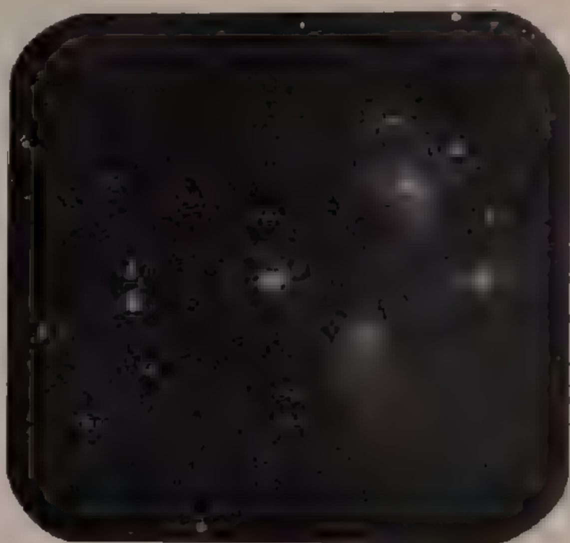
All matter or correspondence relating to General Astronomy, remittances, subscriptions and advertising should be sent to Wm. W. Payne, Publisher and Proprietor of ASTRONOMY AND ASTRO-PHYSICS, Goodsell Observatory of Carleton College, Northfield, Minn.; and the Associate Editors for General Astronomy are: S. W. Burnham, Government Building, Chicago Ill., E. E. Barnard, Lick Observatory, Mt. Hamilton, Cal., and H. C. Wilson, Goodsell Observatory, Northfield, Minn.

Manuscript for publication should be written on one side of the paper only and *special care should be taken to write proper names and all foreign names plainly*. All drawings for publication should be *smoothly* and *carefully* made, in *India Ink* with lettering well done, because such figures are copied exactly by the process of engraving now used. If drawings are made about double the size intended for the printed page, better effect will be secured in engraving than if the copy is less in size. It is requested that manuscript in French or German be typewritten. If requested by the authors when articles are sent for publication, *twenty-five* reprint copies, in covers, will be furnished free of charge. A greater number of reprints of articles can be had if desired, at reasonable rates.

Rates for advertising and rates to news agents can be had on application to the publisher of this magazine.

CONTENTS FOR MARCH, 1894.

PLATE XI.



THE PLEIADES NEBULA AND TRAIL OF ASTEROID No. 203 POMPEJA.

From a Photograph by H. C. Wilson at Goodsell Observatory Jan. 30, 1894. Exposure 4 hours.

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RESULTS OF AN INVESTIGATION OF THE ABERRATION AND ATMOSPHERIC REFRACTION OF LIGHT, MADE WITH A MODIFIED FORM OF THE LOEWY PRISM APPARATUS.*

GEORGE C. COMSTOCK.

In December, 1888, the committee having charge of the Watson Fund of the National Academy of Sciences placed at my disposal the sum of eight hundred dollars in aid of the investigations which are set forth in this paper, and I desire here to express my sincere thanks for this aid, without which the prosecution of the research would have been seriously impeded.

The observations which furnish the data to be discussed consist of measurements of the angular distance separating zodiacal stars which are very approximately 120° apart in the heavens, and the apparatus employed for this purpose was a six-inch equatorial telescope by A. Clark & Sons, provided with a system of mirrors so placed in front of its objective that images of the stars whose mutual distance was to be measured were simultaneously produced in the focal plane of the objective at a distance from each other sufficiently small to admit of accurate measurement with a filar micrometer. It has been shown by M. Loewy in the *Comptes Rendus* that the angular distance between the stars will be equal to this distance measured with the micrometer plus twice the angle between the reflecting surfaces. Since this angle does not admit of very precise determination the observing program proposed by M. Loewy is a purely differential one in which the angle between the mirrors is eliminated, but it is evident that if instead of two mirrors three or any greater number be employed, and be so placed as to make approximately equal angles, one with another, the mean value of the angles formed by the several pairs of mirrors will be determined by purely geometrical considerations, *e. g.*, in the case of three mirrors the angle will be 60°, and if each pair of mirrors be employed in the determination of the microme-

* Read at the meeting of the National Academy of Sciences, April 17, 1894.

ter distance the absolute angular distance between the stars may be obtained independently of the adjustment of the mirrors.

Practically no other number of mirrors than three can be employed, and it is the adoption of this combination that has limited my observing program to pairs of stars 120° apart. The essential difference between the method of M. Loewy and that here adopted is the elimination of the angle between the reflecting surfaces by means of the geometrical condition above considered, instead of by taking the difference between measurements of different pairs of stars made with only two mirrors. A consequence of the modification is that I have been able to determine absolute distances between the stars and to utilize the same observations for a determination of both aberration and refraction. The observing program has been so arranged that the aberration results are nearly free from the effect of undetermined error in the refraction tables and the concluded corrections to the tabular refractions are entirely independent of the aberration constant.

In my first design for an instrument the mirrors were the silvered faces of an equiangular glass prism mounted in front of the objective, with its axis at right angles to the line of sight of the telescope. Such a prism was constructed for me by Mr. Brashear and was used for a short time, but the effect of temperature changes in distorting the surfaces was so great that I was compelled to abandon it as wholly unserviceable and to substitute for the prism three plane mirrors of rectangular cross-sections which were so mounted in a metallic reel that each reflecting surface passed through the axis of the reel. Theory indicates that when this condition is satisfied any small distortion of the reflecting surfaces will have no effect upon the measured distances between the stars. Nevertheless, in order that the distortions might be made as small as possible I had silvered each of the eighteen surfaces possessed by the three pieces of glass, although only one surface on each piece was polished. By direct experiment in artificially producing effects similar to those which would result from large distortion of the mirrors I have ascertained that such magnified distortion does not sensibly affect the measurements made with the apparatus. It should be added that an opaque screen was placed between the mirrors and the objective and along a diameter of this screen three circular apertures were made each two inches in diameter. The screen was so placed that one of these apertures was nicely centered below each mirror, thus in effect resolving the six-inch objective of the telescope into three two-inch objectives each of which corresponds to a mirror. The purpose of

these screens was primarily to insure that the axes of the several pencils of light transmitted from the mirrors to the objective should all fall upon a particular diameter of the objective indicated by theory, and secondarily to obtain round images of the stars. The optical qualities of the combined mirror and two-inch objective may be inferred from my having repeatedly seen the components of α Piscium, $s = 3''$, nicely separated.

The theory of the errors of the apparatus indicates that the effect of mal-adjustment of the mirrors may be represented in terms of four instrumental constants whose values have been determined from time to time during the progress of the observations. No one of these constants has ever had at the time of observation a value so great as five minutes of arc, and since only their squares and products affect the measured distances it is apparent that the instrumental corrections to be applied are extremely minute and are sufficiently determined by even rough values of the constants. Save at the beginning of the work, when the theory was imperfectly understood, these corrections never amount to so much as $0''.1$.

The mode of observing with the apparatus is substantially as follows: The telescope is directed to the middle point of the arc joining a pair of stars about fifteen minutes before the time at which the stars attain equal altitudes, and the reel containing the mirrors is rotated into such a position that images of the stars are produced in the field of view by two of the mirrors, *e. g.*, mirror *A* produces an image of the east star, and mirror *B* an image of the west star. The distance between the images of the stars having been measured with the micrometer, the reel is rotated so as to produce successively the following positions of the mirrors, the mirror turned toward the east being always given first *AB*, *BC*, *CA*, *AC*, *CB*, *BA*. The distance between the images of the stars is measured in each of these positions, care being taken to so arrange the pointings as to eliminate the micrometer zero and to produce a symmetrical grouping of the observations about the instant at which the stars were at equal altitudes. The result of such a series of from six to eighteen micrometer settings is called an observation of the apparent distances, Δ, between the stars composing a pair.

If the refraction and aberration are known this apparent distance may be transformed into a mean distance which save for the effect of parallax and proper motion of the stars should be the same at all times. Any error in the assumed mean value of the refraction will make the absolute value of this mean distance

in error, while a periodic variation of the refraction or an error in the adopted aberration constant will introduce periodic errors into the mean distance. The investigation of the aberration and of periodic variations in the refraction must therefore be made together, and the investigation of the mean amount of the refraction may conveniently be separated from them.

For the first of these investigations each observation furnishes an equation of the form,

$$\Delta_0 + f \, . \, x + A\alpha + B\beta = \Delta$$

where

$\Delta_0 =$ the mean angular distance between the stars.
$\Delta =$ the observed angular distance between the stars.
$x =$ the correction to the adopted observation constant.
$\alpha, \beta =$ coefficients of an annual variation of the refraction.

The numerical coefficients f, A and B have the following values:

$$f = -2 \cos \beta \sin (\lambda - ☉).$$

$$100\,A = -R_0 \sin (360^\circ \, . \, \tau) \qquad 100\,B = -R_0 \cos (360^\circ \, . \, \tau)$$

where

$\lambda, \beta =$ coördinates of the middle point of the arc Δ.
$\tau =$ the fraction of the year that has elapsed at any date.
$R_0 =$ the mean effect of refraction upon Δ at the time τ.

R_0 varies from winter to summer on account of the change in the mean temperature of the air.

The material available for a discussion of the aberration consists of 755 observations of 38 pairs of stars which are distributed with a rough approximation to uniformity throughout the twenty-four hours of right ascension, and nearly all of which lie within 20° of the equator. But before proceeding to the solution of the equations furnished by these observations it is necessary to consider some sources of error to which they are subject. The absolute term of the equation, Δ, may for the present be considered as the direct-result of observation corrected for an assumed value of the aberration, plus a refraction correction computed from the Pulkowa tables, and plus a reduction to 1890.0 derived from the best proper motions available. These latter corrections are all small, since the observations extend over a period of only two years but the effect of refraction is large, amounting on the average to nearly 200″ for each observation. This correction therefore requires especial attention, and in its computation I have taken into account the difference in the force of gravity at

Pulkowa and Madison, and also the difference in the radii of curvature of the Earth's surface at the two places. I have further derived a theoretical expression for the effect of humidity upon the refraction, based upon laboratory determinations of the index of refraction of aqueous vapor (Mascart, Fizeau), and have applied this as a correction to the computed refractions. A comparison of the observations made in a dry, and in a humid atmosphere furnishes the following mean result:

Humid — Dry = $+ 0''.17 \pm 0''.08$ Without correction
" " = $- 0''.03 \pm 0''.06$ When corrected

thus confirming the correction. It was only after these corrections had been applied that I learned that substantially similar ones had been developed by Radau, *Annales de l'Observatoire de Paris, Memoires, 19.*

I have discussed the effect of temperature upon the apparatus employed and in this connection have determined from the observations the coefficient of expansion of dry air under constant pressure. The resulting value

$$m = 0.003674 \pm 0.000008$$

differs from Regnault's laboratory determination by less than half its own probable error, but is appreciably smaller than the value adopted in the construction of the Pulkowa tables. I have therefore united with the temperature correction of the apparatus, commonly called the temperature coefficient of the micrometer screw, a correction to reduce the computed values of the refraction to what they would have been had the γ factor of the tables been constructed from Regnault's value of m. The combined corrections for temperature and humidity in no case exceed $0''.4$

Of the 755 available observations about one-fourth part were made by Mr. A. S. Flint, the remainder by myself, and before combining these different results it seemed necessary to determine the amount of the systematic difference between the observers. The results of a comparison made for this purpose are contained in the following table whose argument, d, is the apparent distance in minutes of arc between the images of stars seen simultaneously in the field of view of the telescope. A negative value of d indicates that the image of the western star was on the eastern side of the field.

d	No of Pairs.	Weight.	$C - F$	$\Delta - F$
′			″	″
− 8.4	5	11	− 0.67	+ 0.55
− 4.3	3	12	− .43	− .10
− 3.3	4	14	− .60	− .41
− 1.6	3	14	− .21	− .16
− 0.3	3	11	+ .33	+ .33
+ 1.6	2	15	+ .42	+ .37
+ 2.4	3	14	+ .26	+ .16
+ 4.1	3	13	+ .47	+ .18
+ 4.7	2	12	+ .13	− .26
+ 5.8	4	10	+ .59	+ .01

It is evident from the column C − F that a considerable systematic error affects the work of one or both observers, and since it appears in the sequal that this error is mainly in my own observing, I have added to the table the column $\Delta - F$ derived from C − F by applying to the latter my personal errors. The sequence of signs in this column does not indicate any appreciable systematic error affecting the observations made by Mr. Flint.

To determine the nature of the errors indicated by the preceding table I have compared my own observations with the computed values of Δ derived for each pair of stars from the right ascensions and declinations of the stars, using a set of places derived from all the modern catalogues of precision supplemented by an extensive series of determinations of right ascension made for this purpose by Mr. Flint with the meridian circle of the Washburn Observatory. The results of this comparison are contained in the following table where Δ denotes such a computed distance. The quantities d and C have the same meaning as in the preceding table:

d	No. of Pairs.	Weight.	$\Delta - C$	$\Delta' - C'$
′			″	″
− 7.0	9	10	+ 0.56	− 0.52
− 2.8	7	10	− 0.40	− .57
− 0.3	7	10	− 0.53	− .53
+ 2.6	8	10	− 0.77	− .62
+ 5.2	7	11	− 1.08	− .49

The sequence shown by the values of $\Delta - C$ could be in great part removed by altering the adopted value of a revolution of the micrometer screw, and the column $\Delta' - C'$ exhibits the results which would be obtained were this value diminished by 0″.07. But against the introduction of such a correction there is to be urged that it offers no explanation of the differences C − F, and that the adopted value of a revolution of the screw depends upon the accordant results of two entirely independent methods of determination, *i. e.*,

One revolution = 27″.412 ± 0″.004 from transits of stars.
= 27 .412 ± 0 .004 from the meridian circle

which render the presence of so gross an error as $0''.07$ exceedingly improbable. Neither can the differences Δ — C be due to a progressive error in the micrometer screw which causes the value of a revolution to vary with the distance measured, since these errors have been investigated both by means of the meridian circle and by the measuring engine of the Transit of Venus Commission at Washington. Both of these methods furnish very small and accordant results which have been duly applied to the observations.

A possible explanation of the differences may be found in the supposition of a fixed habit of observing on the part of C by which the micrometer threads are not placed upon the centers of the star images but are set a little farther apart than the images. Such a personal error may easily arise from the necessity experienced by the observer of turning his attention rapidly from one star to the other in estimating the coincidence of the micrometer threads with the images. Such a habit of observing would give too great values of Δ with positive d's, and too small values with negative d's; but it appears to me probable that with increasing distances the errors would increase more rapidly than d, and I have therefore chosen, somewhat arbitrarily, to represent these errors as functions of the square of d. From a least square solution of the data furnished by thirty-eight pairs of stars I find as the definitive expression for this correction when d is expressed in minutes of arc

$$\varphi(d) = \pm\ bd^2 \qquad b = 0''.0173 \pm 0''.0017$$

The legitimacy of this correction should not be considered as depending upon the hypothesis above advanced with regard to its origin. It is an empirical term whose justification is that it brings the observations by C into agreement with the computed distances between the stars, and is presumably of subjective origin because the observations by F require no such correction. I have adopted it and applied it to all of my own observations.

All of the corrections above considered having been duly applied the observations of each pair of stars were treated by the method of least squares and a set of normal equations formed for each pair. These several groups of normal equations contain three unknown quantities which are common to all of them, *viz.*, the correction, x, to the adopted constant of aberration and the two coefficients α and β of a supposed annual term in the refraction. They also contain a term which is peculiar to each pair of stars, the correction to the assumed distance between the stars.

This term being made the first in the elimination the summation of the values of $[bb\,.\,1]$, $[bc\,.\,1]$, * * * $[dn\,.\,1]$, etc., resulting from the several sets of normal equations furnished a final system of normal equations containing only the unknown quantities x, α and β.

I have made three such solutions of the data, differing from each other in the following respects:

A. This solution adopts the refractions of the Pulkowa Tables and assumes that the correction above represented by $\varphi(d)$ is illusory, and that the observations by C require no empirical correction.

B. This solution adopts $\varphi(d)$ as a real correction and applies to the Pulkowa refractions the corrections above discussed.

C. The same as *B* except that in forming the final normal equations every pair of stars for which $[bb\,.\,1]$ was less than 25 was rejected. This is equivalent to rejecting those pairs of stars which were observed under unfavorable conditions for a determination of aberration, the coefficient presenting a very small range of values. The pairs of stars thus rejected were introduced into the observing list for the determination of the absolute amount of there fraction and the conditions determining their choice were such that in most cases they present large values of d; the observations are of a slightly inferior degree of precision, and are much more affected by any uncertainty in the empirical term $\varphi(d)$.

Since I prefer the results of solution C, I give the normal equations resulting from it alone, but I give the resulting values of the unknown quantities from each solution.

NORMAL EQUATIONS FOR THE ABERRATION

$$
\begin{aligned}
+\,952.28x - 133.81\alpha - 119.99\beta - 3.61'' &= 0\\
-\,133.81x + 844.92\alpha - 414.62\beta + 45.09 &= 0\\
-\,119.99x - 414.62\alpha + 571.59\beta - 20.31 &= 0
\end{aligned}
$$

The values of the unknown quantities furnished by the several solutions, together with their probable errors and the probable error of a single observation are as follows:

	Const. Ab.	α	β	r_1
	"	"	"	"
Solution A	20.470 ± 0.010	− 0.012 ± 0.013	+ 0.075 ± 0.016	± 0.32
B	.449 ± .010	− .045 ± .013	− .010 ± .016	± .32
C	.443 ± .010	− .058 ± .013	− .008 ± .016	± .30

The difference between the values of the aberration furnished by solutions *A* and *C* is due almost entirely to the introduction of the empirical correction $\varphi(d)$ and does not arise from the correc-

tions applied to the refraction. It may be noted as a singular coincidence that the effect of $\varphi(d)$ is to transform a value of the aberration in close agreement with that of Nyrén into one which agrees even more closely with that of Struve.

The effect of the correction to the refractions is shown in the quantities α and β whose character is completely changed by its introduction. These quantities were introduced into the equations as the representatives of an assumed annual variation of the refraction not taken into account by the tables. Their significance is most conveniently shown by transforming them into the equation

$$R = R_0\{1 - 0.00058 \sin(360^\circ . \tau + 8^\circ)\}$$

where R denotes the actual, and R_0 the tabular value of the mean refraction at a date expressed by τ, the elapsed fraction of the year. The probable error of the numerical coefficient is

$$\pm 0.00013.$$

It appears from this equation that the refraction in the spring is less, and in the autumn greater than its mean value by about one-twentieth of one per cent.

Since the form of the equations is such that α and β are necessarily assigned real and finite values by the solution, the very small variation in the refraction indicated by the coefficient of R_0 may be considered to have no real existence and to be produced by the unavoidable errors of observation, although the small value of the probable error does not support this view. If, nevertheless, this supposition be adopted and the values of α and β in the expression for the aberration be put equal to zero, we shall obtain for the value of the constant $20''.452 \pm 0''.010$. It appears to me better to retain the values of α and β furnished by the equations since they represent not only the effect of a variable term in the refraction but also the combined influence of all terms whose effect upon the observations varies with the seasons. Whatever physical interpretation is placed upon the quantities it is apparent that the Pulkowa tables represent the seasonal variation of the refraction with surprising accuracy.

I adopt as the definitive result of this part of the investigation

$$\text{Constant of Aberration} = 20''.443 \pm 0''.0106.$$

The probable error includes the effect of uncertainty in the value of a revolution of the micrometer screw.

Definitive values having been found for the quantities thus far considered it becomes possible to derive from the observations of

each pair of stars the angular distance between its components, and a comparison of this measured distance with that computed from the coördinates of the stars may be used to determine a correction to the absolute amount of the tabular refraction. It should be noted that since the observing program contained only equatorial stars the computed distances will depend mainly upon the right ascensions and will be but little affected by small errors in the adopted declinations.

The results of such a comparison are exhibited in the following table, in which there are united into mean values the individual results derived from all those pairs of stars the middle points of whose joining arcs fall within convenient limits of right ascension. The adopted coördinates of the stars were referred to the system of the *Berliner Jahrbuch* which is substantially the same as that of the *American Ephemeris*. I have included in the table columns showing the differences between the computed and observed distances which would have been obtained had the right ascensions of the stars been reduced to the respective systems of the *Nautical Almanac* and the *Connaissance des Temps* of the year 1883.

COMPARISON, C—O, WITH THE EPHEMERIDES.

Limits of R. A.	No. of Pairs	B. J., A. E.	N. A.	C. T.
h h		"	"	"
1.1… 3.2	2	+ 1.03	+ 2.13	+ 2.42
5.5… 6.4	4	+ 0.32	+ 0.80	+ 0.72
7.1… 7.5	3	+ 0.70	+ 0.82	+ 0.81
8.4… 9.3	4	+ 0.11	+ 0.26	− 0.20
9.9… 11.1	4	− 0.20	− 0.16	− 0.56
12.0…13.5	2	+ 0.08	+ 0.28	− 0.22
15.0…15.6	2	− 0.43	− 0.27	− 0.33
17.5…18.6	4	+ 0.20	− 1.08	− 0.74
18.8…19.6	3	− 0.11	− 1.11	− 0.87
20.4…21.2	3	+ 0.06	− 0.45	− 0.31
21.6…22.2	4	+ 0.48	+ 0.44	+ 0.49
22.7…23.5	3	+ 0.08	+ 0.53	+ 0.61

The differences between the values of C — O which stand on the same line of the table are due solely to the differences between the ephemerides, and the very considerable values which they attain illustrate the uncertainty affecting the computed length of any long arc in the heavens, even when derived from the coördinates of fundamental stars. The comparisons with the *Nautical Almanac* and *Connaisance des Temps* show unmistakably a periodic variation of C—O depending upon the right ascension and having a period of twenty-four hours. The *Berliner Jahrbuch* and *American Ephemeris* seem to be nearly free from error of this kind, although there is some indication of it. It does not appear

that this variation in the values of C — O can be attributed to the observations since the method of discussion is such that very term having a period of twenty-four hours must have been eliminated through the introduction of the constants α and β.

To determine the mean value of C — O, I have made a graphical adjustment of the numbers in the third column of the table, and from the thirty-eight residuals furnished by comparing the adjusted values with the values of C — O furnished directly by the observations, I find for the probable error of a single C — O, $\pm$ 0".33, and for the probable error of a single right ascension used in the computation of the distances, $\pm$ 0^s.015. According to the discussion by Dr. Auwers (*Astr. Nachr.* No. 2714) the probable error of a right ascension of the best determined *Haupsterne* of the *Berliner Jahrbuch* is, for the epoch 1891, $\pm$ 0^s.014, *i. e.*, the average right ascension employed in the above discussion has approximately the same degree of precision as that of the best determined fundamental stars. It also appears that a single observation with the apparatus employed will furnish a better determination of the distance between two fundamental stars than can be obtained from the best coördinates available at the present time.

The adopted mean value of C — O is + 0".36 $\pm$ 0".05, indicating that the tabular refractions must be increased in order to satisfy the observations. Since this value depends upon the adopted coördinates of the stars, and may be supposed to be affected with some residual error from this source, it seems proper to show that a correction to the refraction may be derived which shall be entirely independent of the adopted right ascensions. The observing program contained six triplets of stars, each of which consisted of three stars within five degrees of the equator, each star of a triplet differing in right ascension from each of the others by approximately eight hours. The three arcs joining the stars of a triplet, taken in pairs are each very approximately 120° in length, and their sum when projected upon the equator must be exactly 360°. The refraction is to be determined from the condition that the sum of the projected arcs of each triplet, *i. e.*, the measured circumference of the heavens, shall equal 360°.

The individual values of C — O furnished by the several triplets, together with their weight are as follows:

Triplet.	A	B	C	D	E	F
C — O	+ 0".38	+ 0".40	+ 0".27	+ 0".17	+ 0".59	+ 0".17
Weight	5	5	3	3	4	6

The weighted mean of the several results is + 0".31, and the

close agreement of this value with that above found from the adopted right ascensions indicates that the residual error affecting the latter is well eliminated from their mean. I adopt as the definitive value of C — O, + 0″.34 ± 0″.04. Since the average value of the refraction for all the observations is 195″.0 it appears that the refraction with which the observations were reduced must be multiplied by the factor, $1 + \frac{0.34}{195} = 1.00173$, or to log α, which in the Pulkowa tables is called μ, there must be added, 0.00075 ± 0.00005.

If we combine the several corrections derived for the Pulkowa refraction tables and represent their effect as a correction to the mean refraction it may be put in the form

$$\varDelta \log \alpha = \varDelta\mu = +\,0.00075 - 0.00007{,}6b$$

where b is the mean value, in millimeters, of the aqueous vapor tension during clear weather at Pulkowa. I have no adequate data from which to derive a value of b. Its mean value for Madison is 6.6 and it seems probable that a somewhat greater value should be assigned for Pulkowa, but probably not so great as 10mm. Any value of b within these limits will furnish as a correction to the mean refractions a quantity less than ± 0.00028 which according to the data contained in *Obs. Poulk.* Vol. 5, is the probable error of the constant of the tables. The result of the investigation is therefore that the actual error of the Pulkowa mean refractions is less than the probable error assigned by the data from which they were constructed.

The comparison with Bessel's refractions (*Tab. Reg.*) is much less satisfactory. The temperature factors of this table, γ, are considerably in error and the mean refraction requires a correction represented by

$$\varDelta \log \alpha = -\,0.00063 - 0.00007{,}6b$$

Assuming the mean aqueous vapor tension to be 8mm the above correction is equivalent to multiplying the refractions by the factor 0.99715, agreeing approximately with the factor 0.9959 found by Nobile from corresponding observations at Capodimonte and Cordoba, and with the factor 0.9988 adopted by Stone at the Cape of Good Hope. It is however doubtful if the defects of Bessel's table can be cured by the use of any constant multiplier.

I cannot conclude this paper without touching upon one further conclusion to which these investigations have led me. The so-called anomalous refractions which have been made to serve as the explan-

ation of so many discordances in observations of zenith distances may be greatly reduced in magnitude by a more careful determination of the temperature at the objective of the telescope and by taking into account the barometric and thermometric gradients, *i.e.* the inclination to the Earth's surface of the strata of homogeneous air. From a discussion of the observations with the Pulkowa vertical circle, which are doubtless the most accurate zenith distances ever measured, Gylden finds as the probable accidental error of a computed *refraction*, R, $\pm$ 0.0015 R, which corresponds exactly in amount to the probable error of a single *observation* in the present series. Since a considerable part of the latter probable error must arise from instrumental and personal sources it appears that the attainable precision of the refractions has been underestimated and that errors chargeable to other sources have been attributed to fortuitous irregularities in the refraction. Since the effect of refraction upon the angular distance between two stars is nearly independent of their zenith distance, the effect of inclination of the strata of homogeneous air to the plane of the horizon is completely eliminated from the present series of observations. It is in large part due to this circumstance that the precision of the observations, $r = \pm 0''.30$ at a zenith distance of 68°, exceeds what has hitherto been attained.

ON THE DIAMETERS OF CERES, PALLAS AND VESTA.

E. E. BARNARD.

Our great telescope would seem eminently suited for a micrometrical determination of the diameters of those of the minor planets that have sensible discs.

In examining the brighter asteroids I found them to be readily measureable with the large telescope, and that their diameters can be obtained with a reasonable amount of accuracy. I have, therefore, undertaken the work of measuring a few of these bodies with the 36-inch.

Various attempts have been made previously to determine their diameters micrometrically with smaller instruments. These results are very discordant.

They have also been observed photometrically, but this method, it seems to me, is one of considerable uncertainty as it rests upon the assumption that their reflective surfaces are the same as that of some particular standard with which they are compared.

Such an assumption is rather risky and the results can only be very approximate at best.

I have so far succeeded in getting direct measures of Ceres, Pallas and Vesta, and I hope to continue the work as it is one of considerable importance.

As I have said, the previous measures of these bodies are very discordant, and were evidently made with instruments too small to deal with the subject. A collection of these measures, up to 1881, can be found in the *Vade-Mecum* of Houzeau.

I will copy here these values as there given for the four brighter asteroids.

CERES (1)

1802	W. Herschel measured diameter	0″.127
1805	Schroeter " "	1 .259
1839	Galle " "	0 .32
1866	Knott " "	0 .510

PALLAS (2)

1805	Schroeter measured diameter	1″.626
1807	W. Herschel " "	0 .09
1837	Lamont " "	0 .26

JUNO (3).

1805	Schroeter measured diameter	1″.144

VESTA (4).

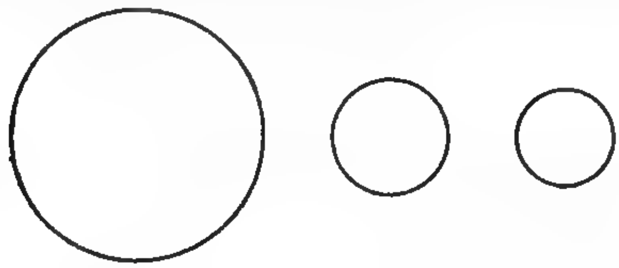
1816	Schroeter measured diameter	0″.313
1847	Mädler " "	0 .272
1855	Secchi " "	0 .428
1881	Tacchini " "	0 .830

[redacted]

Ceres (1)	diameter,	196 miles
Pallas (2)	"	171 "
Juno (3)	"	124 "
Vesta (4)	"	214 "

It will be seen that in both these tables, Vesta comes out the largest of the asteroids.

In connection with the above, I have thought it might be interesting to give my own measures, so far obtained, of the diameters of Ceres, Pallas and Vesta. I have not yet secured any measures of Juno, but will as soon as it is in favorable position.



CERES. PALLAS. VESTA.

The measures have been made with the 36-inch equatorial, and uniformly with 1,000 diameters. The details of the observations are reserved for a later and more complete paper.

FILAR MICROMETER MEASURES OF THE DIAMETERS OF CERES, PALLAS AND VESTA, MADE WITH THE 36-INCH EQUATORIAL.

CERES (1).

Diameter 1″.330 ± 0″.064 at distance unity.
Diameter 599 ± 29 miles (3 nights' measures).

PALLAS (2).

Diameter 0″.605 ± 0″.026 at distance unity.
Diameter 273 ± 12 miles (4 nights' measures).

VESTA (4).

Diameter 0″.527 ± 0″.033 at distance unity.
Diameter 237 ± 15 miles (3 nights' measures).

During these measures it was readily apparent that, though Pallas and Vesta were nearer than Ceres, the latter was noticeably twice as large as either of them. It will be seen that the measures also show this, thus proving that Vesta is certainly not the largest of the asteroids, and placing Ceres at the head of the list.*

* An inspection of Juno, without measures, shows it to be about comparable with Pallas and Vesta in size.

These three are apparently round and uniform in their light. Pallas is yellowish, Vesta slightly yellowish, while Ceres is whitish. There was nothing in the appearance of their discs to suggest any specially extensive or dense atmosphere.

The diagram on the preceding page is intended to show at a glance the relative sizes of Ceres, Pallas and Vesta, from my measures.

MT. HAMILTON, 1894, March 26.

THE PERSEID RADIANT.*

W. H. S. MONCK.

Mr. Denning in his article on the Perseid Radiant in *Popular Astronomy* for February reiterates his statement that the Perseid Radiant shifts its position from night to night moving eastward (with a slightly increasing declination) at the rate of a little over 1° each night from the 19th of July to the 17th of August. The duration he thinks may possibly be longer but the observations are rather too scanty to warrant that conclusion.

So far as I have been able to ascertain this shifting of the radiant is only established by selecting from among the meteors which come from Cassiopeia or Perseus at this period a certain number which fulfil the alleged law to which the name of Perseids is applied and rejecting as not genuine Perseids the meteors which do not fulfil the law. This will I think appear from an examination of Mr. Denning's great Catalogue (the most valuable that we at present possess) in the *Monthly Notices* of the R. A. S. for May 1890.

His initial position is at 19° + 51° on the 19th of July. He observed the radiant but once and then deduced it from 4 meteors only. He does not appear to have observed any meteors from the shifting radiant on the 20th or 21st but on the 22d and again on the 23rd he observed meteors coming from 25° + 52°. It is then lost sight of until the 27th when he finds it at 29° + 54° and on the same day he also obtained a radiant at 32° + 53°; on the 28th his determinations in different years gave 27° + 55°, 30 + 55° and (twice) 32° + 53°, after which he obtained meteors pretty nearly in accord with his shifting radiant up to the end of the maximum on August 12 if not later. But these meteors did not stand alone. On the 20th of July he obtained meteors from 21° + 57°. These might be referred to the shifting radiant if we

* Communicated by the author.

suppose an error of some degrees in the declination; but meteors from the same point were observed on July 28, July 29 and Aug. 1. Again the radiant at 32° + 53° gave meteors not only on the 27th and 28th of July but on the 30th and 31st and on the first of August, and if we identify it with a radiant at 31° + 54½° observed on the 29th of July we seem to have six days of continuous activity for this radiant without any change of position. The radiant at 21° + 57° on July 20th seems to turn up again at 20° + 58° on the 2d and 4th of August. 18° + 63 is another favorite radiant for meteors during this period. It was active on July 19, 20, 21, 23 and 24, and is no doubt identical with a radiant observed at 20° + 65° on July 22; and after some tolerably near approaches it is again found active on August 7 and 12. Even as regards the main Perseid radiant, Mr. Denning observed meteors from 43° + 58° on July 27, 28, 29 and 31 from 42° + 57° on Aug. 5, 41° + 58° on August 7 and 40° + 59° on August 20 and 21. (I have used the column "Other Nights of Observation" as well as the principal column in making out these details). All these meteors possess the same swift, streaky character.

I therefore conclude, 1st, that the shifting radiant has not been observed at every point of its supposed career, but that its position on several days has been obtained by interpolation. This is a legitimate process after the shifting has been proved but not till then. 2nd, the starting position is deduced from a very few meteors and I may make the same remark as to the final position. 3rd, several points on (or nearly on) the track of Mr. Denning's shifting radiant produce meteors both before and after this shifting radiant is supposed to pass over them—this being specially true of the principal radiant and of another radiant situated at about 32° + 53°.

In fact I believe both of these are permanent radiants. The principal radiant situated at somewhere about 44° + 57° is not only active (at least intermittently) from July 27 to Aug. 21, but before and after these dates. It turns up at intervals in Mr. Denning's Catalogue from the 14th of June to the 10th of December. The radiant at 32° + 53° does not seem to occur earlier than the end of July when its activity is at a maximum; but I find it at 32° + 50° on Aug. 14.23, at 33° + 54° on Sept. 6.9, at 31° + 52° on Sept. 21.25 and at 32° + 50° on Oct. 8.

I have confined myself in these remarks to Mr. Denning's own observations. I might perhaps have confirmed them by introducing the observations of others. But I suspect that there is a good deal of what is called "personal equation" in the observa-

tions of the majority of observers which renders their figures hardly comparable with each other. Mr. Denning is one of the oldest and most painstaking of meteoric observers and his observations may at all events be regarded as following the same scale. And so far as Mr. Denning confines himself to observation I have no quarrel with him. But the shifting of the Perseid radiant is not a fact of observation, but an inference from observations, and in my opinion an inference which the observations do not warrant. Nor is the greatest skill in making observations any protection against drawing erroneous inferences from them. All Mr. Burnham's accuracy in determining the distances and positions of a pair of binary stars does not render him infallible in his determination of the orbit; but to make the cases really parallel, we should suppose that the larger star was surrounded by a number of smaller ones which could only be glimpsed occasionally on a clear night, and that Mr. Burnham's orbit was made out by assuming the identity of a certain number of these glimpse-stars notwithstanding that small stars had more than once been glimpsed in the old places of supposed *comes* long after the latter was supposed to have moved further on in its orbit.

It is true that on the current theory a meteor radiant ought to shift night after night, and I may add that I do not see how it could continue for anything like a month. But as a matter of fact the great majority of radiants which continue for some nights do not shift. Where the radiant is not a diffused one it seems to be almost absolutely fixed, and in the case of a diffused radiant there is, as a rule, no steady shifting but rather a fluctuation of the supposed central point The current theory must be wrong, for it does not explain the facts. When it is replaced by a theory which affords an explanation of the fact of stationary radiation (the ordinary case) the new theory will probably show whether there are exceptional circumstances which might cause a shifting, and whether these exceptional circumstances occur in the case of the Perseids or not. The facts as at present known do not seem to me to establish any shifting; but they might perhaps also be explained (1), by the shifting of a diffused radiant, or (2), by a shifting radiant passing nearly over two or more stationary radiants in its course. The latter is probably Mr. Denning's idea, but I do not think it will explain the facts unless there is a good deal of diffusion in the shifting radiant itself. I include the existence of sub-radiants under the head of diffusion.

THE CANALS OF MARS.*

J. R. HOLT.

The general opinion with regard to Schiaparelli's canals is that they represent cracks in the crust made during solidification, and in consequence, that they are older than the seas. On the other hand, some astronomers incline to the view that they are artificial; the inhabitants having rectified the rivers and dug other canals, probably with a view to irrigation. Other hypotheses which I have seen put forward are, (1) that they are furrows formed by aerolites, (2) that they have been formed by tidal erosion. A consideration which seems to me to constitute to some extent a discriminating test between these views is as follows.

If the canals are cracks which existed before the present seas, we might expect that some of the longer cracks run across the sea bottoms: generally speaking, they would be invisible while actually traversing the sea, but would reappear on islands or continental surfaces. On these, they would appear as continuations of the original cracks. It is even possible that under very favorable circumstances, the canals may actually be traced across the shallower seas. On the other hand, if they result from the rectification of the rivers, any such prolongation, if observed, must be merely accidental, and although, if they are entirely artificial, we cannot deny that the inhabitants might have *some* object in so constructing them, we cannot imagine what the object could be. Consequently, if such prolongations are observed in greater number than can fairly be ascribed to mere coincidence, the fact is so far evidence in favor of the more general view, as against the hypothesis that they are artificial.

Now, on a careful examination of Schiaparelli's map, given at p. 440 of M. Flammarion's "La Planète Mars", I have come to the conclusion that such prolongations do exist. The following cases seem unmistakable.

(1) Eumenides—Nectar, (2) Pyriphlethegon—Ambrosia, (3) Triton—Ascanius, (4) Gigas—Scamander, (5) Galaxias—Xanthus, (6) Astaboras—Nepenthes.

The following, although more doubtful, seem reasonably probable.

(1) Anubis—Alpheus, (2) Cerberus—Peneus, (3) Tartarus—Herculis Columnæ. Another consideration is that these cracks were lines of weakness in the crust, and therefore probably had consid-

* Communicated by the author.

erable influence in determining the distribution of sea and land. Thus the Mare Cimmerium seems to have some connection with the Triton; the Mare Tyrrhenium with the Typhon; the Mare Sirenum probably with the Tartarus; and Herschel II Strait, and possibly the Hadriaticum Mare, with the Hydraotes. Professor Pickering has traced some of the canals across the seas. This is also a further confirmation. Lastly, we can find many instances of canals apparently prolongations of one another, in which the intervening portion crosses the *land*. Thus, for instance, the Typhon—Jordinas.

In these cases, the most obvious view is that the intervening portion of the canal exists, but that for some reason, it is not conspicuous. I drew up a map, made by copying Schiaparelli's chart, and marked these hypothetical canals, to the number of some 20 or 30, on it. Almost every canal, shown on drawings made subsequently to Schiaparelli's chart, falls into its place on it.

All these arguments make for the view that the canals are cracks in the crust, as against the view that they are artificial.

But they make with equal force against the tidal erosion and aerolite hypotheses. In addition, there are other arguments against these views. Thus the tidal view is that the water found its way from one ocean to the other across the land. If the canals were straight parallel lines, which did not intersect, this view might be tenable; but I cannot see how the straight intersecting lines could be so formed. So with regard to the aerolite hypothesis; the idea seems to be that the rarified atmosphere of Mars would not be an effective screen. But this leaves out of consideration the slow decrease of density. If we take this into account, we find that, even if the density at the surface is only one-tenth of ours, the atmosphere is a much more effective screen against meteorites than ours.

Accepting the view that the canals are cracks in the crust, the next point is as to the mode of formation. The suggestion which first occurs to the mind is that they were formed by contraction of the crust during cooling; but it is not certain that similar cracks *could* be formed in this way. M. Flammarion mentions that M. Daubrée tried to produce a similar appearance by covering a hollow caoutchouc globe with a layer of paraffin, and subjecting it to pressure. Nothing resembling the canals was produced, although our terrestrial mountains were fairly imitated; on the other hand, when the globe was distended by water being forced into it, similar cracks *were* produced. M. Lebour has

pointed out the resemblance of these canals to cracks in glass broken by torsion. Another consideration is, that when a molten body solidifies, at first the crust contracts more rapidly than the still molten nucleus; the crust accordingly closes in on the latter, and compresses it, so that the molten matter is forced up through the cracks; this may be what has occurred in the case of the Moon, and the great difference in aspect between the cracks in the Moon and Mars, is evidence that there is a fundamental difference between the ways in which they originated. Taking all these considerations into account, I have been led to form the following hypothesis.

A molten mass of badly-conducting material freely cooling, would soon form a thin crust, while the matter a short distance below the surface was still perfectly liquid. This formation of a thin crust is in general prevented by the disengagement of gas from the molten mass, which breaks it up at once, before it has time to get thick enough to have sufficient strength to withstand the gas.

Now I suppose that in the case of Mars, for some reason, there was a temporary lull in the emission of gas, sufficiently long to allow of the formation of a thin crust, so strong as not to be broken up by the gas when emission began again, but not strong enough to be able to compress the gas when contracting. Consequently, when it began to contract, being prevented from closing in by the expansive force of the imprisoned gas, it shrank laterally, forming long rents. It is even possible, that between the force of expansion tending outward, and gravity tending inward, portions of the crust may have been subjected to actual torsion, and some of the canals may have been formed under its influence; in general, however, my suggestion is that they are, properly speaking, rents formed by lateral shrinkage. As soon as these fissures had formed, the imprisoned gas escaped, and the ruptured crust fell in upon the nucleus; but by this time, the latter had cooled to some extent on the surface, and hence was not easily forced up through the fissures; besides, the only force tending to so drive it up would be pressure caused by gravity, resulting from the cooled fragments of the crust being somewhat denser than the still molten material; there would be no force of compression acting. Under these circumstances, the molten matter would not be forced up through the fissures, or at any rate, not completely, so as to fill them.

The resemblance of this hypothesis to M. Rateau's hypothesis of sub-continental gaseous belts, *L' Astronomie, 1893, p. 412,* will

be at once apparent. In fact, the idea occured to me eighteen months ago, but I dismissed it as probably absurd, until the publication of M. Rateau's hypothesis.

The fundamental difference between the cases of the Earth and Mars, I take it, is as follows: In Mars a thin crust was early formed and ruptured. The gas then escaped, and the fissures served as vents for the internal volcanic energy, so that the crust has remained almost in its primitive condition ever since. On the Earth, the crust was not formed so early; in consequence, it escaped rupture as a whole, and was able to compress the gas.

The volcanic energy, not having any preformed vents, deformed the crust considerably, and formed vents for itself here and there. These, however, were insufficient to allow of a complete escape; in consequence, the surface has suffered from volcanic action to an amount contrasting considerably with Mars.

This hypothesis is of course very crude; however it may possibly suggest a better one.

A curious fact, which the above does not explain, and which should be explained by any adequate theory, is the way the canals fall into parallel groups. Thus in (1) we have Phison, Hiddekel, Oxus, Jamuna, Chrysorrhoas, Cyclops; in (2) Asutapes, Anubis, Euphrates, Gehon, Hydaspes, Ganges; in (3) Araxes, Gigas, Avernus, Cerberus. Many other groups may similarly be noted. Portions of the coast often are parallel to the canals in one of these groups. Thus the coast of the Hour-Glass Sea, from the mouth of the Asutapes to Cape Banks is parallel to group (1).

So the coast at one side of Margaritifer Sinus is a continuation of the Oxus; one side of Auroræ Sinus is parallel to Nilosyrtis, Astaboras, Typhon, Jordanis, Hydraotes-Nilus, Eumenides, Erinnys, etc. This suggests that these coast-lines are really canals. Direct evidence of a portion of the coast being a canal, is, of course, very seldom attainable. In M. Schiaparelli's obserservation of 20 June, 1890, *La Planète Mars*, p. 476, he saw the south coast of Beer Continent (Herschel II Strait) transformed into a double canal. This is to some extent confirmation of my hypothesis; *i. e.*, when the fragments of the crust, settled down on the nucleus, abrupt changes of level only occurred at the edges, that is, the canals.

Passing on from the question of origin, a new question arises as to the present state. The most usual opinion is that the canals are rivers. In favor of this theory we have (1) their dark colour, like water, (2) the fact that no other rivers have

been observed, (3) the consideration that water would certainly find its way into these ravines, (4) the way in which they terminate in bays on the coast. Against this theory we have (1) the way in which these canals run from ocean to ocean, (2) their nearly uniform width, (3) the consideration that if all are rivers, Mars is much more richly provided with large rivers than the Earth.

At this juncture, we may possibly be helped by considering what would take place, if the Earth were similarly seamed with ravines.

Obviously, the surface drainage would sooner or later find its way into them, and so we would have rivers formed; it is also obvious that the surface drainage would in general not be sufficient to fill them completely, so that we would have ravines with rivers flowing down the middle. Obviously, too, although a ravine might reach from ocean to ocean, the contained river would not do so. In fact, the one ravine might contain two rivers, flowing in opposite directions, one discharging into one ocean, the other into the other. Now, that this should be the case with the canals, it is necessary to suppose that the dry portion of each ravine, on either side of the river, is undistinguishable from water; either through the soil being of a dark colour, or through it being clothed with a dark-coloured vegetation.

I will return to this point again; for the present I will pass over the question as to *what* gives the dark colour to the soil.

Now in this hypothesis, that only the middle of each canal is water, we find a possible explanation of an observation made by Schiaparelli on Dec. 26, 1879. He saw a broad white streak running from Lake Pheonix in a N. N. E. direction, crossing the Fortuna and the doubled Nile, and seeming to join an extension of the polar snows. This white streak appeared to him to be obviously snow, (probably it was the track of a heavy snow-storm) and he examined carefully the place where it crossed the Nile, as he expected that if the latter were water, the snow would melt in it, and the streak be interrupted; while if the Nile were merely a marking on the land, the streak would of course pass across it without alteration. As a matter of fact, the Nile was not absolutely interrupted, but it was reduced to a mere thread running across the white streak. This observation seems to have been regarded as very puzzling. And yet, on the hypothesis that only the middle of the Nile is water, no other appearance could have been expected. This observation is, however, also explicable on the assumption that the Nile was frozen over, except in the mid-

dle; and the time of year, (shortly before the vernal equinox) is consistent with an assumption which involves extreme cold. The following, however, is not so easily explained.

These rivers, running down the middle of each ravine, might be expected to expand into lakes here and there, especially where two or more united. In some cases, these lakes would be less in diameter than the canal, and then, as seen from the Earth, the curious appearance would be presented of a small dark spot *in* the canal.

Such spots have been seen by Professor Pickering, and I cannot suggest any other explanation which is even reasonably probable.

It may be urged that if we can see the lakes, we ought also to be able to see the rivers themselves, as darker streaks down the middle of each canal. But it must be remembered that the lakes are probably in general, deeper than the rivers, and so look darker; (2), that the water of the rivers is more likely to be turbid, and so look lighter than that of the lakes; (3), that the river water, being in motion, may have its surface diversified with ripples, eddies, etc. which would make it look lighter from a distance. And, under very favorable circumstances, the middle of a canal *has* been seen darker than the edges.

As to the question whether the margins of a canal are clothed with vegetation, or merely have a darker color than the rest of the soil, either by being marshy, or any other reason, it is at present hardly safe to speculate; at the same time there is a good deal to be said for the hypothesis of vegetation. Firstly, if vegetation exists at all on the surface of Mars, along the banks of the rivers would seem the most likely place to look for it: secondly, the changes which have been observed in the appearances of the canals seem to point that way. As a single instance, take the Ambrosia; on the 26 September, 1877, one day before the summer solstice, this was seen to be broad and greyish: in November and December, 1879, it was seen as a fine, black line. This is exactly what we would expect if the fine, black line represents the *river* Ambrosia itself, and it is fringed by a broad belt of deciduous trees. At the time of the first observation, being the summer solstice, they were in full leaf; while at the second, which was shortly before the autumnal equinox, the leaves had changed color, or had fallen, so only the river itself was visible. This suggestion may seem to ascribe a degree of similarity to the Martian and Terrestrial vegetation which is *a priori* improbable, but having gone carefully through all the observations recorded

in "La Planète Mars," I have come to the conclusion that if vegetation exists at all on Mars, and the changes in appearance of the dark markings are due to this cause, it very closely resembles our terrestrial vegetation in the way it varies with the seasons.

The next matter to be considered is the duplication; this is generally regarded as optical, not in the sense of an illusion, but as being an unreal appearence, due to the meteorological conditions prevailing on the surface of the planet. Two explanations have been put forward; the first, due to Mr. Proctor, attributes the phenomenon to fogs resting on the rivers; the second, due to M. Meunier, ascribes it to a thin veil of cloud or fog between our eyes and the canals, he having found by experiment that a gauze screen held before lines, etc., duplicates them.

On independent grounds, I have seen reason to believe that there is a very thin cloud-screen at a height of between 10 and 20 miles from the surface, so that M. Meunier's may be the correct explanation. I doubt, however, if it is adequate to explain all cases; it seems to me, that there ought to be some reasonable proportion between the height of the screen and the apparent width of the duplication; and I do not exactly see how a cloud-screen, whose distance from the surface cannot much exceed 20 miles, can cause apparent duplications 200 or 300 miles wide. It seems to me more likely that only the narrower duplications are caused in this way, and that the wider are real.

A hypothesis to explain such cases, is, that of two canals, closely adjacent, and also parallel or nearly so, both draining the same area, one would come to be more used than the other; so in seasons of drought, the less used river would get choked up by stones, etc., falling into its bed, which the flow of water would not be sufficient to remove. So that in time there would only be one active channel. Then, in seasons of flood, the ordinary channel would not be sufficient to carry off all the water, consequently there would at first be flooding, evinced by a general haziness of aspect, until the water had succeeded in forcing its way down the second channel, and scouring it out, after which both canals would be visible.

This hypotheis is apparently inconsistent with the assumption that only the middle of the canals is water. Possibly they may be reconciled, or they may refer to different canals.

It will be seen that I regard the canals as the most ancient features of the surface of the planet. Not only that, but they are in a sense the key to all the others. They have determined the

distribution of sea and land, and fixed the course of the rivers from the first, so that only very insignificant variations have taken place, and traces of fluviatile action are confined to the canals. Moreover, by their acting as vents for the subterranean energy, traces of volcanic action are probably insignificant, and confined to the canals. Mountains are almost exclusively found along the margins of these latter, and, although some may be volcanic, it seems to me more likely that the majority simply result from the fragments of the crust being tilted up. In this case, their form would be totally unlike that of our terrestrial mountains; on one side there would be an abrupt fall, and on the other a gradual slope.

As there have been no upheavals, and no changes produced by fluviatile erosion, the only inportant changes which the surface of Mars has undergone in the course of ages have been those produced by the gradual recession of the sea. It seems not improbable that all the older continents are to a large extent desert, and that life, if it exists at all, is confined to the lands newly left bare by the sea, (as Hesperia, Atlantis, Libya, Thaumasia, etc., probably) and the canals. The fluviatile erosion in these latter must have been enormous, if the rivers have been running for millions of years in practically the same channels.

In the latter part of this, for brevity I have spoken dogmatically; it will of course be understood that these results are merely put forward as probable if the fundamental hypothesis (that the canals are cracks in the crusts of very ancient date) be granted.

DUBLIN, Ireland, 6 Harrington St.

and OΣ catalogues. At no other Observatory in the world has this class of work been so systematically and continuously carried on. For three-quarters of a century, the Struves, father and son, at Dorpat and at Poulkowa, made this their life work. Fortunately they did not, as official heads of these great observatories, consider it an important or necessary part of their duties to direct the work. They went ahead for this long period of time and made the measures themselves; and the result is that in these splendid publications of the Struves, every observation from first to last is the personal work of one of these great observers. It is hardly necessary to say that this gives the measures a much higher scientific value than they would be entitled to if made in any other way.

The measures comprising the present volume are divided into four sections:

Section I embraces the observations of Herschel's Classes V and VI. These stars are taken from the Catalogues of Herschel, Struve and O. Struve. Some of these stars are more closely double, and measures of some of the closer pairs will be found in the other sections. Most of the distant components show very little change, although in some instances variation from proper motion is manifest. Many of the principal stars are prominent and well-known objects, such as α Cassiopeæ, λ Arietis, θ Arigæ, ι Boötis, etc.

Section II deals with stars having large proper motions. Many of them are known as double stars by virtue of a near component traveling in space with the primary. The list includes most of the prominent stars known to be moving in this way, and as the measures connect with stars too remote for any probable connection with the primaries, the best possible data is furnished for a very exact correction of the movement as shown by the meridian observations. Nearly all the pominent stars having considerable proper motions are included in this section such as α Andromedæ, η Cassiopeiæ, 40 Eridani, α Tauri, Procyon, Castor, γ Leonis, 61 Cygni, etc.

Section III is devoted to stars of more modern discovery, and particularly to the β pairs. It includes also some of the discoveries of the Clarks, Dawes, Herschel and others. Most of the measures have been made during the the last twenty years. These pairs are nearly all of an interesting character.

Section IV embraces measures since 1875 of Σ and OΣ pairs. In many instances they are of special interest either from the rapid motion of the components, or because recent measures are

wanting. Among the binary systems we have 40 Eridani, 32 Orionis, ζ Cancri, ω Leonis, 42 Comæ, μ Herculis, φ Ursæ Majoris, OΣ 234, OΣ 536 and others of a like character. In this connection it is worthy of note that OΣ suspected in elongation of 72 Ophiuchi (OΣ 342) in 1877. This star has been examined many times in the last twenty years by various observers, including the writer, but without seeing any trace of the suspected companion. Evidently the star should be watched, since it is possible that it may be double, and in very rapid motion. At the end of the work we have an index to all the stars in volumes IX and X, arranged in order of R. A. To some extent this will do away with the practical inconvenience of referring to a work of this character where there is any classification of the double stars.

No one can more sincerely regret than the writer that this is probably the last extended contribution of the great Russian astronomer, so far as practical observations are concerned, to the literature of double stars. It would be difficult to overestimate the value of his services which cover a period of more than fifty years. His brilliant discoveries of remarkable binary systems, and his great skill as an observer will ensure to him a prominent and enduring place in all the astronomical records of the future.

RECENT OBSERVATIONS OF THE SATELLITES OF JUPITER.

EDWARD S. HOLDEN.

In Astronomy and Astro-Physics for April, 1894 (page 272), and in the *Monthly Notices*, R. A. S., for January, 1894 (page

quired that some of the large instruments, now so plentiful, should confirm his conclusions.

Second.—In the *Publications* A. S. P., Vol. III, pages 355 and 359, Professors Schaeberle and Campbell described markings on Satellite III of Jupiter, and announced that Satellite I "is ellipsoidal, that its largest axis is directed towards the center of Jupiter, and that the other satellites appear to be spherical." Dr. Barnard's conclusions are that all the satellites, including I, are spherical. Professors Schaeberle and Campbell have continued their observations since 1891, and have so far seen no reason to change their conclusions above given, so far as I know. In the same paper they conclude that the periods of axial rotation and of revolution of satellite I are equal. Dr. Barnard says that his observations (as yet unpublished) lead to a different result.

Third.—The appearance of Satellite I in transit as a double body, as observed in 1890 by Professors Burnham and Barnard, is now explained by Dr. Barnard as due to simple contrasts between bright regions on the planet and two extensive dusky polar caps on the satellite (which are separated by a brighter belt).

The simple theory of contrast is probably fully adequate to explain all observed appearances of the phenomena of the transits of the satellites (dark transits, etc.), as has been pointed out in the *Publications* A. S. P., Vol. II (1890) page 296; Vol. III (1891), page 358, and in other places.

Fourth.—Dr. Barnard next considers the transparency of the limb of Jupiter with reference to the question whether the light of a satellite undergoing occultation can be seen through the planet's atmosphere, and says, "I think it is high time that astronomers reject the idea that the satellites of Jupiter can be seen through his limb" since under good conditions, with the 36-inch telescope, "the limb of Jupiter has appeared perfectly opaque, as in all my previous observations with smaller telescopes."

It is quite possible that the limb of Jupiter is really opaque to the light of satellites or stars, but it does not always appear to be so. Dr. Barnard has himself described the appearance of a star at occultation shining through the atmosphere of the planet (see *Astronomical Journal,*) Vol. VIII, page 64), though the observation had probably escaped his memory when he wrote the sentence just quoted. It is conceivable that the observations of satellites which he criticises were due to the same causes which affected his own observations of the star in question. At any rate his words "the star was last seen with three-quarters of its disc within the limb" of Jupiter, show that good observers have sometimes recorded appearances of the kind.

These few points from recent papers show very forcibly that everything is not yet settled with respect to Jupiter's satellite system, and may serve to direct the attention of the possessors of large telescopes to some of the problems involved.

Lick Observatory, April 10, 1894.

Astro-Physics.

ON THE SPECTRUM OF β LYRAE.*

H. C. VOGEL.

The variable star β Lyræ, remarkable through the peculiar form of its light curve, is to be counted among the most interesting spectroscopic objects in the northern heavens. The spectrum belongs to Class I, and extends far into the violet. In the visible portion it is crossed by individual bright lines, among which the hydrogen lines $H\gamma$ and $H\beta$ and the D_3 line are especially conspicuous.

The idea of connecting the bright lines in the spectrum of β Lyræ, which seemed to undergo changes of visibility, with the light-variation of the star, was one which naturally occurred to observers; nevertheless the various attempts which were made to discover such a relation led to no satisfactory and conclusive result, and a new interest in the star was first awakened by Pickering's communication (in A. N. 3051), announcing that photographs of the spectrum revealed the presence of closely adjacent bright and dark lines whose relative positions were subject to change.

Unfortunately, the prospect at the Potsdam Oservatory of being able to engage in work on this interesting object, was very slight, owing to the lack of an instrument which would give a spectrum of sufficient brightness. The star was too faint for the large spectrograph with which the motions of stars in the line of sight had been determined; and when instruments with less dispersion were connected with the 11-inch refractor, the effect of the visual correction of the telescope objective was at once apparent, for only a small part of the photographed spectrum appeared sharp, and the investigation must have been confined to essentially a single line. The success which had been obtained in an investigation of the spectrum of Nova Aurigæ, with a spectrograph of small dispersion used in connection with the 13-inch photographic refractor, justified the expectation that details could also be found in the spectrum of β Lyræ that would not be without importance in adding to our knowledge of the nature of this star, and I therefore took some pains to perfect the instrument which had served in the above-mentioned observations, and

* Translated from the *Sitzungsberichte der k. preuss. Akademie der Wissenschaften zu Berlin.* 1894. VI.

which had been constructed in only a provisional manner, and above all to increase its stability.

With the improved apparatus the spectra are of extraordinary sharpness. Owing to the fact that the collimator and camera objectives are achromatized for the same rays as the objective of the telescope, star spectra are obtained with this apparatus which are nearly equally sharp throughout the great extent of spectrum between λ 380 and λ 450. The dispersion is somewhat greater than with the earlier apparatus, in the ratio of about 5:4.

The material yielded by the observations up to the present time is, thanks to the zealous labors of Dr. Wilsing, quite considerable in amount. It is especially valuable for the reason that several photographs were taken on each observing night, with different exposures and breadth of spectrum, thus securing a result for the night which was as free as possible from accidental errors, and was in general dependent only upon the more or less favorable condition of the atmosphere. The times of exposure varied between 15 and 60 minutes; the breadth of the spectrum, produced by slightly altering the rate of the driving clock, varied between a mere line and a breadth of 0.4 mm. Photographs taken with a breadth of 0.2 mm, have proved to be the best, with respect to certainty of measurement and the recognition of fine details.

From March 25 to December 22, 1893, Dr. Wilsing made 144 photographs. Besides these, 7 plates were made on three evenings in November, 1892, by Dr. Wilsing, and 9, on nine evenings in April and May, 1892, by Professor Frost. Sixteen per cent of the total number of plates were unsuitable for a detailed investigation; among these are reckoned the plates with too short exposure, which are, however, instructive in a certain sense, inasmuch as they show the great importance of nearly correct exposures in the scientific application of photography, and how details, easily recognized on a sufficiently exposed plate, can be entirely lost on an under-exposed one taken on the same evening. The strong and disturbing influence of the state of the atmosphere, especially of a thin veil of haze, which in many other astronomical observations is so advantageous, was in general very noticeable in the photographs.

With regard to the spectrum of *β* Lyræ in general, measures of particularly suitable plates by Dr. Wilsing and myself show that the whole series of hydrogen lines from Hγ to Hδ is present.*

* I here use for the first time the momenclature for the violet hydrogen lines which was proposed by me (A. N. 3198) and accepted by Huggins, instead of the earlier one by Huggins; I shall however, in the first examples, add in brackets the old symbol for the best-known lines.

The lines appear as broad, for the most part definitely bounded absorption bands. Besides the conspicuous line K, some other lines, resembling those of hydrogen, and several delicate lines, are visible in the spectrum. On the less refrangible side of nearly all the stronger absorption lines, bright lines sometimes appear, among which Hγ and Hζ (α) are especially noticeable.

The results of measures of plates taken on March 26, April 7 and 8, December 9 and 22, 1893, are collected in the following table. The wave-lengths have been determined with the aid of a table based on very numerous measures of solar spectra taken with the same apparatus. The uncertainty, between Hγ and Hη (β), is, liberally estimated, about, $\pm$ 0.15 $\mu\mu$. For lines near the limit of the photographed spectrum, which cannot be determined so accurately, the value given above is quite exact. It should not remain unmentioned that in the determination of wave-lengths the assumption has been made that the absorption line Hδ in the star spectrum coincides with this line in the spectrum of the artificial source, which has no motion relatively to the observer. It has not been possible hitherto to determine *absolute* wave-lengths in the star spectrum with greater accuracy then 0.15$\mu\mu$.

Micrometer. Reading.	Wave-length.	Description of Line.
r − 1.00	370.4$\mu\mu$	Weak absorption line at the limit of visibility. Hξ.
− 0.40	371.2	Weak absorption line. Hν.
+ 0.40	372.2	Weak absorption line. Hμ.
1.40	373.5	Weak absorption line. Hλ.
2.64	37.51	Absorption line. Hχ.
3.51	376.3	Delicate absorption line.
4.08	377.1	Broad absorption line. Hι.
6.03	379.8	Absorption line. } Hθ.
6.25	380.2	Bright line, very weak. } Hθ.

PLATE XIII.

Spectrum of β Lyrae (Vogel)

from λ 380 $\mu\mu$ to λ 450 $\mu\mu$

Micrometer Reading.	Wave-Lengths.	Description of Line.
r 19.97	402.6	Absorption line; strong. } Conspicuous lines, resembling the hydrogen lines.
20.17	403.0	Bright line; broad.
21.86	406.2	Absorption line. } Very faint.
21.96	406.4	Bright line.
22.15	406.8	Absorption line. } Very faint.
22.32	407.1	Bright line.
23.84	410.2	Absorption line; strong, broad. } Hδ.
24.03	410.6	Bright line; broad.
24.28	411.1	Absorption line. } Rather weak.
24.42	411.3	Bright line.
24.73	412.0	Absorption line. } Weak.
24.93	412.4	Bright line.
25.22:	413.0	Absorption line; narrow, weak.
25.40:	413.3	Bright line; weak, very wide.
25.83	414.3	Absorption line.
26.04	414 7	Bright line.
34.16	434.0	Broad absorption line. } Hγ
34 34	434.5	Broad bright line.
35.95	438.8	Absorption line; broad.
36.15	439.3	Bright line; broad, very weak.
38.93	447.0	Broad absorption line. } Conspicuous lensis.
39.10	447.5	Bright line.
39.33	448.1	Absorption line; narrow, weak.
39.44	448.4	Bright line; very weak.

In the accompanying plate* I have also given a representation of the spectrum as it appears at the time of a principal minimum, as there has hitherto been published only a map of the visible part of the spectrum between D_3 and Hγ in the memoir on β Lyræ by Bélopolsky, to which these observations from an extension.

Changes in the spectrum, apparently having some relation to the period of the star, were immediately and easily recognized in the observations of March and April, 1893, which were made during an extraordinarily favorable period of weather. It first appeared, without any doubt whatever, that the intensity of the continuous spectrum varied with the light-phase, and that neither a variation of relative intensity in separate parts of the spectrum, nor a conspicuous change in the general nature of the spectrum, was immediately involved in the different light-phases of the star. It cannot be certainly determined whether the bright lines increase in brightness with the continuous spectrum or not; but in any case they do not merely make their appearance at the time when the star is brightest, as it has been asserted that they did. On the contrary, the photographs which have been taken here point rather to the opinion, that the bright lines do not take part in the periodic light-variation of the star, for they are most

* The plate is as close a reproduction as possible of Professor Vogel's plate in the *Sitzungsberichte.—Ed.*

conspicuous at the time of the principal minimum, very likely in consequence of the contrast with the weaker continuous background. The number of lines is also subject to no regular variation; it is constant for the principal lines, and the visibility of a considerable number of finer lines, bright as well as dark, which can be recognized at times, seems to depend only upon the particularly favorable atmospheric conditions under which they are obtained, or the excellence of the photograph. (Such especially good photographs lead to the supposition that with sufficient dispersion the spectrum would be found very rich in lines.)

Only small changes, undoubtedly standing in a certain relation to the periodic variations of the star, can be observed in the relative positions of the bright and dark lines, which for the most part are arranged in pairs throughout the spectrum. It appears that at the time of a principal minimum the absorption lines come out very distinctly, and the bright lines lie close to their less refrangible sides. At the time of a maximum the absorption lines are less distinct; at the time of the first maximum the bright lines are still on the side toward the red; at the time of the second maximum they fall on the absorption lines, (which in consequence appear very narrow), and assume the aspect of bright lines situated one on each side of the dark line, although on account of the greater brightness of the continuous spectrum they are often hard to see. The aspect of the lines in the intermediate minimum is nearly the same as at the time of the second maximum, but the absorption lines are broader and more conspicuous.

The above observations apply to most of the stronger lines between λ 385$\mu\mu$ and λ 450$\mu\mu$, and not to the whole spectrum shown on the plate; for the intensity falls off very noticeably at λ 385 if the correct exposure has been given to the other parts; and no reliable statements, based on the plates which are at present available, can be made as to the behavior of the more refrangible hydrogen lines. Some of the plates show that in this part of the spectrum also, bright lines are at times situated close to the absorption lines, while at other times the bright lines fall on the absorption lines and cause them to appear very faint.

The phenomena described above appear in a much more pregnant manner in the case of the Hζ line. This is by far the most striking line in the photographic spectrum. While some of the bright lines are at times scarcely perceptible, the bright line Hζ is always plainly evident; in poor photographs it is often the only feature in the spectrum worthy of remark. The bright line is narrower than the other lines of hydrogen; the absorption line is al-

ways sharply bounded, and it is outlined with quite especial sharpness when apparently enclosed by the bright line. It is remarkable that the absorption line is absolutely dark; in the negative, forming a spot free from the slightest deposit of silver, it is often brightly transparent. This appearance may in part be due to the peculiarity of photographic processes, that contrasts brought out by powerful development are stronger than they are in reality; but the behavior of this line as compared with that of the other lines in the spectrum remains a remarkable fact, well worthy of special consideration.

Although I do not regard our observations of β Lyræ as completed, I have sought to collect all the hitherto observed phenomena relating to the Hζ line in a preliminary manner, in order to secure a basis for further investigations, and give them here in some detail, as they may also be of some importance to other observers. The observed changes are noted with reference to the phase of the star's period, since as I have said, the first observations revealed a certain connection between the two phenomena.

A. 1. At the time of the principal minimum and the maximum following it, the bright line lies close to the dark one, displaced in the direction of greater wave-length. Occasionally,—particularly at the time of the first maximum,—the dark line is bounded on the upper side also by a bright but quite narrow line. (Fig 1 and 2, 1893. April 7 and April 10).

2. At the second minimum the absorption line lies in the middle of the bright line, in such a manner, however, that the red component of the bright line, which then appears as a wide double, exceeds the other in breadth. On the less refrangible side of the bright line appears a weak absorption line.

3. At the second maximum the absorption line lies either exactly in the middle of the bright line, or toward one side, so that the more refrangible component is somewhat the broader. The above-mentioned absorption line on the less refrangible side of the bright line, is very distinct, and quite characteristic of the appearance at the time of the second maximum. Occasionally two or three more lines can be recognized, at a short distance toward the red. (Fig. 4, 1893, April 4).

4. While the passage from the first maximum to the second minimum is a quite gradual one, the change in the aspect of the double Hζ line between the second maximum and the principal minimum is very sudden,—indeed, it occurs shortly before the advent of the principal minimum.

B. The aspect of the lines described above, which is that

shown on most of the photographs, is sometimes very perceptibly different. At the principal minimum and first maximum the bright line does not lie entirely on one side of the absorption line, but encloses it, as in the case of the second minimum (A, 2) although the red component of the bright line is considerably broader. At the time of the second minimum and the following maximum, the more refrangible component of the bright line is considerably broader than the other component, and on the photographs of Oct. 13, 1893, (between the second minimum and the second maximum) the bright line can be seen on only one side of the absorption line, displaced in the direction of smaller wavelengths, and hence the arrangement is just the reverse of what it is (A, 1) at the time of the principal minimum.

The differences mentioned above are not, however, peculiar to the Hζ line, but are found in the other lines also. Thus on the plate of Oct. 13 several of the absorption lines have bright lines in their more refrangible sides. The impression produced is that the bright lines are displaced with reference to the dark ones throughout the whole spectrum.

The series of observations at the times of these conditions of the double lines in the spectrum of β Lyræ is still defective, but there is little doubt that further observations will enable us to succeed in discovering the cause of these hitherto unknown or unnoticed changes which may, be supposed to require a longer period, perhaps one of several months.

C. Not only the bright line Hζ, but the other strong bright lines, undergo a change in breadth and intensity which must probably be regarded as real, since it can hardly be ascribed to atmospheric conditions or length of exposure. Whether the change is periodic or entirely irregular we have not yet been able to determine; but at any rate, as mentioned above, it stands in no immediate relation to the light phase. That the dark lines also show large variation in breadth and intensity has already been mentioned; the breadth depends greatly upon the situation of the bright lines—whether they appear on both sides of the absorption lines or on one side only; but the length of exposure has undoubtedly in this case a great influence, which it is difficult to determine.

I give here a few measurements of the breadth of the Hζ line, in order to show the limits within which the change of breadth takes place.

1893		Breadth of absorption line r	Breadth of bright bar r		Remarks
April	8	0.13	0.24		The bright line lies on the red side of the dark one. On the other side of the absorption line is visible only a quite fine bright line there 0.03r to 0.04r in breadth
April	17	0.15	0.45	{0.15 {0.15	The absorption line lies in the middle
April	23	0.11	0.20		The bright line lies on the red side
Oct.	13	0.10	0.15		The bright line lies on the violet side
Nov.	10	0.13	0.51	{0.19 {0.19	The absorption line lies in the middle of the bright one.
Nov.	21	0.12	0.33	{0.15 {0.06	The absorption line within the bright one, but displaced from the middle toward the red
Dec.	23	0.11	0.20		The bright line lies on the red side

The measures are given in micrometer revolutions; $0.1r$ corresponds to a change of wave-length of $0.16\mu\mu$.

It is a matter of special interest to determine the amount of the relative displacement of the lines, since this displacement can hardly be otherwise regarded than as resulting from the motion of different bodies, having dissimilar spectra which are superposed on account of the small distance between the bodies. Unfortunately it has not yet been possible to make absolute determinations of the positions of the lines with sufficient accuracy; but I hope that further observations can be made complete in this direction also.

The table on page 366 shows that at the time of greatest displacement the distance between the centers of the bright and dark lines of a pair is in the average 0.2 rev.; this would correspond to the very considerable motion of about 300 kilometers per second; but a more critical consideration of the phenomena and comparisons of the different lines with one another, show that the determination of a velocity by simple measurement of the distance between the bright and dark lines cannot be regarded as reliable since the bright lines undoubtedly partially overlap the dark ones, even at the time of their greatest separation. The extent to which this partial overlapping influences the two lines depends upon the breadth and intensity of the bright line, the form of its intensity-curve, and also upon the intensity-curve of the dark line. The intensity of the continuous spectrum, which varies with the light phase of the star, also plays a considerable part in this connection, and explains some minor differences which are found in the different lines of one and the same spectrum.

It has sometimes been obsrved that the broad absorption line $H\gamma$ completely encloses the bright line, so that the absorption line appears on each side of bright one, with the two parts of

greatly unequal width, while the bright Hζ line lies entirely on one side of its dark companion. At the times of the second minimum or second maximum it happens that the absorption line Hγ is so completely covered by the bright line that it is hardly distinguishable from the continuous spectrum. If, however, the bright line is at this time broader than the absorption line, narrow bright lines are produced at the borders of the hidden absorbtion lines as in the case of Hζ. It will be seen from this that the spectrograms require a very close and critical study if erroneous conclusions are to be avoided, which may easily be drawn from appearances that seem to differ for different pairs of lines, but are really explicable on simple principles, and referable to the same special case.

The case first mentioned, where at the time of greatest displacement the bright line lies within the absorption line, and which was several times observed in Hγ (Fig. 5) and in the line λ 477$\mu\mu$ (Fig. 6)*, affords the possibility of obtaining a fairly close approximation to the magnitude of the displacement of the lines and consequently to the amount of motion. I give below a number of measurements which I have made on two plates taken on December 22, 1893. The readings given are means of settings on the edges of the lines or on their centers.

Line Hγ.

0.1 rev. corresponds to a difference in wave-length of 0.26$\mu\mu$.

0.1$\mu\mu$ corresponds to a velocity of 69 kilometres.

Plate I.

Absorption line {0.747r, 1.227

Line λ 447.

0.1 rev. corresponds to a difference in wave-length of 0.28$\mu\mu$.

0.1$\mu\mu$ corresponds to a velocity of 67 kilometres.

Plate I.

Absorp. line {0.179r, 0.719 Distance

[redacted]

Plate II.

$$\begin{array}{ll} \text{Absorp. line} \begin{cases} 0.198r \\ 0.765 \end{cases} & \text{Distance} \\ \text{Bright line} \begin{cases} 0.411 \\ 0.739 \end{cases} & = 0.093r \end{array}$$

The mean gives for Hγ a displacement $= 0.083r$, corresponding to $0.216\mu\mu$ or a velocity of 149 kilometres; for the line λ 447 the displacement is $0.075r$, corresponding to $0.210\mu\mu$, or a velocity of 140 kilometres.

(TO BE CONTINUED).

ON THE INFLUENCE OF THE SLIT-WIDTH ON THE APPEARANCE OF COMET SPECTRA.*

H. KAYSER.

It was quite early assumed that the spectra of comets consist essentially of bands which are seen when compounds of carbon are burned or rendered incandescent by electric discharges, notwithstanding the fact that considerable differences were indicated by the observations. The most important of these differences are the following:

(1). Each of the groups of bands in the carbon spectrum (for it is now quite certain that the spectrum of carbon itself is concerned, and not that of a hydrocarbon), begins with a maximum of brightness on the red side; in cometary spectra, on the other hand, the maximum is within the band, and therefore displaced toward the violet.

(2). The wave-lengths of the maxima in cometary spectra are always found to be smaller than the wave-lengths of the heads of the carbon bands; the wave-lengths of the edges of the cometary bands are likewise smaller than the edges of the carbon bands; only in the case of the brighter cometary band at λ 516 is the wave-length of the beginning frequently found to be greater.

(3). Whenever a dispersion could be employed sufficiently great to show the second or third maximum of the bands in the comet spectrum, as well as the first, these maxima were also displaced toward the violet.

(4). While in each carbon band the first maximum is the brightest, and the following maxima become gradually fainter, in comets it has frequently been observed that the second maximum is brighter than the first.

* Translated from A. N. 3217.

(5). A series of observations was made by Harkness on Comet Encke 1871, V, according to which the wave-lengths of the maxima were greater, the greater the brightness of the comet.

(6). Measures by different observers on the same comet exhibit extraordinary differences, which often far exceed the possible limits of error.

All these anomalies in cometary spectra have remained unexplained up to the present time, although most of them have frequently been pointed out, and (as for example in the excellent collection of measurements by Hasselberg*) placed clearly before the eyes of observers. Only two attempts have been made to explain at least some of these anomalies. H. C. Vogel observed the mixed spectra of carbon and carbon monoxide. The CO band at λ 5609 then fell within the beginning of the C band at λ 5634, apparently producing a maximum of the C band at the former place. Further, the CO band at λ 5198 fell before the third C band at λ 5164, and thereby produced an apparent displacement of the beginning of the C band toward the red. Finally, a weak CO band at λ 4698 fell within the fourth C band at λ 4737, causing an apparent displacement of the maximum of the C band toward the violet. Vogel therefore makes the assumption that the spectrum of a comet often consists of the superposed spectra of C and CO, and in this manner seeks to explain the displacement of the maxima.

This hypothesis appears at first sight to accord quite well with the facts of observation, nevertheless it seems to me inadmisible for two reasons:

(1). If some of the CO bands appear in comet-spectra, then since one of the weaker bands is supposed to be strong enough to answer the requirements of the explanation, the other bands, at least the strongest, as for example those at λ 6079, 4834 and 4510 should be seen. This is however not the case.

(2). One would at least expect that the different measures would show some correspondence with the appearance of the bands in the mixed spectrum; for instance, that the apparent maximum of the third C band would always be found near λ 5609. But there is absolutely no hint of this in the observation: thus three measures by Vogel himself give for comet Encke 1871 V, λ 5552, for 1871 IV; λ 5571, for 1874 III: λ 5538.

The second attempt to furnish an explanation is due to Hasselberg. He has found that when very weak electrical discharges

* Mém. de l' Acad. de St. Petersb. Ser. VII. Bd. 28, No. 2.

are sent through carburetted hydrogen in Geissler tubes, the second maximum of the fourth carbon band (and perhaps also that of the second) can under certain circumstances become brighter than the first maximum. But even when it is assumed that this experiment is quite free from objection, and that the appearance is perhaps not due to CO, as in the case of Vogel's experiments, (and Hasselberg says himself that the CO spectrum was present at the same time), it explains only one of the anomalies which have been observed in the spectra of comets.

It must be said, therefore, that the measures of the spectra of comets have as yet found no explanation. Hasselberg constructs a "type of cometary spectra" which differs from the carbon spectrum, and Scheiner even says (*Spectralanalyse der Gestirne*, p. 227,) that the distance of the maximum from the edge of the band is perhaps really variable in cometary spectra, and for this reason recommends that the edge of the band and not the maximum, should be measured.

I believe, however, that such an assumption, which is in contradiction to all the fundamental principles of spectrum analysis, is unnecessary, and that all these anomalies admit an extremely simple explanation. It is, very probably, exactly this great simplicity that has caused this explanation to be so long overlooked. All the observed phenomena follow from this circumstance; that the comet spectrum is extremely faint, and hence the observer is compelled to use a low dispersion and a wide slit. He will always make the slit as narrow as possible, and will therefore choose different slit-widths, according to the birghtness of the comet and the character of his instrument. *This varying slit-width is the cause of all the observed anomalies,* as I shall proceed to show.*

In every spectrum a spectral line is an image of the slit. If, as is customary in spectrometers, the lenses of the collimator and the observing telescope have the same focal length the breadth

* In the work by Scheiner, mentioned above, are a number of remarks on the influence of the slit-width which are not clear to me. Schiener says (page 226), "With a widely opened slit the sharp edge of such a band is of uniform intensity within the breadth of the image of the slit, and the observer, in setting on the edge of the band, has therefore a tendency to select, not the edge itself, but the middle of the uniform brightness, by which an error in setting of half the slit-width toward the violet is caused." As I will show, the breadth of the image of the slit is not of uniform brightness and all that follows rests exactly on this point. The second half of the sentence I do not understand. If the image of the edge were actually of "uniform brightness," the setting on its center would give a measurement entirely correct; provided only that the standard lines, for determining the wave-lengths from the measures, were measured with the same slit-width, and that the middle of the image was also taken in their case. But this requirement is so obvious that Scheiner could not assume that it was neglected by the observer.

of the line is the same as that of the slit, (widened lines being of course left out of consideration) In what follows it will be assumed for the sake of simplicity that this is the case * The breadth of the line therefore increases with the width of the slit This is no detriment to measurement in a line spectrum, if only the settings are made on the same parts of all the broad images—center, edge, or any other part the observer chooses.

But the case is quite different when many lines lie close together, as in bands or flutings; by the superposition of the broad images the image of a single line disappears; we obtain in the spectrum only an increasing and diminishing intensity, which at any place represents the sum of the lines that fall there. In order to obtain conveniently a clear view of the effect produced, let us imagine the whole spectrum, as projected with a narrow slit, (and for the sake of brevity I will call this the true spectrum, that given by a wide slit the apparent spectrum), divided into narrow strips, each of which we may regard as having a constant mean intensity. The breadth of each strip we may take to be, say, 2 Angstrom units Now let the slit-width be equal to three such strips; then on every strip of the apparent spectrum fall the images of three strips of the true spectrum, and we obtain the intensity of a strip of the apparent spectrum by taking in the true one the sum of its intensity and that of the two neighboring strips.

Similiar considerations apply to every slit-width, for which the apparent spectrum may be found thus: consider a slit of the chosen width to be pushed over the true spectrum successively from strip to strip; for each position take the sum of the intensities of all the strips covered by the slit, and the result is the intensity in the apparent spectrum of the strip which occupies the middle of the slit.

It is now very easy to understand the conditions if we assume that we are dealing with a band, which has only one sharp edge, on (say) the red side, and which falls off gradually toward the violet We allow the slit *S*, whose breadth is equal to *n* strips, to pass over the true spectrum, and draw the apparent spectrum in accordance with the foregoing principles. We obtain an intensity for the first time when the advancing edge of the slit has passed over the first stip of the true spectrum, and the intensity

* In reality the observing telescope naturally does not come into consideration but only the angle subtended by the edges of the slit as seen from the center of the collimator objective The focal length of the telescope is a matter of indifference, since the dispersion and the breadth of the slit-image vary together thus with any focal length the same number of Angstrom's units are included in the image. It is the number alone which is concerned.

is that of this strip. Thus the beginning of the apparent band is displaced toward the red by ½ S. As the slit passes on, two, three, etc., strips of the true spectrum gradually fall within its limits, and the intensity of the apparent band thus gradually increases; but continually at a slower rate, since the strips which are added are always of diminishing intensity. As soon as the slit lies for the first time in the true band, the highest possible maximum of intensity is reached, for any further motion brings in strips on the violet side which are weaker than those which pass out on the red.

Thus we find that our apparent band begins ½S further toward the red, gradually increases, reaches a maximum which is displaced ½S toward the violet, as compared with the edge (and maximum) of the real band, then gradually falls off.

Very much more complicated are the relations when, as in the case of the carbon bands, we have to deal not with a single band but with a group of bands, having several edges, the intervals between which diminish in passing from red toward the violet and are filled with light from the gradually weakening bands. As long as the slit is still narrower than the average distance between the edges, the foregoing considerations hold; the beginning of the whole group of bands is displaced ½S toward the red, and every maximum ½S toward the violet. But if we take the slit wider—as wide say, as the distance between the first two edges—and again imagine S to be entering the true band, then the intensity of the apparent band will begin to increase from zero, and will reach its first maximum when the advancing edge of S exactly touches the second edge. If S moves further toward the violet, the first, second, . . . strips of the first partial band will drop out, but the first, second, . . . strips of the second partial band will come in. If now, as actually is the case in the carbon bands, the second, and likewise the third partial band are only slightly weaker than the first, the motion of the slit will bring in strips on the violet side of nearly the same intensity as those which leave it on the read; *i. e.*, the intensity of the apparent spectrum over a certain distance will diminish very gradually. Since the distance of the third edge from the second is less than that of the second from the first, the intensity will increase rapidly, on continuing the motion of the slit, as soon as the advancing side reaches the third edge of the band; for the strips which leave the slit on the red side are last and faintest of the first partial band, while those which enter on the violet side are the first and strongest of the third partial band. The second

maximum of the apparent band is reached when the following side of the slit reaches the second edge, and this maximum is greater than the first since two maxima of the true band contribute to its intensity. In like manner further maxima are obtained whenever the following side of the slit reaches an edge of the true band.

The result is then this: that we see a group of bands with several edges, but which has the following peculiarities:

(1). The band is also diffuse on the side toward the red;

(2). The edges are displaced ½S toward the violet;

(3). The first maximum is less insense than the maxima which follow it.

If the slit is allowed to become still wider, the following conditions still obtain:

(1). The beginning of the entire band is always displaced ½S toward the red;

(2). The maxima are always displaced ½S toward the violet.

But these maxima gradually become less pronounced as the slit widens, since in the great number of strips which are to be summed up, the effect of any single strip is less. No general statements can be made with regard to the relative intensities of the different maxima. The anomaly of the apparent spectrum, that the following maxima are greater than the first may exist, or it may be caused to vanish, according to the assumptions which are made as to the rate of diminution of intensity in the edges of the bands in the true spectrum. In any case it is less pronounced.

Since now the distances between the edges of the partial bands are different in the different carbon groups of the cometary spectrum, and since above all the dispersion in the prismatic spectrum

choose the fourth group of bands, whose edges are, in round numbers, at λ 4737, 4715, 4697, 4685, and 4677. I shall consider the spectrum to be drawn on the normal scale; the ordinates represent intensities, and the spectrum is divided into strips each two Angstrom's units wide. The mean intensity of each strip must now be estimated, a task which it is difficult to perform with accuracy, but which I have undertaken to perform as well as possible, with the aid of photographs, many of which were taken by myself and some by others. I shall represent the intensity of the first edge by 20, and, on this scale, estimate the intensities of the following edges to be 19, 18, 16, and 14. In the following table is given the intensity of each strip into which the spectrum is divided. The strips are numbered by calling the strip corresponding to the first edge 0, those following in the direction of the violet 1, 2, ...; and those toward the red $-1, -2$, ... The table shows then the distribution of intensity, (1) with a narrow slit; (2) with a slit whose width is five strips = 10 Angstrom's units; (3) eleven strips = 22 A. U.; (4) fifteen strips = 30 A, U.; (5) nineteen strips = 38 A. U.; (6) 35 strips = 70 A. U. In each case the maxima are made conspicious by heavy-faced type.

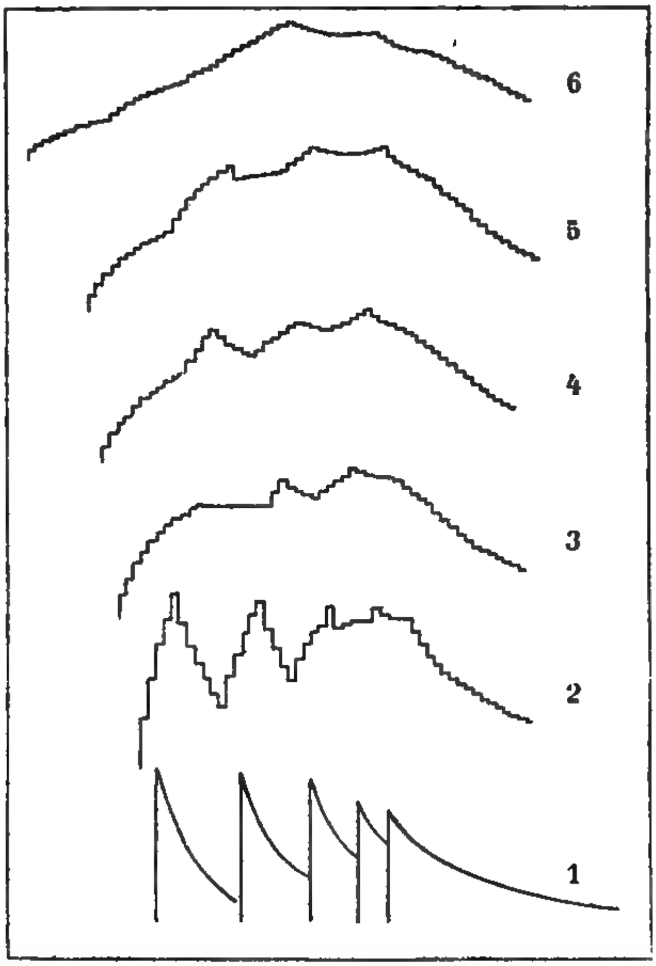
In the accompanying figure these intensities are shown for the six different cases; but the ordinates of 2 are reduced to $\frac{1}{3}$, those of 3 to $\frac{1}{7}$, those of 4 to $\frac{1}{8}$, those of 5 to $\frac{1}{10}$, and those of 6 to $\frac{1}{20}$, in order to make the figure sufficiently small. Thus the curves give the aspect of the group of bands as seen with their corresponding slit-widths.

The table and the drawing show with a slit-width of 10 Angström's units all five maxima, but each is displaced 5 A. U. towards theviolet. The slit-width of 22 A. U. shows only the first three maxima each displaced 11 A. U. in the same direction; but the second maximum is 21 per cent brighter than the first, and the third 34 per cent brighter. The slit-width of 30 A. U. also gives three maxima each displaced 15 A. U., but the second is only 4 per cent brighter than the first and the third 14 per cent brighter. With a slit-width of 38 A. U. thesecond maximum is the highest of the three which are present, but it is only a little higher than the first and third. These last however are so little elevated above their surroundings that they would hardly be visible, and it is probable that only the second maximum would be measured, thus showing a displacement of 40 A. U. with a slit only 38 A. U. wide. The last spectrum, finally, shows only one maximum, displaced by half the slit width; of the second there is present only an indication, which would be practically invisible.

Number of the Strip.	[1] Narrow slit.	[2] Slit=10 A.U.	[3] Slit = 22 A.U.	[4] Slit = 30 A.U.	[5] Slit = 38 A.U.	[6] Slit=70 A.U.
− 10	0	0	0	0	0	91
− 9	0	0	0	0	20	96
− 8	0	0	0	0	36	100
− 7	0	0	0	20	49	103
− 6	0	0	0	36	60	122
− 5	0	0	20	49	70	137
− 4	0	0	36	60	78	150
− 3	0	0	49	70	85	161
− 2	0	20	60	78	91	170
− 1	0	36	70	85	96	178
0	**20**	49	78	91	100	185
1	16	60	85	96	103	191
2	13	**70**	91	100	122	196
3	11	58	96	103	137	204
4	10	49	100	122	150	218
5	8	42	**103**	137	161	230
6	7	36	102	150	170	241
7	6	30	101	**161**	178	251
8	5	35	101	150	185	260
9	4	37	101	142	**191**	276
10	3	46	100	136	176	289
11	**19**	54	100	131	178	300
12	15	61	100	126	179	310
13	13	**67**	100	136	180	324
14	11	56	100	143	181	337
15	9	48	114	149	183	345
16	8	41	**125**	155	185	356
17	7	35	118	161	195	**366**
18	6	44	114	**167**	203	355
19	5	50	111	164	210	347
20	**18**	55	109	162	**217**	342
21	14	60	116	160	212	338
22	12	**65**	121	159	210	335
23	11	56	125	164	208	333
24	10	58	129	169	208	332
25	9	59	**138**	174	209	332
26	**16**	59	133	179	210	333
27	13	59	131	**184**	211	335

[redacted]

The spectrum of a comet will differ from the appearances described above, in that the beginning of the band will not seem to be displaced ½ *S* towards the red. As we have seen, the beginning of the apparent band has the same brightness as the true edge of the band with a narrow slit. The slit is however made wide, merely because this brightness is not sufficient; it follows therefore that the beginning of the apparent band is also invisible,



and with the same slit-width, the place at which the band seems to begin will depend only upon the brightness of the comet, the character of the instrument, and the weather. There is no object, therefore, in measuring this place of beginning unless it be for the

purpose of estimating intensities. On the other hand, the position of the apparent maximum depends only upon the apparatus, and from it the wave-lengths of the true edges can be computed when the dispersion and slit-width are known.

It seems to be hardly necessary that I should make a detailed application of the above discussion to the case of cometary spectra. According to the light-gathering power of the telescope and the dispersion used, observers have chosen the width of the slit, and have thus measured quite differently the position of the maxima; it would seem that they have usually been obliged to employ a slit-width corresponding to the fifth or sixth example above, in which each group of bands shows only one maximum. If the comet became brighter, the slit was gradually made narrower, causing a progression of all the maxima toward the red—the observation of Harkness. If the slit could be made so narrow that several maxima became visible, that width might accidentally be hit upon which would make the following maxima weaker than the first. It will be seen that all the observations are explained naturally; and thus a hitherto somewhat unpleasant chapter in the subject of cometary spectra is elucidated in the most beautiful manner.

It only remains that I should make a numerical comparison of my explanation with the results of observation. This is only in a very limited degree possible, since I have found no data regarding the slit-width and dispersion of the instruments which have been employed by observers. Several points may, however, be brought out.

With a strongly dispersive flint glass prism of 60°, having a collimator and a telescope of 25 centimetres focus, I have measured the dispersion at the places of the three principal carbon bands. I found that at λ 563 in the spectrum 230 A. U. fall on one millimetre, at λ 516, 160 A. U., and at λ 437, 110 A. U. Although different prisms, and particularly those with direct vision, may have sensibly different relations in this respect, the numbers given above nevertheless supply us with a certain foothold, and we may expect that the observed displacements of the maxima in the three bands will bear the relation to each other of about 230 : 160 : 110, or in round numbers 4 : 3 : 2. Hasselberg has computed the mean values from all observations up to 1880, and finds for the maxima: 5564, 5127, 4705, and these compared with the true values 5634, 5164, 4737 give the differences 70, 37, 32. It seems to me that when the uncertainties of measurement are considered, this is not a bad result. The middle band only

gives a displacement which is too small, as it should be about 50. The result that the middle band is often less incorrectly measured than the others, is even more evident when the separate measurements are regarded; those collected by Hasselberg as well as those which have been made later. From them it would almost appear as if some of the observers had measured the middle band with a narrower slit; this would be explained by the greater brightness of the middle band, allowing the use of a narrower slit. According to numerous observations by Vogel, Lindsay, Tacchini, v. Konkoly, v. Gothard, the relative intensities of the three bands (somewhat different in different comets) are about as 4 : 10 : 3. When, for example, in the comet 1881 III, Harkness finds the displacements 141, 40, 65, Vogel, on the other hand, 94, 74, 39, this seems to me to indicate that Harkness measured the middle band with a narrower slit, and Vogel, more correctly, with a slit of the same width as that employed for the other two bands.

The measurements of the visible beginnings of the bands also agree quite well. The beginning of the middle and brightest band is often seen to have a greater wave-length than that of the true beginning. Hasselberg's mean value makes the beginning of this band 4 A. U. too great, that of the two other and much fainter bands 10 and 8 A. U. too small.

The amount of the observed displacement leads also to not improbable slit widths. The displacement of the first band is seldom as much as 100 A. N. With my apparatus this would correspond to a slit-width of 0.8 mm., but with a smaller dispersion, such as no doubt has generally been employed in observations of comets, to a slit much narrower than this. The displacement is in general, however, materially smaller, so that it would correspond to a slit width of 0.1 to 0.3 mm. In those cases where several maxima were seen, the displacement is small since a moderately narrow slit is essential in such an observation.

A more definite proof of my explanation would be possible if observers of comet spectra would publish the data regarding the dispersion of their instruments and the slit-width employed by them, as well as any changes which were perhaps made. But so far as one can judge without this information, I find no contradiction between my conclusions and experience. In the mean time the best confirmation seems to me to lie in this,—that in recent measurements, whose heightened accuracy we owe in part to larger telescopes, but particularly to photography, all these anomalies disappear, and the cometary spectrum appears as a true carbon

spectrum, with bands of exactly the same position and structure as those which we observe in our laboratories.* Without the explanation of the origin of these anomalies, no proof would however yet have been brought forward that the earlier comets also gave the ordinary carbon spectrum.

In closing I wish to mention that the appearences which I have described can be observed without difficulty in the laboratory. The experiment is particularly beautiful when a spectrometer is used with a Kruss double slit, the jaws of which widen symmetrically. If the carbon arc is projected on the double slit, and one of the slits is opened, it will be seen how isolated lines, for instance those of calcium and iron, which are always visible in the carbon arc, gradually widen, but have their centres always in the same position with reference to the narrow lines generated by the second narrow slit. The edges of the carbon bands, on the contrary, not only widen, but the maxima travel in the direction of the shorter wave-lengths. It is also easy to choose the breadth so that the first maximum is weaker than the second.

THE THERMAL RADIATION FROM SUNSPOTS.‡

W. B. WILSON.

These observations were made by means of a large heliostat, lent by the Royal Society, and a Boys's radio-micrometer. The heliostat consists of a plane silver-on-glass mirror of 15 in. aperture. It is mounted equatorially, and driven by a clock. When in use, it is adjusted to reflect the sunlight to the north pole, and, as long as the driving clock is kept in motion, the beam of light remains fixed in that position. In the track of this beam, and about 12 ft. from the plane mirror, is mounted a concave silver-on-glass mirror of 9 in aperture, and about 13 ft. focus. Its axis points to the south pole, so that the cone of rays formed by it strikes the centre of the plane mirror, and a short distance inside the focus. A small plane mirror mounted on the end of an arm is then so placed as to intercept the cone of rays, and reflect it hori-

* The excellent measures of Campbell which I recently noticed (A. N. 3214, ASTRONOMY AND ASTRO-PHYSICS, March, 1894) also show that all the edges of the bands have a wave-lengtn 1 or 2 A. N. too small. Campbell himself directs attention to this, but he does not know the reason for it. I conjecture that his slit width must have corresponded to about 3 Angstrom's units.

† Preliminary Notes of Observations made at Daramona, Streete, Co., Westmeath, 1893.

‡ Read before the Royal Socity, London. Communicated by the author.

zontally into the Observatory window; an achromatic lens enlarges the solar image which is formed on a screen in the room to 4 ft. in diameter.

Behind this screen, and standing on a pier of concrete, is mounted the radio-micrometer. The aperture through which radiant heat reaches the sensitive thermo-couple is a round hole drilled through a thick sheet of brass, and is only 1 mm. in diameter. A white card-board screen is placed in front of the brass one to cut off heat from falling on the latter, and is provided with a hole slightly larger. A beam of lime light is thrown on the mirror of the radio-micrometer, and reflected on to the scale in the usual way. The diagonal mirror of the heliostat is provided with slow motions in two directions, which are moved by long rods and hook joints inside the observatory. Thus any part of the Sun's disc can be placed on the small aperture of the radio-micrometer, and the driving clock will then keep it there.

The observations are taken in the following manner. A small screen is placed over the aperture of the radio-micrometer, and the zero position of the spot of light on the scale noted. The screen is then removed, and the umbra of a Sun spot placed on the aperture. The reading is then taken and entered in column u. The image is then moved, so that a part in the neighborhood of the spot, but at *the same distance from the centre of the solar disc*, is placed on the aperture. This reading is entered in column N. Finally, a reading is taken at the centre of the disc, and entered in column C. The throws of the instrument are then got by subtracting the figures in columns u, N, and C from the zero. The deflections of the instrument have been experimentally proved to be *strictly proportional* to the amount of radiant heat falling on the thermo-couple. The following is a typical observation taken August 7, 1893, of a large Sun spot then visible. The *umbra* of this spot measured 0.8 in. across on the screen, so that the aperture of the radio-micrometer was only covering about $\frac{1}{160}$ of the area of the umbra.

Zero.	u	N.	$u - z$	$N - z$
15.8	17.1	20.4	1.3	4.6
15.6	16.9	20.2	1.3	4.6
15.5	16.8	19.9	1.3	4.4
15.3	16.7	19.8	1.4	4.5
15.2	16.6	19.6	1.4	4.4
15.1	16.4	19.5	1.3	4.4
14.9	16.1	19.4	1.2	4.5
		Means	1.31	4.49

The ratio $\frac{\text{umbra of spot}}{\text{neighboring photosphere}} = \frac{1.31}{4.49} = 0.292.$

Five concordant readings gave a mean deflection of 4.57 for the centre of the Sun, which gives for the ratio $\frac{\text{umbra}}{\text{centre}} = 0.287.$

This spot was at a distance from the centre of the disc of about 0.4.

As the radiation from the photosphere falls off from the centre to the edge of the disc, it seemed an interesting point to determine if any change in the ratio of u C would take place as a spot was carried across the disc by the Sun's rotation. If the spot is, as is generally thought, a depression, the absorption of heat ought to increase as it is carried towards the limb, on account of the increased depth in the solar atmosphere through which the radiation would have to pass. On the other hand, if the spot was floating *above* the absorbing atmosphere the radiation from it would remain constant in any position on the solar disc.

The following is the value of the heat radiation from the photosphere taken along a radius of the Sun, where 0 = centre and 100 the limb. The radiation R equals 100 at the centre.*

D.	R.	D.	R.
0	100.0	60	92.2
10	99.8	70	87.8
20	99.5	75	85.3
25	99.3	80	82.5
30	98.9	90	72.0
40	97.2	95	61.3
50	95.3	98	51.5
		100	42.9

It will be seen by the following observations of spots, taken from August 5 to November 9, that there is distinct evidence that the radiation from the spot does not fall off as rapidly when near the limb as the neighboring photosphere; in fact the ratio u C remains nearly constant, whereas the ratio u N gets nearer unity as the spot approaches the limb. The spot observed on the 22d of October is a good example, as the same spot was observed again on the 26th, 29th, and on the 30th, when it had reached within a distance, D, of 95 from the center. It will be seen that on these four dates the ratio u C was respectively 0.338, 0.360, 0.313, 0.356, whereas the ratio u N was 0.349, 0.410, 0.706, 0.783.

* "The Absorption of Heat in the Solar Atmosphere," by W. E. Wilson and A. A. Rambaut, 'Proceedings of the Royal Irish Academy,' 3rd series, vol. 2, No. 2 Monthly Notices, vol. 37, No. 1.

	Date.		$\frac{u}{C}$	$\frac{u}{D}$	D
1893.	Aug.	5............	0.370	0.427	60
		7............	0.287	0.292	40
		8............	0.286	0.323	50
		8............	0.339	0.377	40
		8............	0.418	0.512	90
		14............	0.364	0.373	50
		19............	0.368	0.375	50
	Sept.	2............	0.309	0.309	10
		3............	0.298	0.298	10
		4............	0.420	0.450	30
		4............	0.430	0.446	30
		7............	0.287	0.355	85
	Oct.	1............	0.398	0.401	30
		1............	0.489	0.570	80
		22............	0.338	0.349	52
		26............	0.360	0.410	40
		29............	0.313	0.706	90
		30............	0.356	0.783	95
	Nov.	8............	0.365	0.800	97
		9............	0.330	0.848	85

Langley,† in 1874 and 1875, measured the radiation from the sun spots. He used a thermo-pile and galvanometer, and obtained as the mean of his results a ratio of 0·54 ± 0·05.

His method was first to take a reading in the neighbourhood of the spot, but between it and the centre of the disc. He then took a reading in the umbra, and, finally, a third reading in the neighbourhood between the spot and the edge of the sun.

The mean of the two photospheric readings he used as a divisor for the umbral reading. He then says, "The decrement of heat as we approach the limb is, though not exactly, yet so very nearly, in the same ratio for photosphere and spots, that no correction is needed on this account for the present observations."

If Langley failed, through want of instrumental means, to notice the difference between the absorption in a spot and the photosphere near the limb, his method would make his umbral readings too high. The mean of twenty observations here equals 0.356, against Langley's 0.54. This is a serious difference, and, I think, can only be accounted for either by the use of superior instrumental means, or by a possible variation in the radiation of spots in different years of the Sun-spot cycle.

It is difficult to see how *too low* a value for umbral radiation could be got, whereas too high a one might be found by want of definition and trembling in the image, so that some of the penumbral radiation would reach the thermo-couple.

EXPERIMENTAL INVESTIGATIONS ON THE EFFECTIVE TEMPERATURE OF THE SUN, MADE AT DARAMONA, STREETE, CO. WESTMEATH.*

W. E. WILSON, M. R. I. A., AND P. L. GRAY, B. Sc.

The only tolerably complete series of investigations on this subject up to the present time have been those of Rossetti and Le Châtelier. The results given by other writers have depended more or less on guesses relative to the law connecting radiation and temperature, and differences on this point alone have given values varying between 1500° and 3,000,000° to 5,000,000° C.

Rossetti worked with a thermo-pile exposed directly to the heat of the Sun; the law connecting the deflections of the galvanometer with the temperature of an artificial source of heat having been obtained up to a temperature of *about* 2000° C., from the deflection produced by the heat of the Sun the solar temperature was calculated by extra-polation.

Le Châtelier worked on an entirely different principle, measuring the intensity of the light transmitted through a certain piece of red glass, first from sources at known temperatures up to 1700° or 1800°, and, secondly, from the Sun, the temperature of which was then obtained, as in Rossetti's case, by a process of extra-polation, which is, of course, necessary in any method, until we can raise substances to a temperature actually as high as that of the Sun, an experiment at present impossible.

Rossetti obtained finally a temperature of 10,000° C., approximately, while Le Châtelier gives 7600° (± 1000°) as the mean of his results. In the paper the difference between Rossetti's result and our own (6200° C.) is discussed, and a possible explanation given.

The method adopted by the authors is a zero method, and the essential point is the *balancing* of the heat from the Sun with that from a platinum strip heated to a high known temperature.

The artificial source of heat was a modified form of Joly's meldometer, the calibration of which can be performed with a very high degree of accuracy. The "radiation balance" is a form of Boys's radio-micrometer containing a duplex circuit, so designed that the heat from the Sun can be made to exert a turning moment in the opposite direction to that due to the artificial source of heat, and by making the apparent area of the latter sufficient-

* Abstract of paper read before the Royal Society, London. Communicated by the authors.

ly great, the radiation from it may be increased so far as to equal that arriving at the radio-micrometer from the Sun.

The following points are considered, after descriptions of the method and apparatus have been given:

1. The law connecting radiation and temperature.

   This is probably the most important factor in the value of the final result. Numerous investigations on the point have been made, which are referred to in the paper.

   After a careful series of experiments we have come to the conclusion that (at least for bright platinum) Stefan's "law of the 4th power" holds, *i. e.*, that for high temperatures (say over 600° or 700° C.) if R = the radiation from a source whose absolute temperature is T, then

   $$R \propto T^4,$$

   a result not wanting confirmation both experimental and theoretical.

2. The emissive power of platinum at high temperatures compared with that of lamp-black.

   On this point the value obtained by Rossetti was used, some considerations being given in support of his figures.

3. The amount of the atmospheric absorption.

   This is fully discussed, and again the value obtained by Rossetti is used.

   Langley's theoretical value for percentage absorption of radiation from a body in the zenith, *viz.*, 41 per cent, is shown to be possibly too great; Rossetti obtained 29 per cent, which appears to be the value best supported by experiment.

   The climate of Ireland entirely prevents a systematic series of investigations on this particular point.

Several subsidiary questions are also discussed, and, finally, the results of about sixty-nine observations are given, which lead to a final mean result for the *effective* solar temperature of 6200° C.

It is pointed out, in conclusion, that the method would probably give excellent results if adopted in some country in or near the tropics, where atmospheric conditions can be trusted to remain more constant for some weeks, or even days, together, and where a series of observations taken at the same part of the year throughout the period of a sun-spot cycle might be hoped to settle the question of how (or if) the solar temperature varies during this time, as any error in the absolute value obtained may probably be considered constant, so that comparative values from year to year might be trusted to indicate any change.

SPECTRA OF THE GREAT NEBULA IN ORION AND OTHER WELL-KNOWN NEBULÆ.*

W. W. CAMPBELL.

A careful study of the new star in *Auriga*, made after its rediscovery in August, 1892, at *Lick* Observatory, led me to the conclusion that its spectrum was nebular. In order to establish that conclusion upon a perfectly firm basis, I investigated the spectrum of the *Nova* and of several well-known gaseous nebulæ as thoroughly as our instrumental means would permit. The observations of the nebulæ were made principally in September and October, 1892, and July, 1893, and lists of nearly all the bright lines observed were published in several journals,† without comment, simply for their bearing upon the questions relating to the new star's spectrum. The investigations were continued on several nights in Sept. and Oct. 1893, and on occasional nights during the past winter, for the information which might be gained concerning the nebular spectrum itself. All the observations of the nebulæ are brought together in the present paper, and discussed to a very limited extent.

Unless otherwise stated, the observations were made with the 36-inch equatorial and the large Brashear spectroscope, using a 21-inch collimator, a dense 60° prism, 10½-inch view telescope and magnifying power of 13 in visual work, and a 10½-inch camera in photographic work. It is unnecessary to say that this spectroscope is satisfactory and efficient when used visually. We shall now show that, theoretically, it is also very efficient in photographing bright-line spectra of large objects. The breadth and length of a monochromatic bright-lined image formed on the photographic plate are substantially independent of the dispersing medium used (prisms or gratings); and for any given width of slit, vary directly as the focal length of the camera lens. The intensity of the bright-line image upon the sensitive plate will therefore vary inversely as the square of the length of the camera. If the collimator and camera are of equal lengths, as is usually the case, the image of any bright-line *on the plate* and the image of the same bright line *in the slit* will be equal to each other in size and intensity,—neglecting loss by absorption, etc. But if the camera is only half the length of the collimator the *area* of the

* Communicated by the author.

† Especially in *Publication* A. S. P. for Dec., 1892, and Sept., 1893, ASTRONOMY AND ASTRO-PHYSICS for October, 1892, and October, 1893; and *Astronomische Nachrichten*, Nos. 3133 and 3189.

photographic image will be decreased four-fold, and its *intensity* increased four-fold. If the camera is only one-fourth the length of the collimator, the intensity will be increased sixteen-fold. The advantage of using a short camera and long collimator for recording faint lines is therefore very great. The principle applies effectively to the study of all large objects yielding bright-line spectra: comets,* large nebulæ, aurora borealis, etc. It is especially applicable to the photography of faint lines in the planetary nebulæ in case the focal-length of the telescope is long: since in that case the images of the nebulæ on the slit-plate are of considerable size, a wide slit can be used, and the reduced and intensified images on the photographic plate are still sufficiently large.

The scale of the photograph is reduced, it is true, by reducing the length of the camera; but the loss can be made up in part by using a denser prism, or entirely by using more prisms. I was able to use a very dense prism to advantage; for though it absorbed ultra-violet light considerably, chromatic aberration prevents me from photographing beyond $H\varepsilon$, except in very large objects.

It will be seen that a three hours' exposure with the spectroscope described above, using the 10½-inch camera, would, for recording faint bright lines in large objects, be equivalent to an exposure of at least 12 hours with the same telescope, and any spectroscope whose camera and collimator were of equal lengths.

The ratio of focal length to aperture is unusually large in the 36-inch equatorial, being 19 : 1. We may assume 15 : 1 as the average value of that ratio for refractors now in use. In the matter of the intensity of nebular images on the slit plates, the average refractor would have an advantage over the Lick refractor of

$$\frac{1}{(15)^2} : \frac{1}{(19)^2},$$

or as 1.62 : 1. If now the average refractor be equipped with a spectroscope whose collimator and camera are equal in length, and the Lick spectroscope's camera be only half as long as its collimater, the advantage of the Lick apparatus over the other in recording faint nebular lines, becomes as 2.49 : 1. In one photograph I used a camera only 5¼ inches long; *i. e.*, one fourth of the focal length of the collimater, which would have increased the ef-

* The use of the short camera in photographing the spectrum of *Comet b* 1893 enabled me to record 25 bright lines with an exposure of about an hour. If the collimator and camera had been of equal lengths, an equivalent exposure would have been at least four hours, which was not possible. I could have secured probably *not more* than six lines.

ficiency at least four-fold more; but the short-focus lens was almost wholly uncorrected and gave poor definition.

Practically the spectroscope is not so efficient. It was designed wholly for visual observations, and when used photographically the effects of flexure enter so seriously as to limit the completeness of the work in many directions. The shifting of the images during a long exposure prevents the accurate measurement of wave-lengths, spoils the definition, and to some extent prevents the recording of the faintest lines. Exposures on the same plate during two or more nights can not safely be made.

I have several times measured the wave-lengths of the principal visual line in the Orion Nebula, G. C. 4373, G. C. 4390 and N. G. C. 7027; but only for the purpose of testing the adjustments of the spectroscope. In no case, I believe, did the resulting velocities in the line of sight differ more than two miles per second from the splendid results obtained by Dr. Keeler in 1890. Further than this, I have made no measures of the nebulæ with high dispersion.

The Great Nebula in Orion.

Visual Observations.

The spectrum of this remarkable nebula has engaged the attention of a great many observers. A history of their work would be interesting and valuable: but it were better written by others; and lack of space here prevents all references to former observations, except those bearing upon what we may regard, up to the present at least, as unsettled problems.

The visual spectrum has been observed by Dr. Huggins and Professor Vogel for the purpose of determining whether the relative intensities of the bright lines vary in different parts of the nebula. Their results, slightly different, are best given by the following quotations, which form an essentially complete history of the subject.

1. "I have suspected that the relative brightness of this line ($H\beta$) varies slightly in different parts of this nebula. It may be estimated perhaps in the Nebula of Orion at about the brightness of the second line (496). The second line suffers in apparent brilliancy from its nearness to the brightest line (501), and may, without due regard to this circumstance, be estimated as brighter than the third ($H\beta$) line." Huggins, in *Phil. Transactions*, 1868, p. 545.

2. "An investigation of the different parts of the nebula gave the results that the three lines (501, 496, Hβ) were everywhere present and that their relative intensities remained always constant."—Vogel in *Astr. Nach.*, No. 1864, 1871, August.

3. "The brightness of these lines (Hβ and Hγ) relatively to the first (501) and second (496) lines varies considerably in different nebulæ; and I suspect that they may also vary in the same nebula at different times, and even in different parts of the same nebula; but at present I have not sufficient evidence on these points." —Huggins, in *Proc. Roy. Soc.*, 1872, May.

4. " . . . In the visible region there is no known alteration of the spectrum of the four bright lines, except, it may be, some small differences of relative brilliancy of the lines."—Huggins, in *Proc. Roy. Soc.*, vol. 46, 1889.

From the foregoing observations, astronomers have generally held the opinion recorded by Miss Clerke, that there is a "fundamental sameness of the visible spectrum of the nebula throughought its entire extent."—*Observatory*, 1889, p. 368; *System of the Stars*, p. 80.

My observations, made Oct. 17 and 18, 1893, lead to a very different conclusion; for I found that the relative intensities of the three lines at wave-lengths 5007, 4959, 4861, which constitute the principal part of the visible spectrum vary within wide limits as the slit of the spectroscope is moved over the different parts of the nebula. For the brightest parts of the nebula, in the vicinity of the trapezium, the relative intensities of these lines are approximately as 4 : 1 : 1. But many of the regionsof medium brightness give a spectrum in which the first and third lines are about equally intense; while for many of the faint portions, especially those on the south and west borders of the nebula, the third line is brighter than the first. The isolated portion northeast of the Trapezium surrounding the star Bond No. 734, yields a spectrum in which the third line is much stronger than the first; indeed for some parts of it, the third line is at least five times as intense as the first. It sometimes happens that of two adjacent portions of the nebula in the slit at the same time the first line is stronger than the third for one part, and the third line is stronger than the first for the other part.

The ratio of the intensities of the first and second lines appears to remain practically constant at 4 : 1. The second line is much fainter than the third in nearly all parts of the nebula. In general the Hβ hydrogen line is relatively very strong in the faint outlying regions. It is relatively stronger even in the bright region

around the Trapezium than in any other nebula I have examined except possibly the planetary nebula S. D. M. 12° 1172. As Dr. Huggins has pointed out, "the second line suffers in apparent brilliancy from its nearness to the brightest line." With low dispersion it seems considerably fainter than the $H\beta$ line, even in the vicinity of the trapezium. But when the very bright first line is covered with a heavy micrometer wire, the second line is seen to be fully as bright as the third. This point was further tested by using two gratings, in the first, second and third orders, with which the second and third lines become very faint and widely separated. By narrowing the slit until the lines were rendered almost invisible, the second line was seen with certainty to be a very little brighter than the third line: but of course this result holds true only for the densest parts of the nebula.

The ease with which these observations can be made is due in part to the fact that the image on the slit-plate is large.

The widths of the three brightest lines were carefully examined, as far as possible in all parts of the nebula, to see if any portion gave lines not truly monochromatic. A 60° prism and the first four orders of gratings were used. No variation in their breadths were detected. The lines everywhere appear to be truly monochromatic images of the slit.

The continuous spectrum can be seen in all the fairly bright parts of the nebula. It seems to be at least as strong, relatively, in the faint portions as in the bright ones. I obtained the impression that it is relatively strong in the faint nebulosity surrounding the star Bond No. 734.

In addition to the three prominent lines and the easily observable $H\gamma$ line, all of which were discovered by Dr. Huggins, the lines discovered by Copeland* at D_3 and λ 4476 are visible in the brightest part of the nebula. Two measures of the D_3 line in October, 1893, gave 5874 as its wave-length. I have not been able to see the lines observed by Taylor† at λ 5592, λ 5200, λ 4703.

On several nights in October, 1893, I carefully examined the spectra of the principal stars in the nebula for bright and dark lines. These stars are Struve's Trapezium stars A, B, C, D, and Bond's Nos. 685, 708, 734, 741. No bright lines were seen in any of them. Special care was taken in observing stars C and No. 685 (Flamsteed's θ_1 and θ_2), but I was unable to verify Espin's‡ 1890 observations that they contain bright lines. $H\beta$

* *Monthly Notices*, R. A. S., vol. 48, pp. 360-2.
† *Monthly Notices*, R. A. S., vol. 49, pp. 124-6.
‡ *Astronomische Nachrichten*, No. 2863.

was observed to be dark in No. 685, and probably also in star C. Hβ was observed to be dark in No. 734. No other dark lines were seen with certainty in any of the eight stars. The points of intersection of the stellar spectra with the bright nebular lines were especially examined for bright and dark lines. In every case it was judged that the stellar spectra were strictly continuous at λ 5007 and λ 4959, and contained dark Hβ lines, for the following reason: The nebular lines in the immediate vicinity of the Trapezium are neither brighter nor fainter, perceptibly, than they are just to either side of it; so that if we pass the slit *between* and *not including* those stars, the spectral lines furnish no clue that those stars are very near. The intersections of the stellar spectra with the bright lines λ 5007 and λ 4959 are certainly slightly brighter than the continuous spectra of the stars just to one side of the points of intersection, as would be the case if at the intersections the bright nebular lines and continuous star spectra are superposed. At the Hβ intersections there seemed to be little or no increase of brightness, as would be the case if the Hβ star lines are dark. But all visual observations of this kind may be more or less uncertain; and the photographic observations, referred to on a later page, are more decisive. The visual observations are made best with gratings.

Photographic Observations.

One very successful photograph of the spectrum was obtained on Oct. 11, 1892. The photographic field was terminated at λ 383 by the narrow camera tube. Sixteen prominent lines were recorded between λ 501 and λ 383, besides some very faint ones. A list of 18 of these lines was printed in A. AND A.-P. for October, 1893, pp. 723–4, with no discussion beyond the statement that the photograph presented almost no points of resemblance to Dr. and Mrs. Huggins' 1888 photograph of the same region. I further said that 12 of the 18 lines were "not previously observed." The number *twelve* is not correct; for I have since found that Lockyer* and Fowler obtained five spectoscopic negatives of this nebula on Feb. 2, 8, 9, 10, 11, 1890, with which my negative appears to be more or less identical. I now take pleasure in crediting them with the discovery of some of those twelve lines, and regret the temporary injustice done them. Likewise another one of the twelve, the hydrogen line λ 3836, was observed by Dr. and Mrs. Huggins,† whose photographs were made on March 14, 15,

* Lockyer's paper, read before the Royal Society on Feb. 13, 1890, is in *Proc. Roy. Soc.*, Vol. 48, pp. 199-201.

† Dr. and Mrs. Huggins' paper, communicated to the Royal Society on April 16, 1890, is in *Proc. Roy. Soc.*, Vol. 48, pp. 213-216.

17, 1890, appear to be substantially identical with those obtained by Lockyer and Fowler. Both these very important series of negatives are as yet incompletely described, and we cannot establish the identity of the two series with each other, nor with my photograph, though they all seem to agree in their general character.

These observations were made early in 1890, before I became specially interested in spectroscopic work, and probably too late to be included in the text or referred to in the literature-index of Scheiner's *Spectralanalyse der Gestirne* (issued late in 1890). I had searched all the technical astronomical journals issued subsequent to the season of 1890 for late observations without success. I regret having temporarily overlooked them.

I resumed photography of the spectrum in September, 1893, as soon as the nebula came into an accessible position. A list of the principal negatives follows.

1893 Sept 12. A three hours' exposure with a spectroscope* attached to the 12-inch equatorial showed about twenty bright lines between λ 501 and λ 372, and traces of a few others. A list of these lines will be given further on. The slit was about 0.42 inch long. It was placed east and west, across the trapezium. It included not only the dense central region of the nebula, but also some of the fainter portions. All the prominent hydrogen lines extend into the fainter regions apparently with *relative* intensities unchanged; while the intensities of the lines λ 501 and λ 496 fall off more rapidly, and the intensity of the line λ 373 less rapidly, than in the case of the hydrogen lines. The latter is due at least in part to chromatic aberration, and it may also be partly real; for Gothard† has found that the line 373 "is always very intense in the large irregular nebulæ, is always very faint in the true planetary nebulæ."

1893 Sept. 17. Another exposure with the 12-inch was made, but with the slit somewhat differently situated, for testing the relative intensities of the brightest lines. The results are the same as those obtained Sept. 12.

1893 Sept. 18. A three hours' exposure was made with the 36-inch and 10½-inch camera, with the slit placed upon the brightest region southwest of the Trapezium, but not including the

* This spectroscope was constructed for me by the Observatory carpenter, all but the slit and optical parts being of wood. It gave splendid definition, and for long exposure showed no signs of flexure. It weighs about 10 pounds. Next winter I hope to have more efficient optical parts similarly mounted for work on the same nebula.

† Astronomy and Astro-Physics, January, 1893, p. 55.

Trapezium, for the purpose of detecting any possible variations in brightness of the hydrogen lines. The photograph seems to be identical in every way with those of the Trapezium region, except as they are affected by chromatic aberration.

I made several comparatively short exposures, during September and October, on the different bright regions in the nebula, in order to see if I could confirm Dr. Huggins' earlier results showing remarkable absences of certain hydrogen lines. My photographs show no changes in the relative intensities of the hydrogen lines. $H\delta$ is always present, and not much fainter than $H\gamma$.

1893 Oct. 12. A four hours' exposure was made with the 36-inch, using a 5¼-inch camera with the 21-inch collimator. The slit included and preceded the Trapezium. The negative shows fully twenty-five bright lines, with traces of others, between λ 501 and λ 372. The cheap 5¼-inch lens employed is not large enough to include the whole beam of light, is poorly corrected around the edge, and necessarily gives poor definition. For that reason no further use was made of it.

1893 Nov. 10. An exposure on the isolated nebulosity surrounding star Bond No. 734 recorded the hydrogen lines $H\beta$, $H\gamma$, $H\delta$ and $H\varepsilon$ and no other bright lines; thus confirming the visual observation given above, of the great relative intensity of the hydrogen lines in this region.

All the long exposures on the condensed parts record the continuous spectrum of the nebula.

The wave-lengths of the bright lines shown with certainty on my principal negatives are tabulated in Table I. The relative intensities are assigned from the negatives taken with the 36-inch which on account of chromatic aberration, etc., are considerably different from those taken with the 12-inch, which in turn would be different from those obtained with a reflector.

Very little can be done in the way of assigning the chemical origin of these bright lines. Nine are due to hydrogen, but the sources of the others are unknown. The line at λ 5874 is probably D_3. The line at λ 4472 is possibly identical with a line always present in our Sun's chromosphere. The others can at present be classed only as nebular lines.

In reference to the *forms* of the bright lines shown on their 1890 (and also their 1888) photographs, Dr. and Mrs. Huggins write: " . . . it is significant that the hydrogen lines are sensibly stronger and broader on the plate as the Trapezium with its stars is approached. . . .

TABLE I.—BRIGHT LINES PHOTOGRAPHED IN THE ORION NEBULA.

1892. Oct. 11	1893. Sept. 12.	1893. Sept. 18	1893. Oct. 12.	1893. Nov. 10.	Description.
5007	5007	5007	5007		1st nebular line, very bright.
4959	4959	4959	4959		2d " " " "
4861	4861	4861	4861	4861	Hβ very bright.
4713			4716		Bright.
4661			4664		"
4473	4472	4472	4472		Very Bright.
4390	4389	4389	4389		Bright.
4363	4364	4364	4364		"
4341	4341	4341	4341	4341	Hγ, brightest line in spectrum.
4265			4265		Very faint.
423			423		Extremely faint.
4145	4143	4143	4143		Faint.
4122		4121	4121		"
4102	4102	4102	4102	4102	Hδ, very bright.
4067	4067	4067	4067		Bright.
4026	4026	4026	4026		"
3969	3969	3969	3969	3969	Hϵ, very bright.
3889	3889	3889	3889		Hζ, bright.
3868	3869	3869	3869		Bright.
3836	3835	3835	3835		Hη, bright.
	3798	3798	3798		Hϑ, faint.
	3770		3770		Hι, "
	3749		3749		Hκ, very faint.
	3727	3727	3727		Very bright.

"The lines of the new (1890) photographs contain two very strong and abruptly bounded blotches, and a third one less marked.

"These brighter blotches, corresponding to different conditions of closely adjacent nebular matter, give an explanation of an appearance which we recorded last year in speaking of the strong line 'about λ 3724' [on the 1888 negative]. 'On one side of the

wide as the slit, if the camera and collimater are equal in length. Any broader part of the line must be wider than the slit, in which case it is really a band made up of light not monochromatic. If photographs obtained with the low dispersion of one or two 60° prisms show the increased breadth of parts of the lines, a grating giving ten or twelve times as much dispersion and a high-power eye-piece should readily show the non-monochromatic character of the lines, and their varying widths in different parts of the nebula. My visual observations, described above, do not show any such conditions.

(*b*).—My photographs made with the 12-inch telescope and the very rigid spectroscope show lines 0.42 inch long *of constant width and all the lines are of equal width.* The slit was long enough to include a wide region each side of the Trapezium.

(*c*).—On those negatives which show lines not of uniform width, the *general* form of the images is the same for all the lines prominent enough to photograph in both bright and faint parts of the nebula. The *general* form is the same whether the slit crossed the Trapezium or was directed to some other region,—other things being equal the middle of the slit gave a slightly greater width to the lines than the ends did;—therefore the increased breadth is due in part to instrumental causes.

(*d*).—The lines are slightly broader for bright parts of the nebula crossed by the slit than for adjacent faint parts, for two reasons: first, the spreading of the over exposed images of the bright parts; secondly, and chiefly there is flexure in the spectroscope during the exposure. The effect of flexure is to broaden the brighter parts of the line more than the fainter parts; for the brightest parts photograph in their first and last positions on the plate, whereas the faintest parts photograph only where the successive positions of the images overlap each other.

In this connection we should not lose sight of a vigorous discussion carried on in 1890, regarding the character of the chief line in the Nebula of Orion. From their published paper* I understand Dr. and Mrs. Huggins' views [confirmed by the observations of Young, Copeland, Keeler, and others] to be that the principal line in this spectrum is sharply defined, very narrow under the highest dispersions, and perfectly monochromatic. My photographs show the principal nebular line (λ 5007) to have always the same form as the $H\beta$, and $H\gamma$ and other prominent lines, just as Dr. and Mrs. Huggins' negatives show that the hydrogen and other strong lines are similarly broadened. Now if the

* *Proc. Roy. Soc.*, vol. 48, pp. 207-212.

principal line was shown by visual observations in 1890 to be sharp, narrow and monochromatic, it seems to me that to accept the photographic irregularly broadened form of the images as their real form, is to ascribe to the principal line a very different character.

I have photographed the spectra of the six principal stars in the central part of the nebula. They are Struve's Trapezium stars A, B, C, D, and Bond's stars Nos. 685, 734. The photographs were made principally on Oct. 18, Nov. 10, Dec. 15, 1893 and Jan. 11, 12 and 23, 1894. So far as I know these spectra have never been photographed with a slit before, except in the case of two of the Trapezium stars by Dr. and Mrs. Huggins in 1888. My photographs show continuous spectra, numerous dark lines, and no bright lines; and all the spectra conform closely to the "Orion type;" though some of the dark lines are broader than we would expect to find in that type. Only one part of the spectrum of the faintest Trapezium star, B, was photographed, on account of its faintness. The Hδ line is dark, there are no bright lines in that vicinity, and its spectrum is probably very similar to the others. Two long exposures on the nebular spectrum show the combined spectrum of the Trapezium stars up to Hκ, and the hydrogen lines Hζ, Hη, Hϑ, Hι are seen to be dark. They may safely be attributed to the brightest star C. All the dark lines photographed in the six stars are recorded in the first six columns of Table II. On many of the negatives, some of the bright nebular lines were necessarily recorded at the same time; but they are in general considerably narrower than the dark star lines, and pass through the centers of the dark lines. The dark Hβ lines in the stars C, D, A, are scarcely wider than the Hβ bright nebular line, but there is no doubt they are dark. The points of intersection of the nebular lines λ5007 and λ4959 with the spectra of the stars C, D, A are brighter than the continuous spectra each side of the points of intersection, owing to the superposition, probably, of the bright nebular lines and continuous star spectra. It is possible, to be sure, that the increased brightness at those points could be due to bright lines in the stars, and such may be the case; but taking the other parts of these spectra and other spectra of this type into consideration, there seems to be no necessity for such an assumption.

Our knowledge of the spectra of β, δ, and ϵ Orionis is very fragmentary, and I have also made a few photographs of them. Nearly all the lines observed in δ Orionis between λ 493 and λ 393 are tabulated in column seven below. β and ϵ Orionis contain a

great many lines, and I have tabulated in columns eight and nine only those which match lines observed in the six faint stars; except that in these cases I photographed with isochromatic plates, also, and detected a prominent dark line in the position of the sodium D_{12}, and a very prominent dark line in the position of the helium D_3. The latter is probably D_3; in which case it is the first time, so far as I know, that a dark D_3 has been observed. A great many prominent lines in β and ϵ Orionis and a few in δ Orionis have been omitted.

TABLE II.—DARK LINES PHOTOGRAPHED IN ORION STARS.

ΣC	ΣD	ΣA	ΣB	No. 685	No. 734	δ Orion.	ε Orion.	β Orion.
							5893	5893
							5876	5876
4924	4924	4824		4924		4924	4924	4924
4861	4861	4861		4861	4861	4861	4861	4861
				4715		4715	4715	
4688				4688		4688	4688	
						4662	4662	
	4652	4652		4652		4652	4652	
461								
454						454		
4472	4472	4472		4472	4472	4472	4472	4472
4389	4389	4389		4389	4389	4389	4389	4389
4341	4341	4341		4341	4341	4341	4341	4341
								4267
								4230
4203						4203		
	4143	4143			4143	4143	4143	4143
	4121	4121		4121	4121	4121	4121	4121
4102	4102	4102	4102	4102	4102	4102	4102	4102
	4067	4067				4067	4067	
4026	4026	4026		4026	4067	4026	4026	4026
3969	3969	3969		3969	3969	3969	3969	3969

A comparison of these dark lines with the bright lines in the nebula points to a most interesting result.

In 1890 Dr. Scheiner called attention* to a dark line λ 4471.4 discovered by him in β, γ, δ, ϵ, and ζ Orionis (and in β Persei), stating that it probably coincided with Copeland's very faint bright line λ 4476 in the Orion nebula, and that it was evidence of the physical connection of the nebula and the five bright Orion stars mentioned. Professor Pickering† in 1891 called attention to a further coincidence of the dark line λ 470 (probably the line observed by me at λ 4688) in the bright stars in Orion and a bright line λ 470 in the planetary nebulæ.

* *Sitzungsberichte d. Berlin. Akad.*, 1890, p. 146.
† *Astronomische Nachrichten*, No. 3025.

To facilitate further comparisons, I have placed in the first column of Table III the bright lines observed by me in the Orion Nebula, in the second column the dark lines in the faint stars in the nebula, and in the third column the corresponding lines in the bright stars β, δ and ϵ Orionis.

TABLE III.—COMPARISON OF BRIGHT NEBULAR AND DARK STAR LINES.

Nebular bright lines.	Faint-star dark lines	Bright-star dark lines	Remarks.
		5893	D_{12}. Faint stars not photo'd in yellow.
5874		5876	D_3
5007			Stellar spectra apparently continuous.
4959			" " " "
	4924	4924	Bright line suspected in Orion Neb. & G. C. 4390.
4861	4861	4861	$H\beta$.
4714	4715	4715	Observed in several nebulæ.
	4688	4688	" " five "
4662		4662	" " a few "
	4652	4652	Not observed in any nebulæ.
	461.		Probably observed in G. C. 4390.
	454	454	Not observed in any nebulæ
4472	4472	4472	Very common in nebulæ.
4389	4389	4389	Observed in several nebulæ.
4364			Very common in nebulæ.
4341	4341	4341	$H\gamma$
4265		4267	Observed also in G. C. 4390.
423		4230	Not observed in other nebulæ.
4143	4143	4143	Observed also in G. C. 4390.
4121	4121	4121	" " " " "
4102	4102	4102	$H\delta$
4067	4067	4067	Observed in several nebulæ.
4026	4026	4026	" " " "
3969	3969	3969	$H\epsilon$
3889	3889		$H\zeta$

some exceptions. There is no reason known at present for supposing that the prominent nebular lines λ 5007, λ 4959, λ 4364, λ 3869, λ 3727 are matched by dark stellar lines. They appear to remain characteristic of the nebular spectrum, and of no other type. If these dark-line stars are *within* or *beyond* the great nebula, and the stellar light were absorbed by the nebula through which it passes, we would expect dark stellar lines in the positions of these five prominent nebular lines. The fact that some or all of these positions are not occupied by dark lines renders it doubtful whether any of the lines are due to absorption by the nebula proper, and we cannot safely say that these stars are beyond the nebula or are physically connected with it. We can safely say, however, that they are closely related to the nebula in chemical constitution and *relatively* closely in physical condition.

Dr. and Mrs. Huggins' 1888 photograph * of the spectra of this nebula and two superposed trapezium stars is well-known to all readers of spectroscopic literature. Several most interesting and important conclusions have been based upon it. Nevertheless, I have been unable to verify many of their observed facts; and as this subject is of superlative importance,—situated, as it is, at or near the beginning of every system of stellar classification,—it is desirable to point out definitely the differences in our results. I shall limit the comparison to the region λ 420—λ 397, since my photographs of the stellar spectra do not extend beyond Hε.

Dr. and Mrs. Huggins obtained:	Mr. Campbell obtained:
1. No trace of bright Hδ in nebula.	1. Very prominent bright Hδ in nebula.
2. " " " " λ 4067 " "	2. Prominent bright λ 4067 in nebula.
3. " " " " λ 4026 " "	3. " bright λ 4026 in nebula
4. " " " " Hε " "	4. Very prominent bright Hε in nebula.
5. No trace of dark lines at Hδ, λ 4067, λ 4026, Hε in Trapezium stars.	5. Very prominent dark lines at Hδ, λ 4026 and Hε, and prominent dark line at λ 4067, in Trapezium stars.
6. A group of six bright lines in Trapezium stars between λ 4167 and λ 4116, possibly also in nebula; no dark lines in stars.	6. No bright lines in stars between λ 4167—λ 4116; bright lines in nebula at λ 4143, and λ 4121; dark lines in stars at λ 4143, λ 4121.

It will be seen that, in this region, I did not succeed in photographing their faint lines, and they did not record my bright lines; I did not obtain their bright lines in the stars and they did not obtain my dark lines in the stars. It is difficult to explain

* Described in *Proc. Roy. Soc.*, Vol. 46, pp. 40-60.

these differences on the basis of variations in the stellar and nebular spectra. But if they are due to real variations, we have here the most remarkable case of variation known in astronomy.

Although the observations described in the preceding pages are more complete and lead to more interesting results than were even hoped for at the beginning of the work, it is evident that they throw very little light upon questions relating to the composition and physical condition of this important nebula. Apparently we cannot reproduce nebular conditions in terrestrial experiments, and every advance in our knowledge of the nebulæ only adds greater emphasis to that point. Nevertheless, it is desirable that the observations of the spectrum should be made as complete as possible, however backward our interpretations of the observed facts may be. Holding to that view, I hope next season, with more efficient spectroscopes, to extend the results considerably further.

(To be Continued).

THE NEW STAR IN NORMA.*

EDWARD C. PICKERING.

On February 28, 1894, Professor S. I. Bailey succeeded in obtaining a photograph of the spectrum of the new star in Norma. The photograph was taken with the Bache telescope, having the 13° prism placed in front of the object-glass, and had an exposure of 166 minutes. Nearly all the light is concentrated in the line Hγ, wave-length 434. Traces of several other lines are perceptible in the contact print sent by Mr. Bailey. Their wave-lengths can probably be determined when the original negative is received in Cambridge. The brightness appears to have diminished about half a magnitude between October 29, 1893 and February 28, 1894. Professor Bailey has looked at Nova Normæ with the 13-inch telescope at Arequipa, employing magnifying powers up to 800, without perceiving any disk.

HARVARD COLLEGE OBSERVATORY,
Cambridge, Mass. April 13, 1894.

ASTRO-PHYSICAL NOTES.

All articles and correspondence relating to spectroscopy and other subjects, properly included in ASTRO-PHYSICS, should be addressed to George E. Hale, Kenwood Observatory of the University of Chicago, Chicago, U. S. A. Authors of papers are requested to refer to last page for information in regard to illustrations, reprint copies, etc.

* Communicated by the author.

The Influence of Slit-Width on the Appearance of the Spectra of Comets.—Professor Kayser's important paper seems to furnish a complete explanation of the anomalies hitherto observed in the spectra of comets. In a general way, the influence of slit-width on the position of the edge of a fluting has been understood and allowed for by spectroscopists, but a detailed consideration of all the consequences attending the use of a wide slit with small dispersion is now given for the first time. Now that photography has taken the place of the difficult eye observations, it is probable that fewer anomalies will be found in cometary spectra, and that the systematic changes in the position of the carbon bands which have in some instances been recorded, are to be ascribed to the varying conditions under which the observations were made.

Dr. Chandler's Criticisms of the Harvard Photometric Observations.—In A. N. 3214, Dr. Chandler calls attention to serious errors in the photometric observations which have been made with the meridian photometer at Harvard College Observatory. This is evidently an important matter, and it should be thoroughly investigated, as one of the chief purposes of the Harvard Photometry is to provide a series of standards which may serve as a basis for future work. The errors referred to were detected by comparing the observations of variable stars with the magnitudes deduced from their known elements. Dr. Chandler says, "In constructing the Second Catalogue of Variable Stars, recently published, I had occasion to examine with some care the photometric observations in Vol. XXIV. of the Harvard College Observatory Annals. It soon became manifest that there were numerous incongruities in the observations there given, with the known characteristics of variation of many of the stars, as well as with other well attested series of contemporaneous observations. To be more specific, a list of some of these contradictions is here given. It is by no means complete, but comprises perhaps the more important results of a somewhat desultory examination, which was only carried far enough to justify, as it seemed to me, an impression of distrust whether any of these observations are suitable for any precise or critical purpose."

A list of important discrepancies is given by Dr. Chandler, one or two of the most striking of which may be given here as examples.

112. R Andromedæ. The 1886 observations are certainly all erroneous by two or three magnitudes, since the minimum occurred 1886 Dec. 5 at 12.8 mag., by the elements; the latter being confirmed by Parkhurst's observations.

4160. T Leonis. The observation 1882 April 17 gives 7.4 mag. This is very surprising, as the star has never been certainly seen by any one except Peters, and by him only on two occasions at 10.11 mag., on all others at 13 mag. or less.

5826. T Scorpii. This is the new star of 1860, in the cluster 80 Messier, 5″ north following its center, in position angle 55°. It appeared suddenly (7.0 mag.) and faded rapidly to invisibility, and has never since been seen, except possibly once on 1869 June 1, when it was uncertainly suspected by Schönfeld. I am perfectly familiar with this cluster and its surroundings, having looked for the missing star more than a hundred times at the very least, unsuccessfully; Schmidt, at least one thousand times between 1860 and 1877. Except on the very improbable hypothesis that the star, by a happy accident, had returned at the very epoch when the photometer happened to be set upon it in 1886, the three observations of it as 8.2 mag. must be mistakes.

Dr. Chandler attributes many of these discrepancies to erroneous identifications of stars with the meridian photometer, for, the mirrors of that instrument being movable through a small range in Right Ascension, there is no such check on the identity of the star as there is in ordinary meridian observations. If this is the case the errors will be comparatively few in number, and may be corrected. If on the other hand they arise from a fundamental weakness of principle or inaccuracy of observation, as certain discrepancies in comparatively bright stars seem to indicate, the value of the whole work becomes practically nothing. In either case Dr. Chandler has done a valuable service in pointing out the existence of these errors, and it is to be noted that all his remarks are strictly within the bounds of legitimate criticism.

A New Triple Achromatic Object-Glass.—At the March meeting of the Royal Astronomical Society Mr. H. Dennis Taylor described a new and "perfectly achromatic" object-glass which he has for some time been engaged in perfecting. Mr. Taylor explained that by "perfectly achromatic" he meant that the chromatic aberration to be expected in an objective of even two feet aperture is no greater than that which is introduced into a reflector whose focal length is 7½ times its aperture when a Huyghenian eye-piece is used for viewing the image. The objective consists of three lenses of three different kinds of glass. The outside lens is a hard baryta light flint, and is double convex in form; the inner lens is double concave, of a certain new boro-silicate flint; the back lens of the combination is a positive meniscus nearly plane on the outside surface, and is made of light silicate crown glass. All three varieties of glass are made by Schott of Jena. The flint glass of the interior lens is slightly liable to tarnish and hence is protected by being enclosed between the hard outer lenses. It is not stated whether the lenses are cemented, but cementing would be impossible in a large lens without great injury to the definition, and without it gradual tarnishing of the interior surfaces would seem to be unavoidable; nevertheless Messrs. Cooke & Sons guarantee the permanence of objectives made on this plan.

Some years ago Professor Hastings made a double objective, using a certain boro-silicate flint and a potassium silicate crown made at Jena, in which the chromatic aberration was reduced to something like five per cent. of its amount in the usual construction, so that the image was practically free from color, but the flint glass was liable to tarnish, while the crown glass was hygroscopic, and was therefore always covered by a film of moisture. Singularly enough the polish of the crown lens does not seem to have been affected by this peculiarity of the glass, and is apparently as good at the present time as it was when the objective was made, but it would seem that *some* action tending to destroy the surface must be going on, nevertheless. The flint glass is nearly identical with that used by Mr. Taylor.

The theory of the triple objective was discussed by Hastings in the *American Journal of Science*, December 1879. Mr. Taylor has a greater variety of glasses from which to select the lenses of his combination, and has doubtless chosen the kinds most suitable for his purpose; otherwise the objective does not seem to differ materially from that proposed by Hastings. A section of Mr. Taylor's objective accompanies the advertisment of Messrs. Cooke in *Knowledge*. If this figure is drawn to scale, the interior lens is extremely thin in the middle, and must be very difficult to work.

An impression seems to have been produced in some quarters that *all* Jena glass is subject to some such drawback as that mentioned above, and therefore it

may be worth while to state that only a few very special varieties are so affected, and that with these exceptions all the glass made at Jena is perfectly durable.

New Star Spectroscope for the Lick Observatory.—A large and very fine star spectroscope for the Lick Observatory has just been completed at the workshops of Mr. J. A. Brashear. We shall hope to give a detailed account of this fine instrument in a future number.

The Aurora of March 30, 1894.—On the night of March 30, an unusually fine display of the aurora was observed in the United States and in Europe. At Alexandria, Va., the streamers when highest extended nearly to the zenith; at Allegheny they extended fifteen degrees south of the zenith, at 9^h 50^m Eastern Time. Numerous reports have been received, from which we give a few extracts.

"On March 30 at 7:45 P. M. a brilliant aurora borealis was visible here. At first it appeared as a large patch of pale red light in the north east. It faded away, and reappeared suddenly, with streamers reaching up some 45°, and extended around to Polaris; it then faded again, and reappeared in two sections,—bright red patches with dark and light streamers. The two sections seemed disposed to spread out and join each other. The western section at its brightest was about 30° wide, enveloping and almost obliterating the constellation Cassiopeia: the other formed east of Polaris, and extended nearly to Arcturus, streaming upward between Ursa Major and Ursa Minor. It did not become so brilliant again, as long as we watched it, but traces of it could be seen until after midnight."

JOHN W. JOHNSON,
Associate Professor of Physics.

University of Mississippi.
March 31, 1894.

The following is an extract from a letter received from Dr. Veeder:

"You will probably be interested in the results of the observations of the aurora of Friday, March 30, which was well seen, in the vicinity of the 77th meridian especially, from South Carolina northward into Canada, and many reports concerning which are at hand. The peculiar feature is that from Lyons northward into Canada, from 7:40 to 8:38 P. M., it was located south of the zenith exclusively, there being not even the faintest auroral glow toward the north during this hour. In Pennsylvania it was seen north and south of the zenith, and further south it was seen toward the north exclusively, rising 25° above the northern horizon in South Carolina. From this it appears that the center of the luminous mass was at about latitude 40° to 41°, and 600 miles further south its summit was seen toward the north, having the elevation of 25°. This would give approximately a right-angled triangle with a base of 600 miles and an angle at the base of 25°, corresponding to an altitude of the aurora of about 300 to 350 miles at the highest point.

The solar conditions also were characteristic at the time of this aurora, being such as have been described heretofore. There was a disturbance exactly on the eastern limb of the Sun and south of the equator, which is the precise location to have an auroral effect at this season of the year. With the disturbed area north of the equator at this season, increase of thunder storms takes the place of the aurora. Both these points were well illustrated many times during March, as well as in the months preceding. The most noticeable instance showing these relations recently is in connection with a very persistently disturbed area extending north and south of the equator, whose successive reappearances at the limb

have been attended by increase of thunderstorms and auroras. The dates are as follows May 22, June 18, July 15, Aug. 12, Sept. 8, Oct. 5, Nov. 1, Nov. 29, Dec. 26, Jan. 22, Feb. 18, March 18, or eleven returns in 300 days, giving a period of 27¼ days. There were auroras on all these dates, those on June 18 July 15, Aug. 12 and Nov. 1 being especially fine. In like manner there was increase of thunderstorms on all of them, extending throughout the winter in a way that is most remarkable. Thus buildings were struck by lightning in New York State on Christmas night, an occurrence which is most unusual. The outbreak of vigorous electrical storms and tornadoes on March 18th is fresh in memory. Since October the U. S. Weather Bureau has published in the *Monthly Weather Review* a table showing the numbers of stations reporting auroras and thunderstorms each day, and their geographical distribution. In these tables the dates above indicated stand out prominently.

I am receiving some excellent reports from Siberia, Finland, Sweden, Scotland and England, especially. The Greenland reports will become accessible upon the return of the ship "Falcon," which will visit Mr. Peary's station as soon as removal of the ice will permit." M. A. VEEDER.

Lyons, N. Y., April 11, 1894.

Lowell Observatory.—A private Observatory is to be erected and kept up during the coming summer and autumn by Mr. Percival Lowell in Arizona His objects are: (a) the study of Mars during its approaching opposition; (b) the determination of the atmospheric conditions most favorable to astronomical observation. He will take with him Prof. W. H. Pickering and Mr. A. E. Douglass.

The observatory's equipment will consist of an 18-inch lens by Brashear, a 12-inch lens by Clark and a six-inch lens by Clark, the two former to be mounted in a dome 34 feet in diameter. The dome is to be built of wood and constructed in parallel arches covered with wire netting and canvas upon a plan devised by Professor W. H. Pickering.

It is hoped to have the telescope in place and the Observatory in working order by June 1st.

The note in our last number relative to this Observatory was founded on a newspaper report. the information given above is from a reliable source.

Glazebrook's Light.*—It is not often that one steps backward in writing a second volume on a subject which he has previously treated. But we cannot avoid the conclusion that this is what Mr. Glazebrook has done in his new volume on light. For in his *Physical Optics*, which appeared more than ten years ago, he gave us a lucid, and to many a student, very helpful, discussion of the nature of light. In the earlier volume the wave surface was emphasized as being the fact of nature; its normal, the so-called ray, as being a mathematical convention for fixing the plane of the wave-surface at any point. Following Fresnel, everything was deduced from the principle of secondary waves together with the principle of interference. Lenses he described as simply instruments for changing the curvature of the incident wave-front. Phenomena of double refraction were, of course, treated by use of the wave-surface.

* *Light an Elementary Text-book, Theoretical and Practical*, by R. T. Glazebrook, M. A., F. R. S. (Cambridge University Press, 1894).

But, in the volume before us, all is changed. Only with difficulty does one believe that the two books are written by the same man. A prism is no longer a device for rotating a wave-surface; a focus is no longer the centre of curvature of a certain emergent wave-surface. Definitions, demonstrations, and figures are all in terms of rays, excellently adapted to elucidate the corpuscular theory.

One has to read but a few pages of the later volume to learn that it is by no means intended as a substitute for the earlier. But, unless its object be completely misunderstood, it is much more than a laboratory guide, it is an exposition of the nature of light; and, as such, most of its pages are devoted to what light is not, namely, rays.

The plea of mathematical simplicity can hardly be urged in its favor: for, though there are few formulæ in the book, what few there are might have been derived more quickly and elegantly by use of the wave-surface.

To be sure, the subject is here purposely treated in an elementary way; but Mr. Glazebrook himself, not to mention Lord Kelvin and Prof. S. P. Thompson, has shown us that in the wave-surface treatment there is an elegant simplicity which makes the subject at once clear and attractive, and which unifies the discussion in a marvelous way, allowing the student to pass from the treatment of lenses to the treatment of mirrors and gratings without the slightest interruption of continuity either of method or of nomenclature.

What is more important still, the method of the wave-surface allows one to begin the subject with the experimental evidence of Young and Fresnel for the wave theory, and then proceed through the whole discussion without any brea k in method. Two pin holes in a piece of platinum, or two parallel rulings with the point of a knife on an undeveloped photographic dry plate may be used to view a candle flame, or better still an illuminated slit in a visiting card. They furnish the student very cogent evidence for the wave theory. Many of Fresnel's diffraction and interference experiments are easily shown, and make a natural starting point. Lord Kelvin's beautiful wave model is within reach of the most impecunious, and is a powerful aid in clearing up the beginner's ideas of transverse waves.

But it must not be forgotten that the volume under review is intended also to assist the student in the laboratory and it goes almost without saying that it contains many excellently selected experiments.

Our experience is, however, that the student who carries with him, to the laboratory, his ideas in terms of the wave surface has the clearest possible views regarding his experimental work. We are still speaking of the beginner. If his problem be to determine the refractive index of a liquid by its "lifting power," say, he measures the change of curvature impressed upon the spherical wave-front at emergence from the plane surface of the liquid. The adjustment of optical instruments, measurement of focal lengths, and determination of curvatures fall into line at once; so that, in this manner, as well as that employed by Mr. Glazebrook there is no gap between lecture room and laboratory.

And as to some problems, a trifle more advanced, as, for instance, the resolving power of prisms, or the magnifying power of the astronomical telescope, one is continually astonished at the ease with which they yield to treatment in terms of the wave-front, and that without any mathematics other than the elements of geometry.

Mr. Glazebrook's contributions to optics are too well known to need rehearsal here. We all know what he might have done in the way of writing an elementary text-book on light. H. C.

Variation of Gravity with Altitude.—When the new standards of mass were recently received by the United States from the International Bureau, it was remarked by one of our officials that two kilogram masses placed upon one pan of a balance showed a different weight according as they were placed side by side or one on top the other. That their weights *were* different in these two positions no one doubted; for in the second case one kilo was certainly some five or six centimeters farther away from the center of the Earth. But the idea of this difference being detectable struck many as being incredible, not to say ludicrous.

A moment's use of lead pencil and paper would have shown them, however, that the degree of accuracy required is only (!) about one part in sixty million. The possibility of weighing with this degree of refinement appears to be an accomplished fact.

It is now five or six years that Messrs. Richarz and Krigar-Menzel have been engaged in measuring the diminunition of gravity through a height of some seven feet: a piece of work undertaken at the suggestion of Helmholtz. The experiments were carried out in one of the bastions of the fortress at Spandau. The essential feature of the method is the use of a balance with two pans hung from each arm, these pans being separated by a vertical distance of 226 centimeters. A method of double weighing was employed in which one kilogram, A say, in the *upper left* hand pan was balanced against kilogram B in the *lower right* hand pan. Then kilo A was transferred to the *lower left* hand pan while B is placed in the *upper right* hand pan. Thus the quantity measured is twice the change of weight in either kilo.

In their description of the work, [*Wied. Ann., Bd. 51*, pp. 559-583 (1894)] these gentlemen give a valuable and suggestive discussion of the errors of the balance.

The precautions taken against thermal change, the *bete noir* of physical measurements, were very elaborate. The success with which this was accomplished may be judged from the fact that the presence of a man at the balance case meant changes in temperature from which the balance did not recover sufficiently for the work to proceed, until the lapse of five days (p. 566).

A few hundredths of a degree difference in temperature between the upper and lower pans produced convection currents strong enough to mask the effect under investigation.

Of course the shifting of masses from one pan to the other was accomplished

CURRENT CELESTIAL PHENOMENA.

PLANET NOTES FOR JUNE.

Mercury will be "evening star" during June. On the 22d he will be at his greatest distance (elongation) east from the Sun, and will set about an hour and a half later than that body. This month will be a good time both for daylight and evening observations of this planet. Its phase will be gibbous during the first half and crescent during the last half of the month. The moon will pass by Mercury on the evening of June 4, conjunction in right ascension occurring at $10^h\ 32^m$ central time.

Venus will be "morning star" rising about two hours before the Sun. She is getting around toward the farther side of her orbit so that her brightness is decreasing considerably. At the same time her phase is becoming more gibbous. At the beginning of the month 0.67 and at the end 0.76 of her disc will be illuminated.

Considerable has been said lately about the dark part of the disc of Venus being visible, just as the dark part of the new Moon is visible. Several observers claim to have seen the complete outline of Venus' disc a few days before she disappeared in the rays of the Sun this past winter, when her crescent was very narrow. We may say, I think, that this visibility is not from the same cause that renders the dark part of the moon visible, viz.: reflected earthshine. Venus is more than 100 times as far as the Moon from the Earth and therefore would receive less than the ten-thousandth part of the light thrown upon the Moon. The most probable explanation is that Venus has a dense atmosphere, possibly more extensive than that of the Earth, so that her twilight is longer, and extends far enough into the dark hemisphere to become visible from the Earth as a complete ring of light when the crescent of direct illumination is small. The observer discerning the outline of the dark part of the planet, by this faint ring, would naturally have the impression of seeing it all.

Mars will be at quadrature, 90° west from the Sun, June 17, and will be in position to be observed after midnight during this month. Mars will move northeast during June, from Aquarius across a little corner of Pisces into Cetus. The phase of the planet will be smaller this month than at any other time in the year, only 0.84 of the disc being illuminated. Mars will be in conjunction with the Moon, about 3° south of the latter, 48^m after midnight, June 25.

Jupiter and *Neptune* are not to be seen during June.

Saturn is making the turn of the loop in his apparent path among the stars of Virgo. He will begin to move eastward after June 21. The amateur should not fail to make the most of these summer months in the study of this planet. The surface markings on so bright a planet are almost as likely to be seen with a small telescope as with a large one. The Moon will pass by Saturn, 4° south of the latter, June 12, at $2^h\ 41^m$ P. M. central time.

Uranus will be in his most convenient situation for observation during June, being near the meridian during the evening hours. He ought to be easily found by means of stars α and μ Libræ (see Poole Bros. map). Look about 1° 30′ west and 30′ north, *i. e.*, 3 diameters of the Moon west and 1 diameter north, of α, for a star with a dull green disc a little brighter than the star μ.

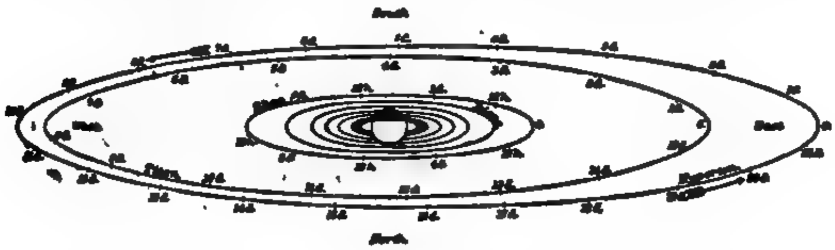
Planet Tables for June.

[The times given are local time for Northfield. To obtain Standard Times for Places in approximately the same latitude, add the difference between Standard and Local Time if west of the Standard Meridian or subtract if east].

Date.	R. A.	Decl.	Rises.	Transits.	Sets.
1894.	h m	° ′	h m	h m	h m
			MERCURY.		
June 5......	6 12.1	+25 31	5 19 A. M.	1 15.3 P. M.	9 11 P. M.
15......	7 20.7	+23 47	5 58 "	1 44.4 "	9 31 "
25......	8 05.7	+20 21	6 21 "	1 50.0 "	9 19 "
			VENUS.		
June 5......	2 04.5	+10 13	2 24 A. M.	9 08.3 A. M.	3 53 P. M.
15......	2 48.2	+13 49	2 13 "	9 12.6 "	4 12 "
25......	3 33.9	+17 01	2 05 "	9 18.8 "	4 32 "
			MARS.		
June 5......	23 20.7	− 6 51	12 49 A. M.	6 25.0 A. M.	12 01 P. M.
15......	23 45.6	− 4 26	12 24 "	6 10.5 "	11 56 A. M.
25......	0 09.6	− 2 03	12 00 "	5 55.2 "	11 50 "
			JUPITER.		
June 5......	4 51.5	+22 03	4 17 A. M.	11 55.0 A. M.	7 33 P. M.
15......	5 01.4	+22 19	3 46 "	11 25.5 "	7 04 "
25......	5 11.2	+22 32	3 16 "	10 56.0 "	[illegible] 36 "
			SATURN.		
June 5......	13 12.7	− 4 50	2 30 P. M.	8 14.7 P. M.	1 59 A. M.
15......	13 12.0	− 4 49	1 50 "	7 34.7 "	1 19 "
25......	13 11.9	− 4 51	1 11 "	6 55.3 "	12 40 "
			URANUS.		
June 5......	14 38.8	−15 04	4 38 P. M.	9 40.6 P. M.	2 43 A. M.
15......	14 37.7	−14 59	3 57 "	9 00.1 "	2 03 "
25......	14 36.7	−14 55	3 17 "	8 19.9 "	1 23 "
			NEPTUNE.		
June 5......	4 48.4	+20 58	4 20 A. M.	11 51.9 A. M.	7 24 P. M.
15......	4 49.9	+21 01	3 42 "	11 14.2 "	[illegible] 57 "
25......	4 51.5	+21 04	3 04 "	10 36.3 "	6 09 "
			THE SUN.		
June 5......	4 54.9	+22 37	4 17 A. M.	11 58.3 A. M.	7 40 P. M.
15......	5 36.3	+23 21	4 15 "	12 00.3 P. M.	7 46 "
25......	6 17.9	+23 23	4 17 "	12 02.4 "	7 48 "
			THE MOON.		
June 2	3 25.6	+22 32	2 54 A. M.	10 41.0 A. M.	6 45 P. M.

Elongations of the Satellites of Saturn.

[In the diagram the points marked 0 are those of eastern elongation of the several satellites. Their positions at intervals of one day after eastern elongation are indicated by the symbols 1*d*, 2*d*, etc].



MIMAS.

		h		
June	2	12.3	A. M.	W
	2	10.9	P. M.	W
	3	9.5	"	W
	4	8.1	"	W
	5	6.7	"	W
	6	4.3	"	W
	7	2.9	"	W
	8	1.5	"	W
	9	1.9	A. M.	E
	10	12.5	"	E
	10	11.1	P. M.	E
	11	9.8	"	E
	12	8.4	"	E
	13	7.0	"	E
	14	5.6	"	E
	15	4.2	"	E
	16	2.8	"	E
	18	12.8	A. M.	W
	18	11.4	P. M.	W
	19	10.0	"	W
	20	8.6	"	W
	21	7.2	"	W
	22	5.8	"	W
	23	4.4	"	W
	24	3.0	"	W
	25	1.6	"	W
	26	11.7	"	E
	27	10.3	"	E
	28	8.9	"	E
	29	7.5	"	E
	30	6.1	"	E

ENCELADUS.

June	1	7.0	P. M.	E
	3	3.9	A. M.	E
	4	12.7	P. M.	E
	5	9.6	"	E
	7	6.5	A. M.	E
	8	3.4	P. M.	E
	10	12.2	A. M.	E

ENCELADUS CONT.

		h		
June	11	9.1	A. M.	E
	12	6.0	P. M.	E
	14	2.9	A. M.	E
	15	11.8	"	E
	16	8.7	P. M.	E
	18	5.5	A. M.	E
	19	2.4	P. M.	E
	20	11.3	"	E
	22	8.2	A. M.	E
	23	5.1	P. M.	E
	25	1.9	A. M.	E
	26	10.8	"	E
	27	7.7	P. M.	E
	29	4.6	A. M.	E
	30	1.5	P. M.	E

TETHYS.

June	2	11.5	P. M.	E
	4	8.8	"	E
	6	6.0	"	E
	8	3.3	"	E
	10	12.6	"	E
	12	9.9	A. M.	E
	14	7.2	"	E
	16	4.5	"	E
	18	1.8	"	E
	19	11.1	P. M.	E
	21	8.4	"	E
	23	5.7	"	E
	25	3.0	"	E
	27	12.3	"	E
	29	9.6	A. M.	E

DIONE.

June	2	12.7	A. M.	E
	4	6.4	P. M.	E
	7	12.1	"	E
	10	5.8	A. M.	E
	12	11.4	P. M.	E

DIONE CONT.

		h		
June	15	5.1	A. M.	E
	18	10.8	A. M.	E
	21	4.5	"	E
	23	10.1	P. M.	E
	26	3.8	"	E
	29	9.5	A. M.	E

RHEA.

June	5	5.9	A. M.	E
	9	6.3	P. M.	E
	14	6.7	A. M.	E
	18	7.0	P. M.	E
	23	7.4	A. M.	E
	27	7.8	P. M.	E

TITAN.

June	3	7.0	P. M.	I
	7	9.9	"	W
	12	12.4	A. M.	S
	15	7.4	P. M.	E
	19	5.1	"	I
	23	8.0	"	W
	27	10.5	"	S

HYPERION.

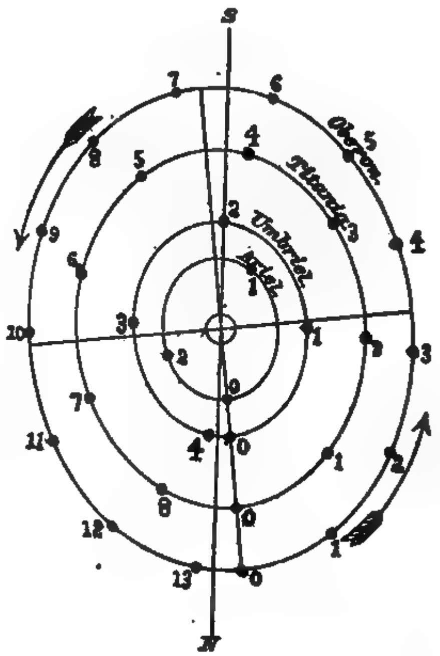
June	3	5.8	P. M.	E
	10	2.0	A. M.	I
	15	8.7	"	W
	19	6.2	P. M.	S
	24	11.6	"	E
July	1	8.9	A. M.	I

IAPETUS.

May	17	4.3	P. M.	W
June	6	3.7	A. M.	S
	26	4.0	P. M.	E
July	14	5.3	"	I

Elongations of the Satellites of Uranus.

[The diagram shows the apparent paths of the satellites of Uranus during the summer of 1894. The black dots with the numerals indicate the positions of the satellites at intervals of 1 day after each northern elongation. The points marked 0 are those of northern elongation.]



ARIEL.

		h	
June	2	5.0 P. M.	N
	5	5.5 A. M.	N
	7	6.0 P. M.	N
	10	6.4 A. M.	N
	12	6.9 P. M.	N
	15	7.4 A. M.	N
	17	7.9 P. M.	N
	20	8.4 A. M.	N
	[illegible]	8.9 P. M.	N
	25	9.3 A. M.	N
	27	9.8 P. M.	N
	30	10.3 A. M.	N

UMBRIEL.

		h	
June	1	9.8 P. M.	N
	6	12.4 A. M.	N
	10	3.9 "	N
	14	7.3 "	N
	[illegible]	10.8 "	N
	22	2.3 P. M.	N
	26	5.8 "	N
	30	9.2 "	N

TITANIA.

		h	
June	[illegible]	7.3 A. M.	N
	6	3.8 P. M.	S
	11	12.2 A. M.	N
	15	8.7 "	S
	19	5.2 P. M.	N
	24	1.5 A. M.	S
	28	10.2 "	N

OBERON.

		h	
June	3	8.3 A. M.	S
	10	1.9 "	N

OBERON CONT.

		h	
June	23	1.2 P. M.	N
	30	6.8 A. M.	S

Minima of Variable Stars of the Algol Type.

[Given to the nearest hour in Central Standard Time.]

U CEPHEI.

		h
June	2	12 M.
	4	12 midn.
	8	12 M.
	10	12 midn.
	13	11 A. M.
	15	11 P. M.
	18	11 A. M.
	20	11 P. M.
	23	11 A. M.
	25	11 P. M.
	28	10 A. M.
	30	10 P. M.

S CANCRI.

June	9	10 A. M.
	18	9 P. M.
	28	9 A. M.

δ LIBRÆ.

June	2	8 A. M.
	4	3 P. M.
	6	11 "
	9	7 A. M.
	11	3 P. M.
	13	11 "
	16	7 A. M.
	18	2 P. M.
	20	10 "
	23	6 A. M.
	25	2 P. M.
	27	10 "
	30	6 A. M.

U CORONÆ.

June	1	7 P. M.
	5	6 A. M.
	8	5 P. M.
	12	4 A. M.
	15	3 P. M.
	19	2 A. M.
	22	12 M.
	25	11 P. M.
	29	10 A. M.

U OPHIUCHI.

June	1	12 M.
	2	8 A. M.

U OPHIUCHI CONT.

		h
June	3	4 A. M.
	3	12 midn.
	4	8 P. M.
	5	4 "
	6	12 M.
	7	8 A. M.
	8	5 "
	9	1 "
	9	9 P. M.
	10	5 "
	11	1 "
	12	9 A. M.
	13	5 "
	14	1 "
	14	10 P. M.
	15	6 "
	16	2 "
	17	10 A. M.
	18	6 "
	19	2 "
	19	10 P. M.
	20	7 "
	21	3 "
	22	11 A. M.
	23	7 "
	24	3 "
	24	11 P. M.
	25	7 "
	26	4 "
	27	12 M.
	28	8 A. M.
	29	4 "
	29	12 midn.
	30	8 P. M.

Y CYGNI.

June	1	12 midn.
	3	12 M.
	4	12 midn.
	6	11 A. M.
	7	11 P. M.
	9	11 A. M.
	10	11 P. M.
	12	11 A. M.
	13	11 P. M.

Y CYGNI CONT.

		h
June	15	11 A. M.
	16	11 P. M.
	18	11 A. M.
	19	11 P. M.
	21	11 A. M.
	22	11 P. M.
	24	11 A. M.
	25	11 P. M.
	27	11 A. M.
	28	11 P. M.
	30	11 A. M.

S ANTLIÆ.

(Every third minimum.)

June	1	11 A. M.
	2	10 "
	3	9 "
	4	9 "
	5	8 "
	6	7 "
	7	7 "
	8	6 "
	9	5 "
	10	5 "
	11	4 "
	12	3 "
	13	3 "
	14	2 "
	15	1 "
	16	1 "
	17	12 midn.
	17	11 P. M.
	18	11 "
	19	10 "
	20	9 "
	21	9 "
	22	8 "
	23	7 "
	24	7 "
	25	6 "
	26	5 "
	27	5 "
	28	4 "
	29	3 "
	30	3 "

Phases and Aspects of the Moon.

	Central Time.		
		d	h m
New Moon	June	[illegible]	4 56 P. M.
Perigee	"	4	11 40 P. M.
First Quarter	"	10	7 14 A. M.
Full Moon	"	18	1 06 A. M.
Apogee	"	20	4 50 A. M.
Last Quarter	"	[illegible]	4 03 A. M.

COMET NOTES.

Discovery of a New Comet (a 1894, Denning).—A telegram from Mr. John Ritchie, Jr., Boston, March 28, announced the discovery of a faint comet by Mr. W. F. Denning of Bristol, England, the following being the discovery position

March 26.396 Gr. M. T. R. A. $9^h\ 55^m$, Decl. $+32°\ 15'$.

The comet was observed at Northfield on the evening of March 28, and found to be a very small object, very difficult to see in the 5-inch finder, but easily seen and measured with the 16-inch telescope. It had a well defined nucleus of the 11th magnitude, with nebulosity surrounding it between 1′ and 2′ in diameter. It had a short, slightly spreading tail, 2′ or 3′ long. From our own observations on the dates March 28, April 1, and April 5, we have computed the following parabolic elements of the comet's orbit.

	Time of perihelion	= 1894 Feb. 14 1900 Greenwich mean time.	
π =	Longitude of perihelion	= 133° 26′ 15″	Mean equinox 1894.0
Ω =	Longitude of node	= 75 34 12	
i =	Inclination	= 6 31 00	
q =	Perihelion distance	= 1.22497.	

These elements do not represent the middle place, the observation of April 1, with a sufficient degree of accuracy, the outstanding residuals being + 8″ in longitude and + 26″ in latitude. These residuals cannot be reduced on any assumption of a parabolic orbit, and it may be presumed that the orbit will turn out to be an ellipse of comparatively short period.

As the comet is growing rapidly fainter it is not probable that those using small telescopes will see it. We therefore give the following ephemeris only to indicate approximately the course of the comet and its increasing distance from us. Its path among the stars during April and May, is shown upon Poole Bros' Map in this number.

EPHEMERIS OF COMET a 1894

		R. A.			Decl.		log Δ	log r	Br.
		h	m	s	°	′			
Mar.	28	10	02	24	+ 31	01	9.7080	0.1421	1.00
Apr.	[illegible]	10	28	36	26	44	9.7032	0.1605	0.71
	13	10	50	16	22	44	9.8201	0.1767	0.50
	21	11	08	25	19	05	9.8778	0.1969	0.35
	29	11	24	36	15	47	9.9345	0.2201	0.25
May	7	11	39	08	12	48	9.9805	0.2403	0.17
	15	11	52	21	10	07	0.0428	0.2601	0.12
	23	12	05	04	7	40	0.0933	0.2797	0.09
	31	12	16	08	+ 5	33	0.1385	0.2995	0.07

A New Comet Discovered in Australia.—A telegram from Mr. John Ritchie April 6 announced the discovery of a comet by Mr. Gale at Sydney, Australia. The discovery position was:

April 2.944 Gr. M. T. R. A. $2^h\ 30^m\ 48^s$, Decl. $-55°\ 35'$.

The motion is easterly. The comet is described as round with a bright condensation. This comet is too far south to be visible at any of the northern Observatories.

A Comet's Tail Discovered by Holmes.—Another telegram received April 11, announces the discovery of a bright comet's tail by Holmes April 9. Its approximate R. A. and Decl. were $17^h\ 58^m$ and $71° + 30'$.

Ephemeris of Tempel's Second Periodic Comet (1873 II).—Mr. L. Schulhof gives an ephemeris of this comet, in *Astronomische Nachrichten*, No. 3219, for the month May 19 to June 16. The elements used are as follows:

Epoch and osculation: 1894, April 25.0 Paris mean time.

$$M = 0^\circ\ 15'\ 27'' \qquad \varphi = 36^\circ\ 26'\ 34''$$

$$\left.\begin{aligned} \pi &= 306\ \ 14\ \ 22 \\ \Omega &= 121\ \ 10\ \ 02 \\ i &= 12\ \ 44\ \ 20 \end{aligned}\right\}\ 1894.0 \qquad \begin{aligned} \mu &= 679''.860 \\ \log a &= 0.478392 \end{aligned}$$

At the time of the last observation of this comet in 1878 its brightness was somewhat less than that which it should be theoretically during this month. It is therefore to be hoped that the comet will be found at this apparition.

EPHEMERIS.

		R. A.			Decl.		log Δ	$1 : r^2\Delta^2$
		h	m	s	°	′		
May	19	0	18	39	− 2	48.6	0.22119	0.190
	20		21	43	2	37.5		
	21		24	46	2	26.6	0.22049	
	22		27	48	2	15.7		
	23		30	50	2	04.9	0.21982	0.188
	24		33	50	1	54.2		
	25		36	50	1	43.6	0.21917	
	26		39	49	1	33.1		
	27		42	47	1	22.7	0.21853	0.186
	28		45	44	1	12.4		
	29		48	40	1	02.3	0.21791	
	30		51	35	0	52.3		
	31		54	30	0	42.3	0.21730	0.184
June	1	0	57	24	0	32.5		
	2	1	00	16	0	22.8	0.21670	
	3	1	03	07	0	13.3		
	4		05	58	− 0	03.9	0.21611	0.182
	5		08	48	+ 0	05.4		
	6		11	37	0	14.5	0.21552	
	7		14	25	0	23.5		
	8		17	11	0	32.3	0.21492	0.179
	9		19	56	0	41.0		
	10		22	41	0	49.6	0.21432	
	11		25	25	0	58.0		
	12		28	08	1	06.3	0.21371	0.177
	13		30	50	1	14.4		
	14		33	31	1	22.4	0.21309	
	15		36	11	1	30.2		
	16	1	38	49	+ 1	37.9	0.21246	0.174

Mr. Schulhof says that the uncertainty of the time of perihelion passage cannot be more than ± 2 days. Should this occur two days early the R. A. of the comet would be increased $3^m\ 35^s$ May 15 and $2^m\ 55^s$ June 16, and the declination would be increased 10′ May 15 and 11′ June 16. If the perihelion passage should be 2 days later than calculated the R. A. would be decreased $3^m\ 43^s$ May 15 and and $3^m\ 02^s$ June 16, and the Decl. decreased 10′ May 15 and 11′ June 16. The observer searching for the comet may need to sweep over a space 20′ wide, extending 1° each way, east and west, from the predicted place of the comet.

Comet *b* 1894 (Gale).—From Science Observer Circular No. 105, we take the following:—A later message received April 15, contained the elements as given below, which were computed by Kreutz. From these an ephemeris has been computed by the Rev. G. M. Searle, which is given below.

ELEMENTS.

$$
\left.\begin{array}{l}
T = 1894 \text{ April } 13.82 \text{ Greenwich M. T.} \\
\omega = 324^\circ \ 18' \\
\Omega = 206 \quad 9 \\
i = \ 87 \quad 24 \\
q = .9856
\end{array}\right.
\quad \text{(} \omega, \Omega, i \text{: Mean Eq. 1894.0)}
$$

EPHEMERIS FOR GREENWICH MIDNIGHT.

	R. A.			Decl.		Light.
	h	m	s	°		
April 28	7	14	40	− 27	35	5.47
May 2	8	[illegible]	[illegible]	− 11	36	
6	8	47	32	+ 2	44	
10	9	21	0	+ 15	15	3.21

Light, April 2 = 1.

The object announced as a new, bright comet, by Holmes, in the position, R. A. $17^h\ 58^m$, Decl. + 71° 30′, proves not to have been a comet.

The comet discovered by Denning on March 26 was observed here the next night, March 27, 8 hours 75 Meridian time, in approximate position, R. A. $9^h\ 58^m$ + 31° 29′. It was an easy object in the 10-inch retractor, having a very small but sharply defined nucleus, so close to the edge of the nebulosity that it was somewhat doubtful at first sight whether it belonged to the comet or was a star. A few minutes watching showed that it was a part of the comet. From the stellar nucleus extended a short, broad fan shaped tail. Many fruitless searches were made here for the bright comet reported to be discovered by Holmes, of England on April 9. As neither direction or rate of motion were given in the announcement, a large region of the sky in the place indicated was swept over until it became evident that some mistake had been made by Mr. Holmes, and this, I have just learned, was really the case. This experience emphasises once more the importance of *always ascertaining motion* beyond the possibility of a doubt before making a public announcement. If this is not possible at discovery, then the suspected comet should be telegraphed to some leading observatory, preferably to Harvard by American observers, with instruction to with hold the public announcement until verified by the observation of motion, either at the observatory so notified, or by the discoverer himself. This simple precaution would often save the very considerable expenditure of a world wide announcement, and what is often of far greater importance, the waste of many hours of valuable time. To all of us, at times, a *clear night* is above riches.

WILLIAM R. BROOKS.

Smith Observatory, Geneva, N. Y., April 18, 1894.

Photograph of the Pleiades.—Plate No. XI gives a reproduction of the central part of the photograph referred to on page 192, March No. of ASTRONOMY AND ASTRO PHYSICS.

The finer details of the nebula and many of the fainter stars are necessarily lost in reproduction by means of the screen, but if the reader will hold the picture about two feet from the eye, the meshes of the screen will be lost and the effect of the photograph in a measure realized.

One of the two asteroid trails, that of (203) Pompeja, is shown near the right edge of the plate about three fourths of an inch from the bottom. The other trail, of 1894 AW, was on the upper part of the plate which has been cut off in reproduction.

NEWS AND NOTES.

In No. 110 of the Johns Hopkins University circulars will be found an address by Maurice Bloomfield on "A century of comparative Philology." In that address it is claimed that a Hindu by the name of Tilak has very recently made important discoveries concerning early date, of Vedic literature. It is also stated that Tilak's discovery is based upon astronomical data recorded in the Vedic literature.

Nautical Office Investigation.—In Vol. XII, pp. 664 and 760 of this publication, some notice was given of the investigation of the Nautical Office at Washington, D. C., while the same was in progress. Inasmuch as this was a public investigation, it was expected that some public notice of the findings of the court of inquiry would be given that persons interested might know the result. Upon inquiry after the report of the investigation had been sent to the Secretary of the Navy, it was learned that the results of the investigation were not for publication. We then wrote directly to the Secretary of the Navy for information concerning the results for publication, and the reply substantially was, that there was no matter for publication such as was asked. This was nearly three months after the report was in his hands and yet one of the parties in the investigation claimed he did not know what the decision in the case was. It may be that he had not used the proper means to ascertain the result, but one would suppose that he would do so from self-interest, and he claimed that he had sought this information from reasonable sources without success. Writing to Professor Newcomb later about the case he kindly placed in our hands a copy of the Secretary's letter, which, by our request, he permits us to publish. It throws some light on the results reached. It fully exonerates Professor Newcomb. It is as follows:

(COPY).

NAVY DEPARTMENT, WASHINGTON, Nov. 3, 1893.

PROFESSOR SIMON NEWCOMB,

Superintendent Nautical Almanac Office, Washington, D. C.

SIR:—I have carefully examined for myself the report made by Captain McNair, the memoranda submitted by the Judge Advocate General of the Navy, and the testimony taken in the matter of the charges preferred against you by Dr. J. R. Morrison, and take pleasure in saying to you that there is no evidence whatever to show that you were guilty of the charge preferred by Dr. Morrison of improperly using employees under you to do private work for you during office hours. The evidence exonerates you from the charge.

I think, however, that it appears from the testimony in this case that it would be of advantage to the Nautical Almanac to have the benefit of your continuous superintendence of the work of that office and that it would be better to revoke the leave of absence heretofore granted to you to attend the Johns Hopkins University as a lecturer two days in the week, durings its sessions. The department does not, however, impute to you any blame for such absence. You were permitted to assume the duties of a professor at the Johns Hopkins University with full knowledge on the part of the Department that it would necessitate your absence from your duties as Superintendent of the Nautical Almanac, and the order which has been issued revoking such leave to you from and after the 1st.

of January next, is simply carrying out the policy which has been recently applied in other cases, and which was determined upon after full and careful consideration.

Taking great pleasure in thus exonerating you from all personal blame in this matter, I am, Very respectfully yours,

H. A. HERBERT,

Secretary of the Navy.

This investigation has affected both parties to it seriously, and we can not imagine why it has been deemed best to withhold from the public the results of this public investigation.

The United States Coast Survey Office.—Under date of March 12 the New York *Evening Post* contains a column devoted to the present "peril" of the United States Coast Survey office, as now constituted and managed. The blow at it is said to be aimed through a provision in the sundry civil appropriation bill which provides for a transfer of the Coast Survey office from the Treasury department to the Navy department. The writer of the article speaks in complimentary way of its present chief officer, Professor T. C. Mendenhall who is well and favorably known, and so far he is right. Whether his grave charges of political scheming against those who are now seeking a change are true or not, we do not know. However this may be, the writer before referred to gives an interesting bit of history regarding the changes in administration at the Survey office. We extract the following:—

"The first law establishing the survey was passed in 1807, and the Treasury department was given custody of it by President Jefferson. The interruptions caused by the war of 1812 with Great Britain were followed by a transfer of jurisdiction to the Navy department in 1818, and this connection continued till 1832, when Congress passed a law enlarging somewhat the scope of the survey, which went back to its old place under the Treasury. This change was undoubtedly largely influenced by the strong recommendation of Samuel L. Southard, who, as secretary of the Navy in 1828, repeatedly declared that the work had not progressed as it ought under his department, and that the charts prepared were disconnected and untrustworthy, and the operations as a whole "very expensive in proportion to their usefulness."

Hardly had the new order been in force two years, when President Jackson caused the transfer of the survey once more to the Navy department, of which Levi Woodbury was then secretary. For two years this experiment lasted. Mr Woodbury meantime had taken the Treasury portfolio, and by 1836 both the President and Mr. Woodbury had become so well satisfied of their error that they restored the survey to the Treasury, where it remained undisturbed till 1843. Then Congress passed a law in connection with the annual appropriations, requiring that the President should appoint a board consisting of civilians, naval officers, and army engineers, submit the question of the organization of the survey to them, and organize it on the plan approved by a majority of the board. This board, though composed of only three civilians and six commissioned officers of the army and navy, recommended that the survey "should be under the control and considered a part of the Treasury department." This recommendation was adopted, and was generally accepted as satisfactory till 1848, when another attempt was made in Congress to transfer the survey to the Navy department, but was promptly quashed by a vote in the house of 90 against, to 36 in favor of

the scheme, and in the Senate, some months later, when the advocates of the transfer could muster only 2 votes. In 1851, the project having been revived, it was given its death blow by a report of Secretary Corwin of the Treasury, answering a Senate resolution of inquiry, in which he reviewed the subject historically and convinced the doubtful members of Congress that it would be best to let things stand as they were.

During the civil war the services rendered by the civilian survey were repeatedly recognized, but most strikingly by Commodore D. B. Porter, who not only tells how he was furnished with accurate charts on which to plan his water campaigns in the South, but describes in one of his letters his dependence on the personal presence of two of the civilian members of the survey who were helping him on board of his flagship, declaring: "I cannot speak too highly of these gentlemen. I assure you that I shall never undertake a bombardment unless I have them at my side."

Another attempt was made to dislodge the survey in 1884, but was given up because the joint commission from both houses of Congress that looked into the matter satisfied themselves that no change would be of benefit to public interests. The reasons against a transfer are even stronger now, since the rehabilitation of our navy has provided employment for so many more officers educated for naval service. It is extremely difficult to-day for the navy to spare the officers actually needed in the survey work under its existing organization; only about half the number formerly detailed to the survey are available at present."

Meridian Circle Work at Washington.—In a letter dated March 26, 1894, Mr. A. N. Skinner first assistant astronomer of the U. S. Naval Observatory, states that he is actively engaged in the observation of the "*Astronomische Gesellschaft*" zone − 13° 50′ to − 18° 10′. He has made since he commenced, some two months since, more than 2,000 observations. The zone contains 8,689 stars, each of which must be observed twice, once in each position of the instrument. He is assisted by Messrs. Littell and King.

The New York Academy of Sciences. Stated Meeting, March 26th, 1894.—The meeting organized with President J. K. Rees in the chair.

A paper by O. F. and A. C. Cook, entitled "A Monograph of Scyonotus," was read by title and referred to the Publication Committee.

The Section of Astronomy and Physics then organized with Professor Rees as Chairman and Professor Hallock as Secretary *pro tempore*.

In the absence of Mr. Jacoby his paper on 61 Cygni was read by Professor Rees. This paper has been published recently in full in the *Monthly Notices* (No. 2 Vol. LIV., Dec., 1893) of the Royal Astronomical Society of London. Professor Rees called attention to the importance of all observations showing changes in the relative positions of the components of 61 Cygni. He remarked on the present uncertainty as to whether the pair formed a true binary system or not. Calculations had given such differing orbits as are indicated by periods of 1159 and 462 years. The observations of S. W. Burnham lead him to conclude that the members of the pair are separating, and Professor Hall, from his observations extending over a period of 12 years, favors the view of a physical connection of these stars (Astr. Journal, No. 258, page 140).

Professor Hallock read a paper on a method of defining standard colors, and showed many samples of colors and the five discs of standard color used. The methods of defining standard color with them were illustrated.

Professor Rees remarked upon the importance of the work.

Dr. See, of the University of Chicago, then discussed the "Origin of the Heavenly Bodies," illustrated with lantern slides. He believes that the double stars originated by the swinging apart of one nebula through a process of splitting.

The paper was discussed by President Rees and others.

Publications of the Lick Observatory, Vol. II, 1894.—Double Star measures made with the 36-inch and the 12-inch Clark Refractor of the Lick Observatory in 1889, 1890 and 1891, by S. W. Burnham.

This publication is an interesting volume and prepared in excellent way. The micrometrical measures of the double stars are given individually accompanied by important notes from various sources pertaining to many of them and illustrations of special features of some give added value to the data. The large, clear, page and the excellent printing are in keeping with the character of the work and make the volume a very desirable one for the astronomer's library.

The frontispiece is a fine photogravure plate of the micrometer of the 36-inch refractor. The kinds of work done by Mr. Burnham and published in this catalogue may be given under the following heads:—

Measures of stars noted as double in Krueger's Catalogue of the Astronomical zone 55° to 65°.

Zusatz by Professor Krueger.

Measures of stars noted as double in the *Astronomische Gesellschaft*, zone 65° to 70°.

New Nebulæ.

Measures of Planetary Nebulæ. Observations of Nebulæ with the 36-inch refractor of the Lick Observatory.

New Double stars discovered at the Lick Observatory in the years 1888 to 1892.

Fourteenth Catalogue of new double stars discovered at the Lick Observatory.

Fifteenth Catalogue of new double stars discovered with the 36 inch refractor of the Lick Observatory.

Sixteenth Catalogue of new double stars discovered at the Lick Observatory in May, June, July, 1889.

Seventeenth Catalogue of new double stars discovered in 1890 with the 36-inchequatorial of the Lick Observatory

Eighteenth and Nineteenth Catalogues of new double stars. Additional notes.

List of the Catalogues of new double stars discovered by S. W. Burnham.

Works issued by the Lick Observatory.

The additional notes cover eighteen pages and form a very useful part of the publication because the matter is to date and the illustrations of orbits are numerous and deduced by modern approved methods.

Chandler's Criticisms of the Harvard Photometric Observations.—Last month we called attention briefly to criticisms made by Dr. Chandler in *A. N.* 3214, in regard to the Harvard photometric observations. We then thought and still do that his criticisms are not without bias. This sentence or two from the first paragraph is a sample. He says: "To be more specific, a list of some of these contradictions is here given. It is by no means complete, but comprises perhaps the more important results of a somewhat desultory examination, which was carried far enough to justify, as it seemed to me, an impression of distrust whether any of these observations are suitable for any precise or critical purpose."

His "desultory examination" is sufficient to justify a distrust in "any of these observations" . . . "for any precise or critical purpose. Is this fair and friendly criticism? We do not think it is at all "legitimate" criticism, if by that word is meant fair and friendly dealing with the work of another. We do not question Dr. Chandler's facts, but we do condemn his broad inferences.

The Peters-Borst Star Catalogue.—About 1889, the manuscript of a catalogue of 35,000 stars was completed at Litchfield Observatory of Hamilton College and made ready for publication. The observations and reductions were made in the main by Charles A. Borst, then assistant to Dr. C. H. F. Peters, director of Litchfield Observatory, who, it was claimed, planned the work and directed it in the beginning. When the manuscript was completed Mr. Borst refused to give it up, claiming it as his own. The matter was taken to court and tried before Judge Williams in Utica in the spring of 1889, and it was decided that the catalogue belonged to Dr. Peters. This trial attracted wide attention in consequence of the scientific features in the case and the prominence of the witnesses called to testify. Since the death of Dr. Peters which occurred in 1890, the contest has been maintained by Hon. Elihu Root of New York as administrator in the appeal made by Mr. Borst to the Court of Appeals of New York. From the Utica *Herald*, April 14, we learn that the Court of Appeals hand down a decision reversing that rendered in 1889 by Judge Williams and ordering a new trial.

While neither of the parties claimed that the star catalogue had commercial valne, it was shown by the testimony of Professors Hall of Washington, and Boss of Albany, that the cost of work was not less than $12,000. For a statement of the case in important particulars, see Vol. VIII *Sidereal Messenger*, pp. 138, 455.

A Brilliant Meteor.—On April 10th at 8:35 P. M. a very brilliant meteor of exquisite beauty, moving possibly at the rate of 10 or 12 miles a second, spanned an arc of the heavens east to north. It entered onr atmosphere between the stars ϵ and ζ Herculis, sailing in the direction of γ Ursæ Minoris. It disappeared near the star θ Draconis. Its duration did not exceed 7 seconds of time. Its color was of a brilliant cerulean blue, followed by a short trail, orange and purple in appearance. Its shape was elliptical, the major axis in the line of motion seemed to be fully 40 minutes of an arc, somewhat larger than the diameter of the Moon, whereas the minor axis did not exceed 15 minutes. These figures may be somewhat exaggerated; as a brilliant object projected suddenly on a dark background by an optical illusion, appears invariably much larger than its real dimensions. This meteor did dot explode, which shows that it did not belong to the family of bolides. The star-gazers of Belize, who were happy enough to catch a glance of this large meteor must have realized that they were the witnessers of a very rare and beautiful phenomenon.

C. M. CHARROPPIN, S. J.

Corozal, British Hinduras, Central America, April 18, 1894.

While observing recently with the 6-inch Grubb equatorial of this Observatory, the writer picked up a double star in the constellation of Canis Major, which appears to be new. In the hope that it may prove a new binary system, the writer measured the star as carefully as circumstances would allow on two nights with our Grubb position micrometer. Owing to inexperience in this sort of work, the faintness of the star in so small a telescope, and the poorness of the driving clock, these measurements leave much to desire.

March 28, 1894 — 5″.18 — 277 .5
March 31, 1894 — 5 .25 — 276 .0
Power = 150.

The magnitues are, possibly, 9.5 and 9.4.

The well known double star Lalande 14292, precedes this star by 46 seconds, and is 10′ south of it. The position of Lalande 14292 for 1880, as taken from Vol. 4 of the Publications of the Cincinnati Observatory is:

$$\text{R. A.} = \quad 17^h\ 14^m\ 12^s$$
$$\delta = -21°\ 50'$$

This gives the new double an R. A. of $7^h\ 14^m\ 58^s$ and δ of $-21°\ 50'$ for the same year.

ROGER SPRAGUE.

Chamberlain Observatory,
Denver, April 2, 1894.

From Canada.—Mr. John A. Paterson, M. A., Vice-President, presided over the meeting of March 20, 1894, the Astronomical and Physical Society of Toronto. Interesting letters were read from M. Paul Henry of the National Observatory, Paris, France, Mr. E. W. Maunder, F. R. A. S., of Greenwich Observatory, Professor W. H. Pickering of Arequipa, Peru, Professor S. W. Burnham, of Chicago, Professor W. W. Payne, Editor of ASTRONOMY AND ASTRO-PHYSICS, Lieutenant A. G. Winterhalter, U. S. N., Mr. Rudkins, Secretary of The Mathematical and Physical Society of the University of Toronto and others, several of whom wrote in terms of high praise of the last annual Report just issued by the Astronomical Society.

A report upon the subject of using contracted apertures, by stopping down telescopes by inserting a card-board disc, covering the central part of the object glass, was sent in by Dr. J. C. Donaldson, of Fergus, who had found that the defining power of a first-class instrument did not suffer.

The Chairman read a series of extracts from a somewhat remarkable article by Sir Robert Ball, recently printed in *The Fortnightly Review*, entitled "Significance of Carbon in the universe."

Several instructive and entertaining papers on Magnetism were read by Mr. G. G. Pursey, Mr. J. A. Collins and Mr. Thomas Lindsay. Experiments were performed by Mr. Collins, also by Mr. A. Aronsberg, who brought with him a powerful magneto-machine.

Meeting of April 17: Chair occupied by Mr. John A. Paterson, M. A., Vice President.

Communications included letters from Miss Agnes Clerke, of London and from the Royal Society of England asking the Society to consider the advisability of assisting in establishing a central office, or bureau, to be sustained by international contributions, and to be charged with the duty of compiling a complete catalogue containing titles of scientific publications, whether appearing in periodicals or independently, the titles to be arranged not only according to authors' names, but also according to subject matter. If sufficient support be given, the new catalogue will commence with the year 1900. Through Mr. Charles Carpmael, F. R. A. S., an appeal was presented from the University of Cambridge, addressed to friends of the University and to all interested in Astronomical Science, for assistance to raise $11,000 to complete the celestial photographing equipment of the University Observatory.

Under observations, Mr. Thomas Lindsay read the first report of work intended to be done with the Wilson telescope, Mr. G. G. Pursey handed in a number of drawings of the solar disc, with spots, obtained very satisfactorily by projection, and Mr. J. A. Copland contributed detailed description of a recent aurora.

Consideration of "magnetism" was resumed, a series of short papers being

read. Mr. Lindsay, was introductory in character, and in a popular style. Mr. W. Barlour Musson's dealt chiefly with the experiments conducted many years ago and related by Baron Reichenbach. Mr. A. Elvin's, was illustrated by a number of unique and clever experiments, at which Mr. J. R. Collins assisted.

To finish, there was an address by Mr. A. J. McDonald, inventor of The McDonald Tellurian, an extremely simple and inexpensive device for explaining astronomical motions.

The Chicago Academy of Sciences. *Section of Mathematics and Astronomy, April 9th.*—The regular monthly meeting was held at the Chicago Athenaeum; Professor G. W. Hough, President, in the chair. After the transaction of routine business, the Section proceeded to the program of the evening. Mr. A. C. Behr read the first paper on "*Observations of the Pleiades since the time of Ptolemy.*" The speaker discussed the observations of Hipparchus and Ptolemy as given in Baily's edition of the catalogue, and referred to the probable variability of η Tauri. Ulugh Beigh's rating of this star as 4th magnitude appears verified by a comparison of 31 3d magnitude and 54 4th magnitude stars in the catalogues of Hevelius, Halley and Tycho. A change of nearly two magnitudes in other stars of the group probably formed the basis of R. Wolf's opinion that some of the brighter stars are slowly variable. A chart by Mr. Peck, F. R. A. S., "The Pleiades as seen by Ptolemy, A. D , 137" was exhibited on the screen; seven stars are shown. 27 and 28 Tauri not included. Pickering's explanation of the absence of 28 Tauri by its bright line spectrum, would leave 27 Tauri still unaccounted for; while according to the legend it was 17 in the extreme opposite portion of the group which disappeared. Mr. Behr did not regard the chart as trustworthy. He referred to the bright line C in the spectrum of η Tauri recently discovered by Campbell, and also to Miss Maury's notes on the spectrum of 28 Tauri. He said that the drawings of Wolf, M. Hall, Temple, von Littrow, and the photographic plates of Common, Roberts and Henry show no nebulosity near 27 and 28 Tauri; but nebulosity had been observed by Spitaler and others at Vienna. In regard to Jeauriat's observation in 1768, Ranyard had said "doubtful, no nebula drawn;" Jeauriat had only marked the Bessel stars 31 and 32 "nebuleuse."

This seemed to be worthy of attention, since 31 and 32 and the visible nebulosity are in the same region. Mr. Behr thought that Bessel's measures of the Pleiades and those recently made by Elkin would in time disclose the mechanism of the cluster.

A general discussion followed the reading of the paper. Professor Burnham did not think some of the observations mentioned were very trustworthy, but made some interesting remarks on Barnard's nebula near Merope, which he declared was a most difficult object to observe. Professor Hough made some remarks on the distribution of the nebulous matter in the Pleiades, and on the nebulosity of the heavens generally. Dr. T. J. J. See read the second paper of the evening, entitled "*A Visit to Some Eastern Observatories,*" which gave a sketch of the work he found in progress at the Observatories which he had recently visited while making a journey to Washington, Baltimore, New York and Boston. He reported that astronomical work was making good progress in the East, and said his journey had been a most agreeable one. After some further discussion the Section adjourned.

T. J. J. SEE,
Recorder.

Errata.—Page 311, line — 2, for 10940 read 20940. Page 312, line + 24, for These read Three. The last one is comparatively unimportant, the first one is. Possibly they were not plain in the MS.

PUBLISHER'S NOTICES.

The subscription price to ASTRONOMY AND ASTRO-PHYSICS in the United States and Canada is $4.00 per year in advance. For foreign countries it is £1 or 20.50 marks per year, in advance. Recent increase in price to foreign subscribers is due to increase of postage because of enlarged size during the year 1892. Messrs. Wesley & Son, 28 Essex Street, Strand, London, are authorized to receive subscriptions. Payment should be made in postal notes or orders or bank drafts.

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Manuscript for publication should be written on one side of the paper only and *special care should be taken to write proper names and all foreign names plainly*. All drawings for publication should be *smoothly* and *carefully* made, in *India ink* with lettering well done, because such figures are copied exactly by the process of engraving now used. If drawings are made about double the size intended for the printed page, better effect will be secured in engraving than if the copy is less in size. It is requested that manuscript in French or German be typewritten. If requested by the authors when articles are sent for publication, *twenty-five* reprint copies, in covers, will be furnished free of charge. A greater number of reprints of articles can be had if desired, at reasonable rates.

Rates for advertising and rates to news agents can be had on application to the publisher of this magazine.

CONTENTS FOR APRIL, 1894.

PLATE XIII.

GALE'S COMET, 1894, MAY 3.

$8^h\ 20^m$ to $10^h\ 35^m$ STANDARD PACIFIC TIME.

6-inch Portrait Lens. E. E. Barnard. Lick Observatory.

Astronomy and Astro-Physics.

VOL. XIII, No. 6. JUNE, 1894. WHOLE No. 126.

General Astronomy.

PHOTOGRAPHS OF GALE'S COMET.*

E. E. BARNARD.

This comet was discovered by Gale at Sydney, New South Wales, April 2.944 G. M. T. in

$$\alpha = 2^h\ 30^m\ 48^s;\ \delta = -55°\ 38'$$

A very rapid northeasterly motion brought it above our southern horizon in the latter half of April.

Unfavorable weather prevented its being seen here until Saturday night, April 28.

As this was a public night and the Observatory was crowded with visitors, no careful observation could be made of it. However, I got permission from the visitors to turn the 12-inch for a moment to its place and found the comet at once. It was large, round and condensed with no trace of tail to the hasty glance I necessarily gave to it. Looking along the tube, I saw the comet was easily visible to the naked eye as a hazy star of the 5th magnitude.

A quick pointing on it—afterwards corrected by a pointing on a star, in nearly the same position above the horizon, gave the following place at $7^h\ 51^m$ Mt. Hamilton mean time:

$$\alpha = 7^h\ 18^m.7$$
$$\delta = -26°\ 19'$$

The comet was certainly not a promising object for photography. On the night of April 29th, however, I gave an exposure of one hour upon it with the 6-inch Willard portrait lens, and the comet was shown to have a very slender thread-like tail over a degree long. On this date no trace of a tail could be seen in the 12-inch. No opportunity occurred to get another satisfactory photograph until May 2d when an exposure of one hour and fifteen minutes showed the tail distinctly for about four degrees. The sky was hazy.

On May 3d another photograph was secured with two hours

* Communicated by the author.

and fifteen minutes exposure upon which the tail was easily traceable eight or ten degrees. The sky was thick on this date also. In this case there was a tendency in the tail to split up into several strands.

On May 4 another photograph made through haze with two hours and five minutes exposure was very satisfactory though the condition of the sky almost forbade any attempt at photography.

Considerable changes had taken place in the appearance of the tail between the 3rd and the 4th, especially near the head. In the pictures previous to the 4th the slender tail retains its brightness up to its junction with the head. On the 4th however, the main tail is as long and slender as on the 3rd, but singularly enough it tapers down to a point and vanishes nearly one degree before reaching the head! There are however, two short tails nearly a degree long and at an angle of about 15° to each other that spring out from the head on each side of where the main tail should join. The northern one of these just narrowly misses a junction with the long tail, but passes free to the north of it. A good deal of diffused cometary matter appears between these two tails.

A fine photograph was obtained on May 5th* with two hours and thirty minutes exposure. This gives a rather complicated tail fully ten degrees long.

The tail leaves the head as a thin streak; at one degree distant it begins gradually to widen out, and a faint streamer runs close parallel with it to the north. At some 3° from the head a division occurs in the tail the south component being a very slender thread, while the northern component broadens and its northern edge is distinctly convex. While the southern side of the slender, thread-like tail near the head is well defined and free from minor tails or diffusion, the northern side has a faint and diffused brushing out from the head at a considerable angle to the direction of the tail itself.

Another photograph on the 8th with two hours and thirty minutes exposure partly in moonlight shows only the feeblest traces of tail. Though this can feebly be traced for upwards of 4° it is difficult to make much out of it. There seems to be several strands from the head, a very thin one of which divides ½° from the head and forms two faint strand-like tails running out for three or four degrees.

The peculiar characteristic of this comet is its slender tail and

* The glass positive was so badly broken in transit that it could not be reproduced.—*Ed.*

round head. There was no apparent development of the head as in comets usually possessed of a tail—that is, there was no apparent diffusion of the material of the head to form the tail.

I have forwarded two of these photographs for illustration and if they are successfully reproduced, I think it will be seen that photographically this is a very important and interesting comet.

To those not familiar with the subject, I would say that, as the comet was moving rapidly among the stars, the telescope was made to move accurately with it to keep the image stationary on the plate, this threw the motion on to the stars which are drawn out into lines of light, in length corresponding to the motion of the comet.

MT. HAMILTON, 1894, May 12.

THE FORMS OF THE DISCS OF JUPITER'S SATELLITES.*

WILLIAM H. PICKERING.

In his interesting paper in the April number of ASTRONOMY AND ASTRO-PHYSICS, Professor Barnard criticises my statement that the first satellite of Jupiter, even when not in transit, usually presents an elliptical disc. While I confess myself very much surprised that a phenomenon which appeared to us so obvious in Arequipa has not been confirmed by the 36-inch telescope, I can only say that the phenomenon there was very obvious, and was seen and correctly described by several persons who were entirely unfamiliar with the use of a telescope. That is to say, they would state correctly which one of the satellites was elongated and would describe correctly in words the direction in which the elongation took place. Upon six nights measures of the position angle of the elongation were secured by both Mr Douglass and myself. Each measure is the mean of six readings, taken alternately in opposite directions. These angles have not as yet been corrected for the constants of the instrument, but a comparison of the uncorrected angles will serve to show the extent of the agreement between the two observers. It will be seen that the position-angle of the elongation of the disc, as measured by Mr Douglass, uniformly exceeds the value as given by myself by several degrees. As the observations were nearly all made in one portion of the sky, with the observer's head consequently always in the same position, a personal correction of this sort due to a

* Communicated by the author.

slight astigmatism of the eye, is perhaps not surprising. The mean value of this constant difference amounts to 7°.1. In the following table the first column gives the date of the observation, the second the observed position-angle of the major axis of the disc, the third the average deviation of the six readings which combined give the individual measures, the fourth gives the initials of the observers, the fifth the differences between their results, and the sixth these differences corrected by the constant angle 7°.1.

DISC OF JUPITER'S FIRST SATELLITE.

Date.	P. A.	Dev.	Obs.	Diff.	Cor.
	°	°		°	°
1892, Nov. 28	100.5	3.7			
" "	108.3	8.7	P	+ 7.8	+ 0.7
Dec. 26	100.0	7.6	D		
" "	106.1	4.9	P	+ 6.1	− 1.0
" 29	103.8	5.0	D		
" "	116.8	3.2	P	+ 13.0	+ 5.9
1893, Jan. 1	90.0	6.0	D		
" "	95.7	3.4	P	+ 5.7	− 1.4
" 13	99.2	4.2	D		
" "	104.8	3.7	P	+ 5.6	− 1.5
" 17	92.4	3.8	P		
" "	97.9	3.2	D	+ 4.6	− 2.5
		± 4.8		+ 7.1	± 2.2

On account of the high power employed, the planet as a usual thing did not come into the field of view, and it is perhaps needless to add that precautions were taken to guard against all outside causes which might vitiate the accuracy of the observations.

not in transit, but which vanished at regular intervals of $6^h\ 32^m$. But apparently when not in transit, Professor Barnard sees nothing peculiar about this satellite, and it is this fact which chiefly surprises me. For even if we admitted a bright equatorial belt, as suggested by Professor Barnard in *Monthly Notices* for January, 1894, as the true explanation of the phenomena, that would not explain away the difficulty, that the 13-inch in Arequipa showed easily a peculiarity in the satellite when off the disc of the planet that the 36-inch could not detect. Moreover, the hypothesis of an equatorial belt does not explain why at regularly recurring intervals the disc of the satellite appeared to us to be circular. It must be understood that I do not deny the existence of the equatorial belt described by Professor Barnard, I merely state that such a belt could not have produced the effects observed by us in Arequipa. In this connection it may be stated that the figures published in ASTRONOMY AND ASTRO-PHYSICS for June, 1893, represent the discs of the satellites at maximum ellipticity, and that therefore the observer will usually find them much more nearly circular than they are there represented.

It is interesting to note that upon two occasions the disc of the fourth satellite appeared to Professor Barnard to be elongated in a north and south direction, just as it did to us in Arequipa. It is also satisfactory to note that our observations regarding the shape of the surface detail upon the third satellite, including the polar spot, were confirmed by him at this opposition, as is also true of our observations upon the polar spot of the fourth.

In conclusion, I may add that while I have not thought it worth while, on account of atmospheric conditions, even to attempt to verify my results by observations made in Cambridge, that I expect to devote considerable time to the matter at this next opposition, which I hope to be able to observe under unusually satisfactory conditions in Arizona.

HARVARD COLLEGE OBSERVATORY,
April 12, 1894.

A GRAPHICAL METHOD FOR DETERMINING THE APPARENT ORBITS OF BINARY STARS.*

CHARLES P. HOWARD.

The following graphical method for determining the apparent orbits of binary stars, is believed to have some novelty and for

* Communicated by the author.

many stars to possess some advantage over those ordinarily used, but on these points the writer speaks with diffidence, being well aware that as an amateur he is hardly sufficiently familiar with other methods to be qualified to make comparisons between them.

Assuming, that the best orbit to be obtained, is the one which while fulfilling the law of equal areas, brings the calculated and observed positions of the same date into the closest agreement; this method is an attempt to carry out this principle in the most thorough manner possible.

An outline of it has already been given in Gould's *Astronomical Journal* for Feb. 4, 1891, incidentally in working out the orbit of the companion of Sirius, but as the novel feature of the method, *viz.*, the process employed for plotting the position of the companion on the assumed ellipse, for the dates of the observed positions, was there only referred to but not described, it is here given in full, while the other steps are simply stated as concisely as possible.

Proceeding to plot the orbit in the manner described in the article above referred to,

1. Reduce the number of positions to be plotted, by taking the average date, position angle and distance, of the groups of observations that admit of such treatment.
2. Plot these positions to a suitable scale, say of one inch to the second of arc.
3. Mark each plotted position with its date.
4. Draw among them a trial ellipse to average all as nearly as possible.
5. Mark on the ellipse the position of the companion star at the two dates that an inspection of the observations indicate to be the most trustworthy. Usually the greater the interval between them the better.

Then because the companion conforms to the law of equal areas it follows that if its position on the ellipse is thus fixed for these two dates, it is necessarily fixed for all other dates, including of course the date of each plotted observed position. The problem now, is to find a quick and easy graphical method to plot on the trial ellipse the position of the companion on the date of each plotted observed position.

In nearly all binary stars, the observed position angles and distances are reliable to two or three significant figures only, while it is not difficult with ordinary care and skill to execute a drawing with such accuracy that all measurements taken from it

shall be reliable to three or four significant figures. Hence any artifice that will enable us to quickly locate upon the ellipse to this degree of accuracy, a series of points such that the radius vectors passing through them will divide the ellipse according to the law of equal areas, is one by all means to be taken advantage of. Moreover, no matter how rough or wanting in extreme precision it may at first sight appear to be, its capacity to fulfil the above condition is sufficient justification for its use and is the only criterion by which it should be judged.

The following is a description of the artifice used by the writer in obtaining the orbit above referred to, and while easy and rapid of execution (to locate a series of 30 positions on the trial ellipse for Sirius requiring less than an hour's work), its accuracy amply meets the requirements of the case and is fully equal to the other processes of drawing, the positions so determined being perfectly reliable to three or four significant figures.

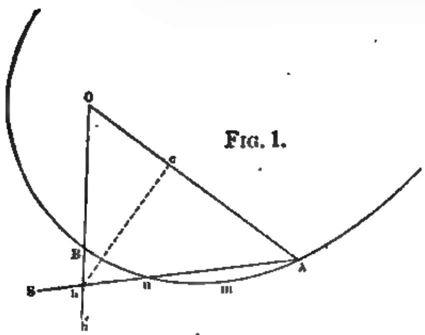


FIG. 1.

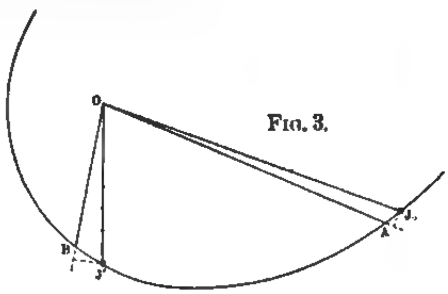


FIG. 3.

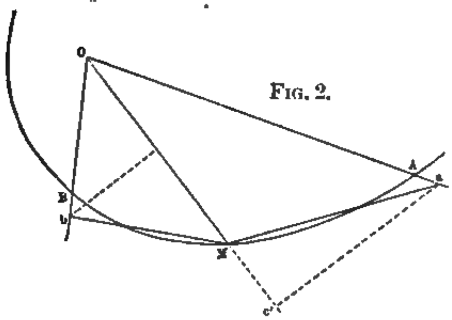


FIG. 2.

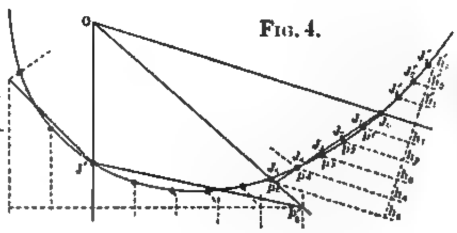


FIG. 4.

In Fig. 1, let O be the larger star, $AmnB$ the apparent orbit of the companion, as represented by the best trial ellipse that can be drawn among the plotted observed positions; A and B the two most trustworthy positions of the companion at fixed dates. The area of the elliptic sector $OAmnB$, on the scale of the drawing, is the area described by the apparent radius vector in the

interval of time between the two dates. Now this area is equal to the area of the triangle OAb, formed by intersecting the line OB, produced by the straight line As drawn so as to make the small internal area Amn equal to the small external area Bbn. The equality of these small areas is best adjusted by stretching from A to S a fine black thread and marking the point b on the line OB, produced when the areas are equal.

Although these small areas are dissimilar in shape, the error due to visually estimating their equality is remarkably small, partly due to the fact that any change in the direction of the line AS simultaneously increases one area and diminishes the other.

Having thus formed the triangle OAb; its area and therefore that of the elliptic sector can be measured on the drawing as equal to $1/2(AO \times bc)$; bc being the altitude, and OA the base, of the triangle.

When the points A and B are so far apart that b cannot be located with sufficient accuracy by the use of one triangle, two triangles can be used as in Fig. 2. These have a common base OM, and their combined area will be simply $1/2(bc^{*} + ac') \times OM$.

When A and B cannot be reached by two triangles, three or more can then be used; to decide this point requires the exercise of judgment, but the use of the check on the accuracy of the plotting to be described below, determines when the triangles have been stretched beyond their limit of accuracy.

By dividing the area of the sector AOB by the interval in years between the dates established for the points AB, the area described by the companion in one year is obtained; which yearly area denote by k.

We next wish to establish on the trial ellipse the place of the companion for the 1st day of January next preceding the dates fixed for the points A and B so as to be able to locate on the ellipse the position of the companion on the 1st of January for any series of years.

For the point A, Fig. 3, let y_1 be the fractional part of the date, and h_1 be the altitude of the triangle having OA for its base and $y_1 k$ for its area; then,

$$h_1 = \frac{2\, y_1 k}{OA}.$$

Hence if a line is drawn parallel to OA at a distance h_1 the point where it cuts the ellipse is the required position for January 1st, the curvature of the ellipse being usually insensible for so short an interval of time.

* The letter c, omitted by engraver, should be on line OM at foot of perpendicular. *Ed.*

Repeat the same process for the point B and we have established two positions for the companion on Jannary 1st of two definite years, *viz.*, those corresponding to the dates of A and B; call these positions respectively J_0 and J_0' in Figs. 3 and 4.

It is now an easy matter to plot the position of the companion on the trial ellipse for January 1st for a series of years.

In fig. 4, starting with OJ_0 as a common base, form a series of triangles on both sides of it, such that

the area of the	1st	triangle shall equal	k
"	2d	"	$2k$
"	3d	"	$3k$
"	4th	"	$4k$
	&c.		&c.

The altitude of the	1st	triangle will then be	$\frac{2k}{OJ_0} = h$
"	2d	"	$2h$
"	3d	"	$3h$
"	4th	"	$4h$
	&c.		&c.

On both sides of OJ_0 and parallel to it, draw a series of lines, h_1p_1, h_2p_2, h_3, p_3, h_4p_4, etc. at the distances h, $2h$, $3h$, $4h$, etc. These are to be drawn outside of the ellipse, to meet it as shown in the figure. Next lay a straight edge from O to near the point where the first parallel line touches the ellipse, and from J_0 stretch a thread to intersect the straight edge on the line h_1, p_1, swinging them about the points O and J_0 respectively, until while this condition is fulfilled, the thread gives equal areas inside and outside the arc; then draw the radius vector represented by the straight edge, and the point where it intersects the curve will be the position of the companion on January 1st, one year from the date of J_0.

Repeat this process for each of the parallel lines, on both sides of OJ_0 and we obtain as many points of the desired series on the ellipse as we have formed triangles.

Next take OJ_0' as the base of a similar series of triangles formed on both sides of it, and in the same way draw the radius vectors whose intersection with the ellipse locate the position of the companion for January 1st of each year.

Between J_0 and J_0' the two series of triangles will overlap, and one or more positions as J_3 can be located by both series. This is a valuable feature of this method, because it can be used to check the accuracy of the work of plotting the yearly positions up to this point. With attention to all the niceties of drawing it is surprising how exactly the same position can be located from

two bases, and this fact gives confidence in the reliability of positions so plotted.

The radius vector passing through the extreme positions thus located, can now be taken as new bases from which to start new series of triangles, and so on until the yearly positions are extended all the way around the ellipse and check with the periodic time as derived below.

After the place of the companion on the trial ellipse for January 1st of each year has been thus found, it is an easy matter to plot its place for the exact date of each plotted observed position; the fractional parts of the year being taken off the scale represented in fig. 5, in the usual manner, except that here too, judgment must be used in making an allowance for the continually varying length of the arc described in successive years.

Having thus obtained the series of positions on the trial ellipse corresponding to the dates of each plotted observed position, each observed position is joined to each calculated position of the same date by an arrow. These by their length and direction bring plainly to view the discrepancies between the two series of positions.

If such discrepancies are systematic and extend over several years, and, moreover, if their magnitude is greater than the estimated errors of the plotted observed positions, it is evidence that the trial ellipse is not the most exact that the observations are capable of yielding. The length and trend of the arrows also indicate the character of the modifications which must be made in the first trial ellipse to give a second ellipse that will more accurately represent the observations.

By repeating the above process several times an elliptic orbit will soon be obtained which gives the best average of all the plotted observed positions to be arrived at by this method.

The periodic time of the final ellipse is found as follows; measure on the drawing its major axis a_1, and its minor axis b_1, then representing the area described in one year by k_1, the periodic time is

$$\frac{0.7854 a_1 b_1}{k_1}.$$

ORBIT OF SIRIUS.

Possible Evidence of a More Distant and Smaller Companion

The orbit of Sirius is particularly well adapted to illustrate the advantages of the graphical method above described, and is therefore the one selected for the purpose. In Figs. 6, 7, 8 and 9, the po-

sitions plotted as small black circles are those given by Professor Burnham on p. 387 of the *Monthly Notices* of the Royal Astronomical Society, for April, 1891, with one exception, *viz.*, for the year 1869 the position 1869.21, 68.9, 11.06 was plotted (this being the average of the last three positions for that year). The points of the arrows rest on the calculated positions of the same dates.

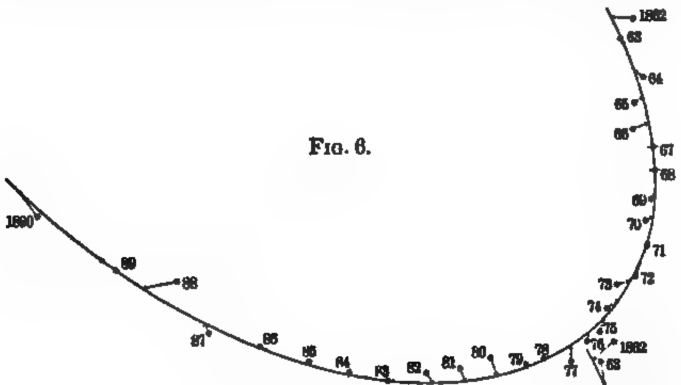


Fig. 6.

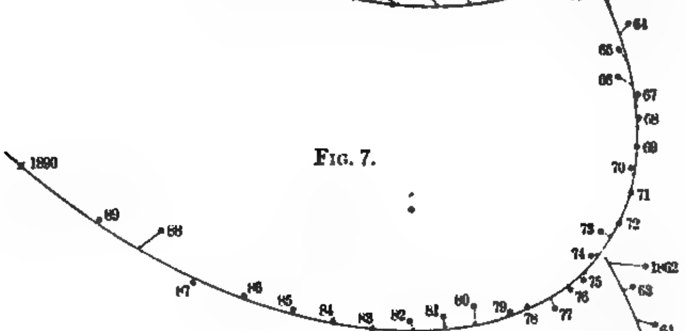


Fig. 7.

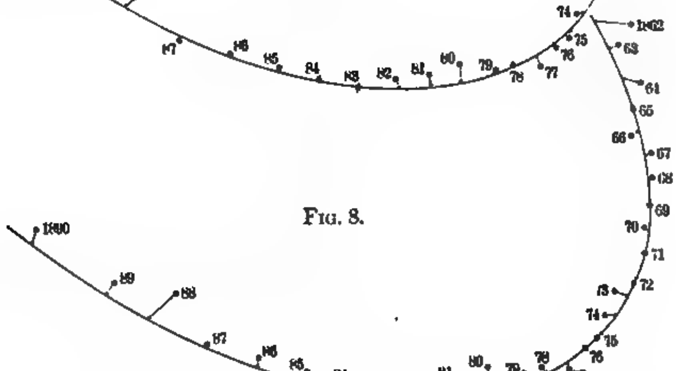


Fig. 8.

Fig. 6 shows the first trial ellipse adopted as giving the best average of all the plotted positions. This it does very satisfactorily, and if no further test could be applied, would be pronounced good. But when all the observed positions are joined

to all the calculated of the same date, it is at once apparent that it does not represent the observations as well as might be. Some of the arrows are longer than the accuracy of the observations would seem to justify, as for instance '89 and '90, and for many consecutive years ('64 to '78 and again '87 to '90), they point in the same direction. If the assumed positions of the companion on the ellipse for the same dates is advanced or carried back, which can easily be done, it does not help the matter, and evidently this is not the best orbit that the observations will yield. It gives a period of about 53 years.

Fig. 7 shows the second trial ellipse; and is better, the first having been used as a guide. From '83 to '90 the agreement of the two series of positions is remarkable and gives confidence in the accuracy of the plotted observed positions and also gives an indication of how closely the finally accepted orbit should agree with them. But between the years 1862 and 1871 the length, but particularly the direction of the arrows, shows a large systematic discrepancy, that also prevents this ellipse from being accepted as the true orbit. Its period is 55 years.

Fig. 8 shows the best ellipse that the writer could obtain, free from systematic discrepancies, in the direction of the companion's path, by continuing this process five or six times, and yet it can hardly be said to represent the observations better than the others. For here a new systematic discrepancy comes prominently to the front; from '62 to '69 all but one of the observed positions lie outside of the ellipse, while from '78 to '90 all without exception lie well within it. This is the peculiarity that gave the writer so much trouble in 1890 and at that time led him at first to suspect an advance of the periastron, but finally to adopt an elliptic orbit of longer period than usually assigned as the most probable explanation of it. The period of this ellipse is also 55 years.

In all the writer has tried more than 20 different ellipses without obtaining one that was thoroughly satisfactory, having to contend in every case with two persistent characteristic discrepancies, and whenever one of them was reduced the other immediately became prominent.

They were (1st,) in the assumed ellipse the motion was such that too great areas were invariably described between the years '62 and '69, and too small areas between the years '80 and '90; and (2d) all the observed positions between the former dates fell outside of the ellipse, and all those between the latter dates fell inside.

PLATE XIV.

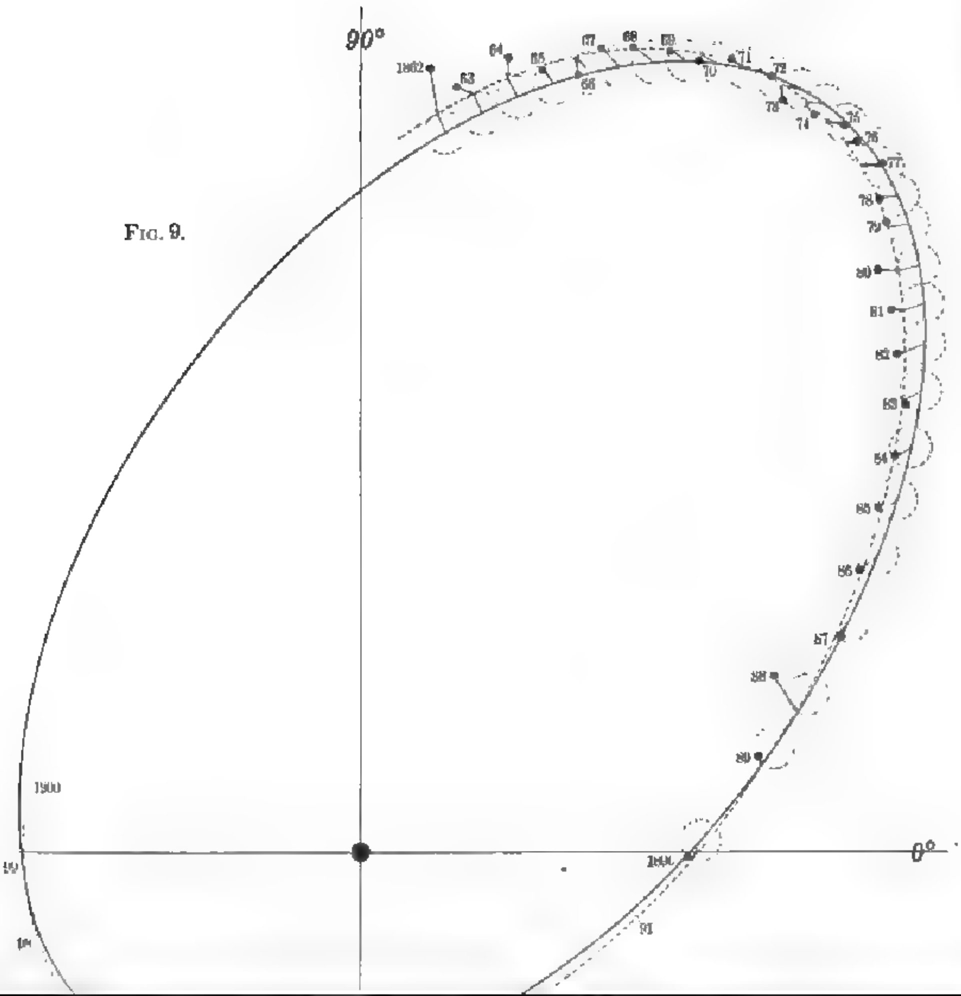


Fig. 9.

It seems difficult to believe that errors of observation alone can produce such symmetrical phenomena extending throughout all of the 29 yearly positions.

The only other explanation is that of perturbed motion caused by a third body in the system of Sirius.

Both the above characteristic peculiarities are satisfied by the assumption of an epicyclic path for the companion due to the revolution of either Sirius or its companion in a small orbit, with a period of about 100 years. By the method employed of plotting the observed positions of the companion, Sirius being the point from which the angles are laid off and the distances measured, any relative motion of Sirius itself in a nearly circular orbit would, on the drawing, appear to be transferred to the companion; changed however in phase by 180°, but with the direction of motion remaining the same.

After several trials on this supposition, the orbit shown in Fig 9 was the best obtained. The assumption is that the companion revolves with a period of about 100 years in a nearly circular apparent orbit of 0″.28 radius, as projected on the plane of the drawing. The direction of its apparent motion is contrary to the hands of a watch. The center of the small circular orbit describes around Sirius the ellipse shown, in accordance with the law of equal areas, in a period of 51 years.

This is by far the best orbit that the writer has yet succeeded in obtaining, and its superiority over the one shown in Fig. 8 is decided. The places for the years '65, '67, '68, '69, '70, '71, '78, '79, '83, '84, '85, '86, '89, '90, are practically in perfect agreement with the observations. If a mistake of 5 degrees has been made in the position angle for '88, this place also comes into close agreement with the observations. The small systematic discrepancy of about 1 degree in the position angle for the years '72, '73, '74, '75, is accidental, as a reference to the observations will show.

Thus the assumption of an epicyclic orbit satisfies the observed positions to a remarkable degree, even better than the trustworthiness of the observed positions would seem to warrant.

On the other hand the assumption of an elliptic orbit does not satisfy the observed positions to the extent that the trustworthiness of the observed positions would seem to warrant.

If epicyclic motion really exists it can be explained in two ways.

1st. The companion has a satellite. Roughly assuming the visible companion to revolve in an apparently circular orbit of 0″.28 radius about the center of gravity of the two bodies, in a

period of 100 years, the mass of the satellite would be one ninth that of the companion, or one ninetieth that of Sirius. Its mean distance would be 3″ and at the present time its position angle from the companion would be about 80°, which would bring its apparent position close to Sirius.

2nd. Sirius has another companion. Assuming that it is Sirius that revolves in the small orbit, the mass of the second companion would be one twentieth that of Sirius and its mean distance would be 6″.5, which would make it one and one half times as far away as the known companion. At the present time its position angle would be about 260°. Its mass being one half that of the known companion, if their surfaces were equally luminous it would appear about 0.6 as bright and therefore easily seen. Still when it is considered that mass for mass the known companion cannot be $\frac{1}{1400}$ as luminous as Sirius (Young), it is plain that if this ratio should increase for this smaller and more distant companion it might exist with such small luminosity as to put it beyond the reach of existing telescopes. Its position on Jan. 1st, '94, '95, and '96, is shown on the right in Fig. 9. This supposition seems the most probable of the two. The apparent retrograde motion in either case would be caused by a great inclination of the orbit.

The reappearance of the Companion of Sirius, which may be expected to occur in a year or two, will be awaited with great interest: but whatever proves to be the true explanation of the systematic anomalies inherent in the present observed positions, the writer believes that this example proves the advantage of treating double star orbits by this graphical method.

ELEMENTS OF SIRIUS.

Period in years	50.00
Periastron passage	1894.59
Inclination	49° 8′
Position of Node	44° 21′
Position of Periastron	144° 39′
Eccentricity	0.557
Semi axis major	7″.85

HARTFORD, CONN. April 27, 1894.

THE VARIABLE PROPER MOTION OF PROCYON.

S. W. BURNHAM

The micrometrical measures of double stars by Otto Struve, recently published in Volume X of the Poulkowa Observations,

* Communicated by the author.

include a long, continuous series of measures of the difference in declination of Procyon and two adjacent stars, commencing in 1851 and continuing each year, with few exceptions, down to 1890. These observations were made for the purpose of determining the amount, direction and character of the proper motion of Procyon, and particularly with reference to the question of the variability of that motion. It was long ago pointed out from meridian observations that Procyon had an irregular, sinuous motion in space similar to that of Sirius; and it was therefore inferred that it had a physical companion, the two revolving about their common centre of gravity, with a period of about forty years. Every effort has been made in the last twenty or more years to discover this supposed disturbing body. A number of imaginary stars have been recorded by different observers from time to time, but it is now conceded that none of these alleged companions had any real existence, and that if any such disturbing body exists at all, it has never been seen by the most powerful telescopes in use. On many nights during three or four successive years, I examined Procyon with the 36-inch at Mt. Hamilton under the most favorable circumstances, but always without seeing the least trace of any near star. It is probable that Procyon has been watched with the Poulkowa 30-inch for a much longer time, and presumably with the same negative result.

The companion stars selected by Otto Struve are: (B) 23ᵗ.3 preceding, and 115″ north; and (C) 23ᵗ.4 following, and 67″ north. These stars are respectively D.M. (5°) 1738 (9.0) and 1741 (8.8). The star C (= W′ VII .990) is well known to double star observers as a close and difficult pair. In connection with the measures above referred to, and again on p. 130 of the same volume, where measures of the close pair are given, the discovery of this double star is erroneously credited to Dembowski by Otto Struve. As a matter of fact it was discovered independently in 1868 by Dembowski (A. N. 1979), but it had been previously seen by Frederick Bird of Birmingham in February, 1864, with a 12-inch reflector (*Astronomical Register*, Vol. 1).

In the accompanying diagram Fig. 1, I have given the observed, corrected differences of declination between Procyon and these comparison stars. The upper horizontal scale refers to the observations of B, and the lower to C. As these stars are north of Procyon, both differences are increasing. The vertical scale on the left gives the date of the corresponding observations, but in order to have the positions of C directly opposite those of B of the same date, for convenience of comparing them with each

other, the observed positions of C are shifted ten years down the time scale. The straight line through the positions of one star was drawn as accurately as possible to represent the observations, and then the line representing the positions of the other star was drawn parallel to it. It would appear that neither of these stars has any sensible proper motion of its own.

When we come to examine these positions of B and C, at first glance they seem to conform exceedingly well to simple rectilinear motion; but upon a more careful inspection it is seen that these positions lie with more or less regularity upon one side and then the other of any straight line which will best represent them as a whole. To be sure these differences are small, but there is no doubt of the fact that from some cause there is a substantial agreement in the measures of both stars with respect to this apparently irregular motion. In considering the weight of this affirmative evidence, it should be noted that the differences in the individual observations, of which means are here used, are in some of the years rather large for measures of this character, in certain instances amounting to as much as 0″.5. This is readily explained by the varying conditions under which the measures must have been made, and the uncertainty of the bisection of the large spurious disc of Procyon. As a further test of this matter, the differences of the corresponding measures of B and C are shown on the left of the diagram, the same scale as to time and declination applying to these as to the other positions. It is evident that these differences, if there were no errors in the measures, would be constant from first to last, or, if one or both of the comparison stars had any independent proper motion, the differences would increase or decrease regularly, and in any event the positions there laid down would lie in a right line. Without knowing what these positions represented, one could easily imagine that there was a periodic variation running through the series, but obviously this cannot be true, whatever may be the apparent indications.

I have not used the observed differences of R. A. between Procyon and B and C for the reason that the work with the micrometer should be much more accurate and reliable, and it would add but little to the evidence already submitted from the meridian observations. It would be of interest to present in graphical form all these apparent variations as shown by the work with the meridian circle so that the consistency and value of the data could be submitted to tests which cannot be applied to tabular results.

PLATE XV. (FIG. 1.)

PROPER MOTION OF PROCYON.

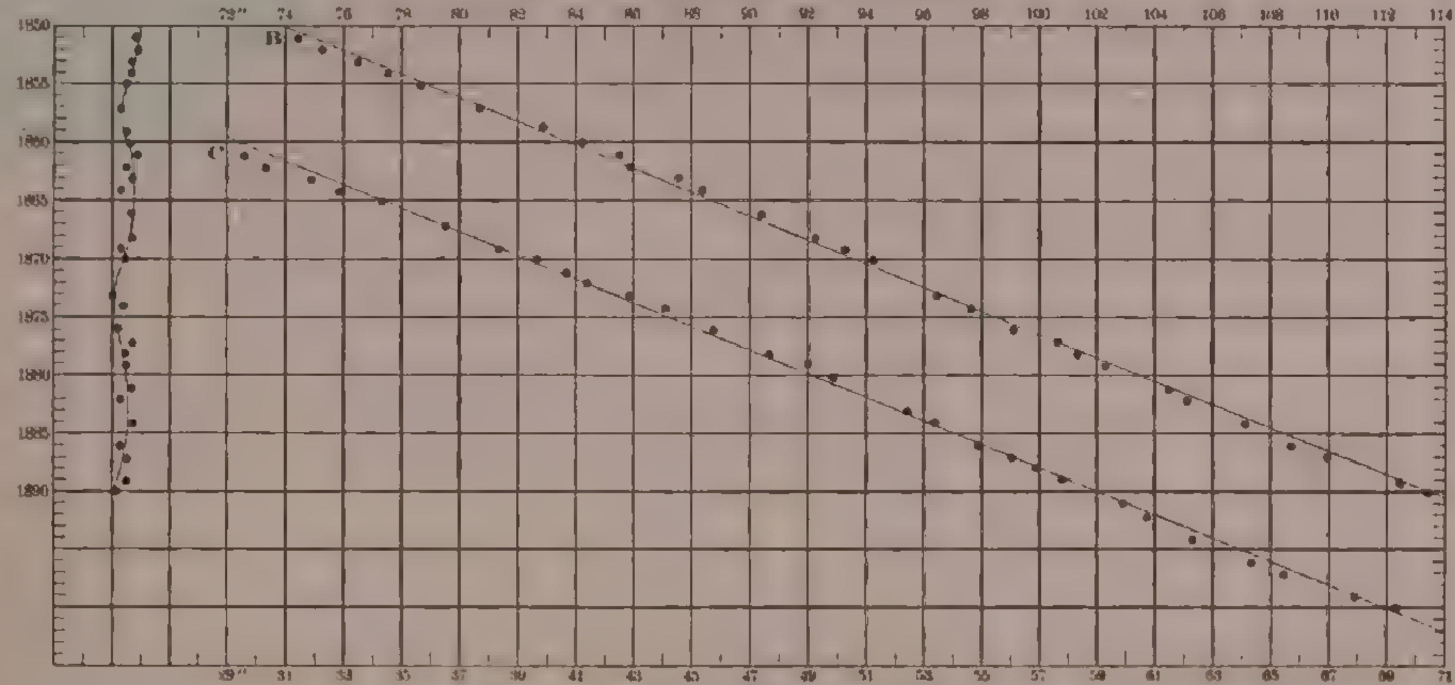
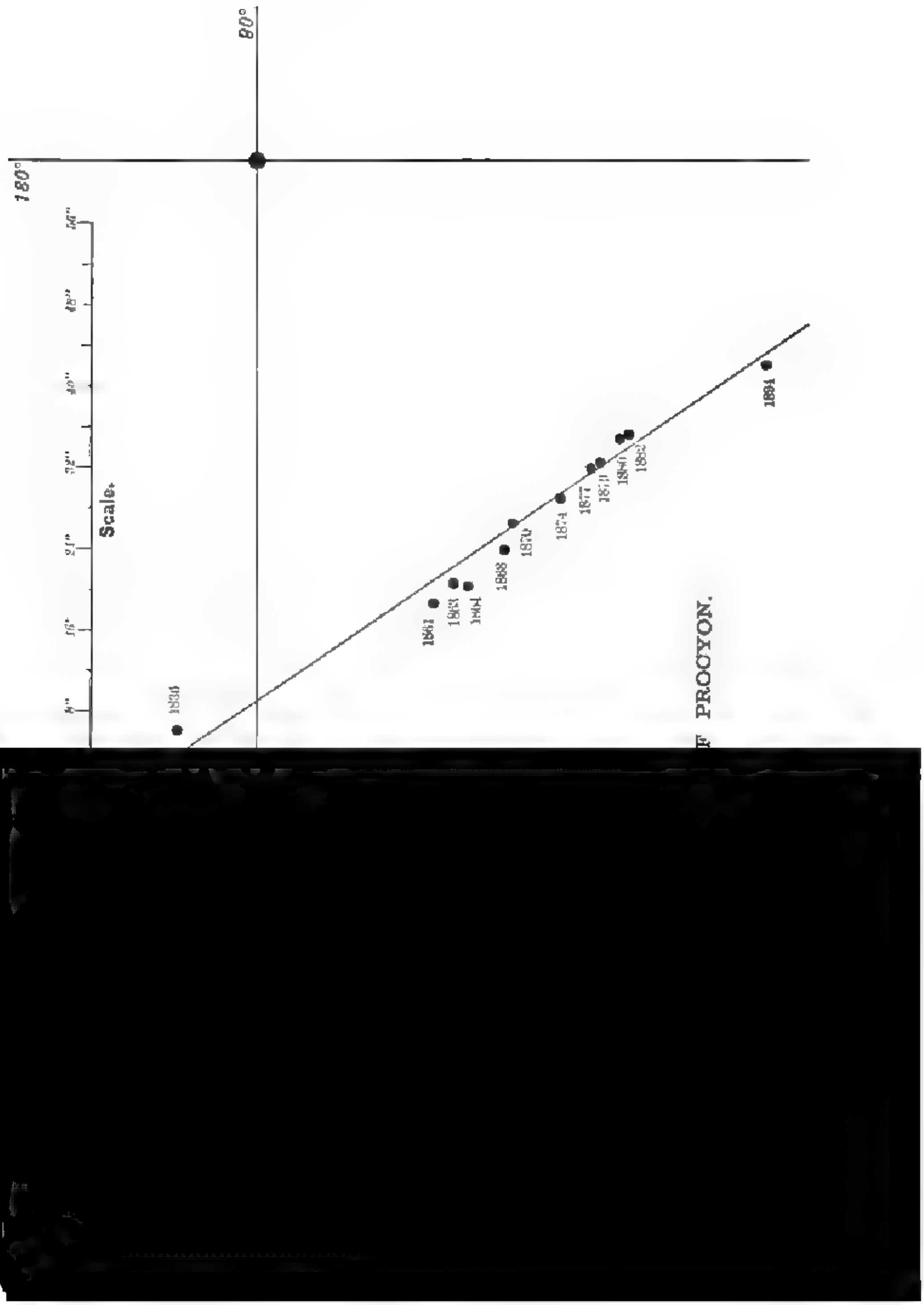


PLATE XVI. (FIG. 2.)



In the diagram Fig. 2, I have given all the measured positions of the distant faint companion discoverd by Lamont in 1836. These measures commence with the year of discovery, and end with a careful set of measures on four nights by Professor Barnard with the 36-inch refractor at Mt. Hamilton in the early part of the present year. The line drawn through these positions is one showing the recognized direction of the proper motion of Procyon as derived from meridian observations, which is in the position-angle of 214°.6, the annual movement being 1″.245. It is certainly the fact that these observed positions cannot be well explained by simple rectilinear motion. They appear to lie on both sides of the line representing the path of the large star in space; and to some extent at least these observations tend to confirm the variations shown by the measures of B and C. On the other hand it should be remembered that this is a faint star, and difficult to measure with moderate apertures; and also that the observations preceding my own, which commence in 1877, are mostly on single nights, and therefore liable to more error in angle and distance than if each position was a mean of several observations. The motion from 1861 to 1894, assuming the angles to be substantially correct, would indicate an annual proper motion of 1″.19, which is not far from the value found from other observations. If we take the interval 1836-94, it gives about 1″.53, a value manifestly too large, and indicating considerable error in Lamont's angle.

While the proof of variable motion, so far as all these observations are concerned, is not as satisfactory as could be desired, it is at least of sufficient significance to make it important to continue the series of measures so long carried on by the distinguished Russian astronomer at the Poulkowa Observatory; and in this connection the Lamont companion should be measured at the same time, if the aperture used is sufficient for accurate results, in order to carry forward the measures already made for future comparison. It will be seen from the diagram that the distance is now increasing, and that hereafter this star will be more easily measured.

Chicago, April 9.

Determination of Geographical Longitudes by Photograph.—In 1893 of *The Geographical Journal* will be found an interesting article on the above title.

RECENT OBSERVATIONS OF THE SATELLITES OF JUPITER.*

E. E. BARNARD

ASTRONOMY AND ASTRO-PHYSICS for 1894, May, page 356, contains a letter from Professor E. S. Holden, director of the Lick Observatory, which bears the title, "Recent Observations of the Satellites of Jupiter." In this paper he notices some recent observations of mine that bore upon the forms of the discs of these moons, and especially with reference to the remarkable observations at Arequipa.

There are several points in the letter referred to that deserve attention, though why it was written it would be hard to tell, as it neither makes any attempt to verify the Arequipa observations nor to disprove my own.

In speaking of the results contained in my paper Professor Holden says: "In this respect his conclusions entirely agree with those of Professor Schaeberle previously printed in the Publications, A. S. P., Vol. V, p. 182, dated September, 1893."

As I have not seen the Publications of the Astronomical Society of the Pacific for some three years, I was unaware of Professor Schaeberle's results, mentioned by Professor Holden. As those results are important in their bearing on the question at issue, I have looked up the number of that journal referred to and will here copy Professor Schaeberle's paper in full.

JUPITER'S SATELLITES.

"The August number of *l'Astronomie*, in commenting upon certain observations of Jupiter's satellites, made by the Arequipa observers asks whether the great Lick Observatory telescope could not for a time be devoted to this curious subject.

"Results of satellite observations made here by various observers have been published at various times, and from some personal observations of the conditions prevailing at Arequipa I have little hesitation in saying that for the same observer, the results given by the 13-inch telescope at Arepuipa can not equal those by the 36-inch on Mt. Hamilton; so that if it should finally turn out that certain marked peculiarities of the satellites had been observed at Arequipa which had not been observed here, this must be attributed to a superior diligence of the South American observers." J. M. S.

1893, September 1.

* Communicated by the author.

This is the paper to which Professor Holden refers and which was not known to me at the time of my observations.

I cordially agree with Professor Schaeberle's remarks.

I have conscientiously made my observations. I have made no record of the appearances of these satellites unless the conditions were favorable. I have given the time of each observation, so that they can be compared with theory. This has been done when my work on the new satellite necessarily directed my attention to the planet. What appearances these satellites may assume at other times than those I have noted in my paper, I do not hold myself responsible for.

The observations at Arequipa have attracted great attention from the astronomical world. They have brought up believers as well as unbelievers. It was absolutely necessary that some powerful telescope be brought into action to decide the question of the reality of the phenomena claimed at Arequipa. I have done this and am satisfied with the results.

Perhaps Professor Holden has some personal work on these moons with the great telescope that will be valuable in further settling this important question.

In reference to my paper on the transparency of the limb of Jupiter (A. AND A.-P. 124, April 1894, p. 272,) I have nothing to change in it. Professor Holden has (inadvertently I have no doubt) neglected to state the atmospheric conditions existing at the time of occultation of 47 Libræ, 1888, June 9. In my paper in A. AND A.-P., page 278, I have expressly stated that "when the seeing is bad there is a spurious limb to Jupiter that well might give the appearance of transparency at the occultation of a satellite."

By looking at page 64, Vol. VIII of the *Astronomical Journal* the observations of that occultation of 47 Libræ by both Professor Holden and myself will be seen.

The seeing was so bad in my case that I reduced the aperture of the 12-inch down to "8.1 inches as the images were too unsteady with full aperture." On a scale of 5 for perfect seeing, I stated the steadiness as 2.

In his paper on this occultation Professor Holden says, "Images, weight 2 (5 perfect; 1 very poor)" and again, "at Chron. time $14^h\ 12^m\ 35^s.2$ the star was entirely inside of the limb, but was still visible.

"The dark circle was no longer seen. For the next ten seconds or so, the star was alternately visible and invisible; the planet's limb was quite unsteady."

The star was 3 hours and 35 minutes west of the meridian and 19° south of the equator, and with an altitude of only 15° to 20°.

Those who have attempted to observe Jupiter at low altitudes like this, will understand from the above notes that the conditions actually existed here for the production of an ill-defined and spurious limb to the planet, and the appearance of transparency was almost a necessary result of the existing conditions, so that really the observations proved nothing—except what was known and noted at the time, *i. e.*, that the seeing was bad.

Since the Publications of the Astronomical Society of the Pacific have been referred to by Professor Holden, I recall some observations that he himself has made proving the transparency of the limb of Jupiter with the great telescope, and as I wish to give both sides of this question a fair hearing, I will quote his own language as dictated to me in 1889. See *Pub. A. S. P.*, Vol. 1, No. 5, p. 104, where I say; "At a number of occultations of the satellites, I watched carefully for any evidences of their being seen through the edges of the planet, but saw nothing of the kind. *Professor Holden informs me, however, that, with the thirty-six-inch equatorial, the whole disc of a satellite has been visible within the planet's atmosphere* [limb], *at every occultation he has observed.*" The italics are mine.

It would be interesting to know the dates of these important observations, and the conditions of seeing at the time they were made.

From the above statements, it would seem that I have simply been unfortunate in not observing a satellite at the time Jupiter's limb was in a transparent condition.

In reference to the period of rotation of the 1st satellite, Professor Holden misstated my conclusions. He says, referring to the observations of Professors Schaeberle and Campbell: "In the same paper they conclude that the periods of axial rotation and of revolution of satellite I are equal. Dr. Barnard says that his observations (as yet unpublished) lead to a different result."

What I have stated in this connection, is contained in a paper in *Monthly Notices* of the Royal Astronomical Society, Vol. LIV, No. 3, p. 135, where I say, in speaking of a bright belt on this satellite:

"From peculiarities in the appearance of the belt, it is probable that the period of rotation on this axis is not coincident with the satellite's period of revolution about Jupiter.

"I have data in my hands now that will after a few more observations, perhaps, settle the inclination of the axis, and probably give us the period of rotation."

These remarks were caused by an appearance of widening on the white belt, sometimes seen on the preceding and sometimes on the following side.

Certainly by observations of this phenomenon—if it is a fixed one—the period may be determined. I have therefore made no positive statement that this moon does not rotate on its axis once only during its revolution about Jupiter.

Indeed from the analogy of our own moon we would expect it to do so. Professor W. H. Pickering, however, gets a period 13^h 03^m $25^s.8$ upon the hypothesis of a direct rotation, and 13^h 03^m $10^s.8$ on the supposition that its rotation is retrograde, which he supposes to be the case. But as his results depend upon the very conditions that my observations negative, *i. e.*, the distortion of its disc, they cannot be considered proved.

The concluding remarks of Professor Holden are rather ambiguous.

"These few points from recent papers show very forcibly that everything is not yet settled with respect to Jupiter's satellite system and may serve to direct the attention of the possessors of large telescopes to some of the problems involved."

These same remarks will apply with equal certainty to every object in the entire heavens.

1894, May 10, Mt. Hamilton, Cal.

WEST INDIAN HURRICANES AND SOLAR MAGNETIC INFLUENCE.*

FRANK H. BIGELOW.

In the February number of ASTRONOMY AND ASTRO-PHYSICS, Professor H. A. Hazen submits a criticism on my suggestion regarding the recurrence of hurricanes in the 26.68 day solar period. It is perhaps desirable to indicate some erroneous steps in his discussion, upon which his negative conclusion was based.

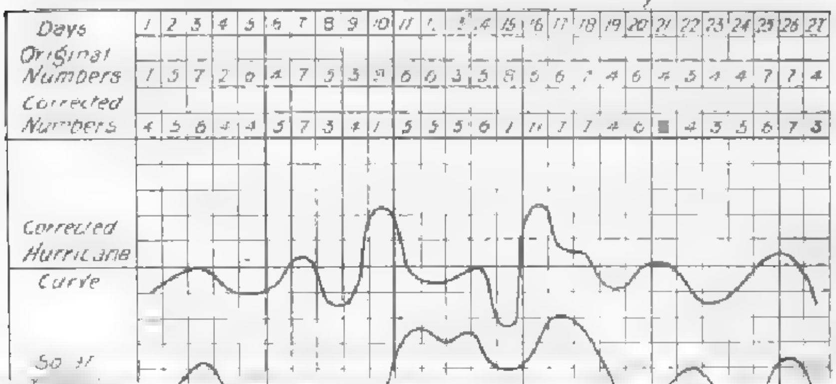
1. He seeks to invalidate the data on which the hurricane recurrence curve was founded, namely Finlay's dates, in his *Hand-Book of Storm Tracks*, pp. 16, 19, as follows: "On examining more closely the material used, we are much surprised to find that 42 out of 80 cases are of North Atlantic storms, or storms above 45° north latitude, and which have no relation whatever to the tropical hurricanes we are supposed to be studying." Now Finlay specifies the 42 cases, which Professor Hazen

* Communicated by the author.

rejects, as those *charted*, and they are given on the chart as typical West Indian Hurricanes, so that the oversight is really that of the critic himself, and his point is invalid.

2. He seeks to substitute for Finlay's hurricane list of 80 cases a more extensive one of his own including 144 storms, A. AND A.-P., p. 108, and by distributing these in the 26.68 day period, he concludes that the curves of comparison are not synchronous. In making the distribution of dates in the period for years 1874 to 1893 inclusive, a systematic error in the use of the ephemeris itself has been introduced by Professor Hazen, which gives a false summary of the dates. This occurred as follows: The construction of the dates of the beginning of the successive periods of the magnetic ephemeris, is such that the fractions of days greater than 50 hundredths, must not be called by the next integer day. Thus 15.78 is to be called day 15, and not day 16.

Hazens Hurricane Dates distributed in the 26.68 Day Period.



is then platted from the corrected numbers, on a scale to be readily compared with my solar magnetic curve, which is placed beneath it.

The comparison of these curves, in connection with the Finlay curve, as given in the *American Meteorological Journal* for January, 1894, p. 382, shows that the extension of the dates from 82 (Finlay) to 144 (Hazen) has considerably improved the curve of hurricane recurrences. The marked discrepancy, to which Professor Hazen objects, at the 20th day of the period has disappeared.

It is hardly possible that two such peculiar curves as these, covering an interval of 20 years, should have come forth accidentally into this interesting agreement. My purpose in the original article was to call attention to a subject that needs elucidation, and to suggest that some light might be gained on the lines of research that are proving so fruitful in allied questions of meteorology. In my judgment, the outcome of Professor Hazen's labor is to strengthen the validity of this line of approach to the mechanism that originates the cyclones of the West Indies and the Gulf of Mexico.

WEST INDIAN STORMS AND SOLAR MAGNETIC INFLUENCE.*

H. A. HAZEN.

The above effort to train my own guns upon me is quite interesting. On many accounts, I would prefer to leave the matter as in the February journal, as I am sure meteorologists in general will be enabled to see the fallacy in this later discussion. I desire, however, to make my own position a little clearer, and at the same time, aid some who may not see readily the exact nature of the above result. The whole subject is much farther reaching than appears on the surface and hence merits a careful study.

I now learn that the dates chosen by Professor Bigelow for his magnetic ephemeris are not civil dates, that is, from midnight to midnight, but the epoch is for the middle of the 24 hours, or practically for noon of each civil day. Of course this would make no difference, except to throw the curve half a day forward or toward the beginning of the period, if we only had enough observations. This will be seen by adding my numbers for "storms by number" two by two and comparing with Professor Bige-

* Communicated by the author.

low's latest set two by two, that is, the sum of my 1 + 2 equals almost exactly his 1 + 2, and my 2 + 3 equals his 2 + 3 and so on. Also if my curve of storms is compared with this latest one by Professor Bigelow it will be seen that there is really very little difference, in general only a single day in the crests and hollows.

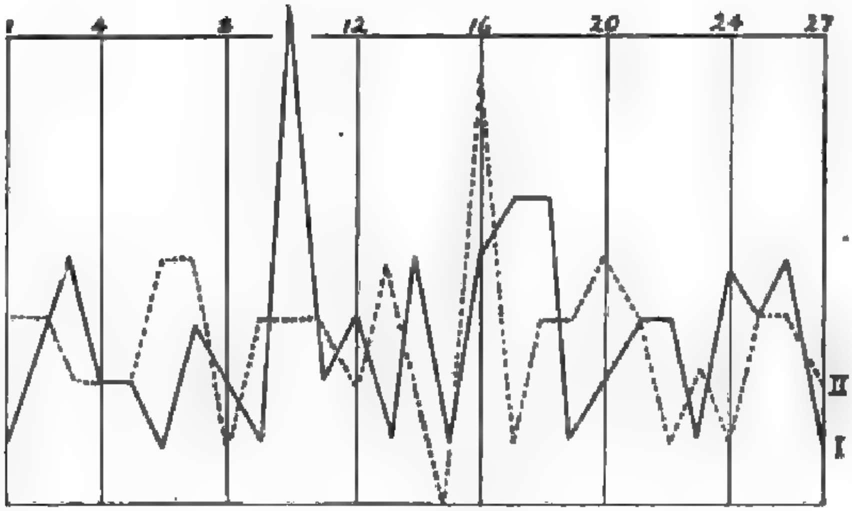
I cannot agree at all to the view that there is a close correspondence between the new curve of storms and the magnetic curve. On the 6th day there is a fall in the former and none in the latter, on the 7th the sharp rise in (1), (curve of storms) has a steady fall in (2), (magnetic curve), on the 8th there is a minimum in (1) which is reached in (2) on 9th, on the 10th there is a sharp maximum in (1) not found at all in (2), and the whole curve of (1) for the 11, 12, 13 and 14 days is totally different from (2), the sharp and long rise in (1) from 15 to 16 has no such condition in (2), the minimum in (1) on the 17th has a maximum in (2) and so on. It seems to me the curves are very dissimilar at almost every turn and throughout the whole rotation.

But this is not all. If we had a great many more observations of storms we could settle this matter once for all, it seems to me, however, that there are enough to give us an inkling of the truth. Suppose we divide my dates of storms into two equal groups and sum each group by itself, we shall then at once see whether this result is entirely fortuitous or whether there is a physical basis at the bottom of this supposed relation between magnetism and storms. The following are the figures in the groups doubled to make them comparable:

Day of rotation	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27
Group I (173 storms)	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
II (174 storms)	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

The curves I and II show these figures graphically. Here we have about as good an illustration of a nonconformity as could be devised. In almost every crest or hollow in the one curve an opposite condition occurs in the other. It seems to one that nothing more could be desired to show how unreliable such a result as that obtained by Professor Bigelow is, and the great danger of relying on an apparent coincidence to prove a relationship between dissimilar phenomena.

I still think strongly that there is a most serious difficulty in the original fundamental magnetic curve. A treatment of the magnetic data in groups, as just given for storms, would very quickly show that there is no common thread running through the groups, and that the crests and hollows in the curve are



purely fortitious. I am also informed that in making up this fundamental mganetic curve the "disturbed days" were omitted. It seems to me that these days are the ones above all others to show the influence of magnetism on storms if there is one. Of course, in getting the diurnal swing of the magnetic needle, it is perfectly proper to omit all the 24 readings on any "disturbed day," as they do not enter into the diurnal range, and by taking out the whole day, the final result is not altered, but it is far otherwise in this case.* To take out a *disturbed* day in a rotation period of 27 days is to take out the very day of all others to show this influence best. I think it is impossible to consider that there can be any magnetic influence from the Sun that goes through such extraordinary and rapid changes as those exhibited by the crests and hollows in this fundamental curve.

March 30, 1894.

PROPER MOTION OF STARS IN THE DUMB-BELL NEBULA.*

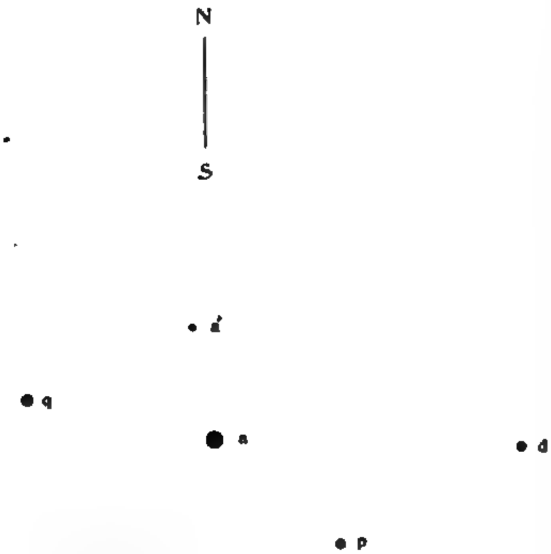
E. E. BARNARD.

In the *Monthly Notices* of the Royal Astronomical Society for March, 1894, is a paper by Arthur A. Rambaut and W. E. Wilson on the Proper Motion of some stars in the Dumb-bell Nebula.

These gentlemen, from comparing measures of the stars on two photographs of this nebula (made by Mr. Wilson with a two foot reflector 1893, Sept. 3 and 8, with exposures of 27 minutes

* Communicated by author.

and 2 hours respectively) with measures of the same stars by Struve some forty-two years ago, find for those marked on their diagram *p*, *d*, *k*, *i*, *b′*, and *y*, proper motions of 4″.6, 3″.9, 5″.7, 6″.0, 5″.2, and 5″.3 respectively, covering the interval since Struve's work.



OBSERVED STARS IN THE DUMB-BELL NEBULA.

These stars in the main are faint and require a good telescope to measure them satisfactorily.

To test the reality of these changes I have re-measured those of this list not too distant from the comparison star *a* with the 36-inch. The small stars were referred to *a* by position angle and distance. Following are the measures of *p*, *d*, *q*, and *a′*.

POSITION ANGLES AND DISTANCES REFERRED TO *a*.

	q			*p*	
1894.347	257°.3	97″.51	1894.347	43°.9	83″.65
.350	257°.1	96″.76	.350	43°.7	83″.56

p		*d*	
+ 58″.00	+ 60.27	+ 153″.25	+ 15″.79
+ 57 .73	+ 60.41	+ 153 .22	+ 16 .27
+ 57″.86	+ 60.34	+ 153″.23	+ 16″.37

The following comparison will show the supposed changes since the time of Struve's observations.

	$\Delta\alpha$	$\Delta\delta$	$\Delta\alpha$	$\Delta\delta$
			Differences from Struve.	
		q		
Struve	− 97″.6	− 20″.9		
Wilson	− 94 .4	− 20 .6	− 3″.2	− 0″.3
Barnard	− 94 .7	− 21 .5	− 2 .9	+ 0 .6
		a′		
Struve	− 12″.3	− 57″.2		
Wilson	− 9 .6	− 57 .5	− 2″.7	+ 0″.3
Barnard	− 9 .6	− 57 .5	− 2 .7	+ 0 .3
		p		
Struve	+ 60″.6	+ 57″.9		
Wilson	+ 56 .6	+ 60 .1	+ 4″.0	− 2″.2
Barnard	+ 57 .9	+ 60 .3	+ 2 .7	− 2 .4
		d		
Struve	+ 152″.5	+ 12″.8		
Wilson	+ 151 .5	+ 16 .6	+ 1″.0	− 3″.8
Barnard	+ 153 .2	+ 16 .4	− 0 .7	− 3 .6

If we assume Struve's $\Delta\alpha$ and $\Delta\delta$ for the star *p* to have been interchanged—and it looks very much as if they have—we find a very good accordance in the different measures of *p* which would negative any motion in that star.

There seems, however, to have been some change in *q*, *a′* and *d* if Struve's distances are correctly given.

In their paper Messrs. Rambaut and Wilson call $d = a$ in magnitude. This is evidently a photographic effect due to the fact that *a* is tinged with yellow, for it is very greatly brighter than *d*. Dr. Roberts photographs however, show *a* decidedly the brighter.

I have assigned the following magnitudes to

$a = 12$ mag.	$p = 15\frac{3}{4}$ mag.
$q = 14\frac{3}{4}$ "	$d = 14\frac{3}{4}$ "
$a' = 15\frac{1}{2}$ "	

The magnitudes as given by Struve are

$a = 10^m$	$p = 13$
$q = 11 - 12$	$d = 11 - 12$

The appearance of the nebula in the great telescope is different from Dr. Robert's photographs of it only in the finer details.

I have examined Struve's original paper in *Philo. Trans.*, Vol. 151, part III, and find that his method of measuring the places of these stars was mainly by measuring position angles and with only a few direct distances from which the other distances were computed. Indeed no direct measures (referred to *a*) were made at all of any of the four stars under discussion. Not doubting the accuracy of the Struve results, yet it would have been more satisfactory if the distances had been directly measured. It might therefore, be well to withhold any opinion as to motion in these four stars until sufficient time elapses to show this motion by another series of measures compared with the present ones.

MT. HAMILTON, 1894, May 9.

Astro-Physics.

THE WOLF-RAYET STARS.*

W. W. CAMPBELL.

These observations of the Wolf-Rayet stars were begun in September, 1892, for the purpose of determining whether any relation existed between them and the new star in Auriga. The first examination of their spectra resulted in the finding of a large number of new bright lines and bands whose wave-lengths could be determined very accurately with our apparatus. In view of the very general interest taken by spectroscopists in bright-line spectra,—a fact shown by the frequency with which even the approximate wave-lengths of the few known lines and bands are quoted,—I decided to investigate these stars as completely as possible. The observations were practically finished in July, 1893.

The stars of this type are characterized by bright bands, and in some cases by bright lines and bands superposed upon their strong continuous spectra. Many are further marked by the presence of absorption bands of different intensities and breadths, and in several I have found dark lines. While many of these spectra differ widely from each other in appearance, and a few present almost no points of resemblance, yet they all fall readily into one class, distinctly separate from every other known type.

The first three of these stars were discovered by MM. Wolf and Rayet, in 1867 at the Paris Observatory, in the constellation Cygnus.

M. Respighi, in 1871 at Madras, found that γ Argus has a simi-

TABLE I.—LIST OF KNOWN WOLF-RAYET STARS.

No.	Star.	Mag.	R.A (1900)	Dec. (1900)	Gal. Long.	Gal. Lat.	Discovered by
			h m	°	° ′	° ′	
1	DM. + 63°83	9.5	0 37.5	+ 64 14	89 53	+ 2 14	H. C. O.
2	DM. + 56°686	9.1	2 33.9	+ 56 18	105 21	− 2 18	"
3	DM. + 56°731	9.5	2 44.8	+ 56 31	106 37	− 1 26	"
4	Cord. G. C. 8631	7.2	6 50.0	− 23 48	202 19	− 8 52	P.
5	γ Argus	3	8 6.5	− 47 2	230 20	− 6 59	R.
6	Cord. Z. C. 8^h4141	9	8 51.6	− 47 12	235 13	− 1 4	C.
7	" G. C. 13554	8½	9 51.6	− 57 15	248 43	− 2 19	H. C. O.
8	——	—	10 6.1	− 62 5	253 0	− 5 14	"
9	——	—	10 7.6	− 60 8	252 4	− 3 30	"
10	——	—	10 13.3	− 57 24	251 14	− 0 48	"
11	——	—	10 14.4	− 62 9	253 51	− 4 46	"
12	——	—	10 22.9	− 58 8	252 42	− 0 46	"
13	Cord. G. C. 14626	7.2	10 37.4	− 59 9	254 51	− 0 46	"
14	" Z. C. 10^h2684	9	10 37.8	− 58 15	254 29	+ 0 4	C.
15	" G. C. 14684	6.9	10 40.1	− 59 36	255 21	− 1 1	H. C. O.
16	" " 14691	8½	10 40.3	− 59 12	255 48	− 0 40	"
17	——	—	10 43.4	− 58 41	255 20	− 0 1	"
18	——	—	10 47.9	− 61 46	257 5	− 2 36	"
19	Cord. G. C. 14965	8½	10 52.0	− 59 51	256 49	− 0 38	"
20	——	—	10 55.8	− 57 17	256 16	+ 1 56	"
21	Cord. G. C. 15220	8.2	11 2.3	− 64 58	259 53	− 4 53	"
22	" " 15305	8	11 5.8	− 60 26	258 38	− 0 31	C.
23	——	—	11 55.2	− 54 33	263 29	+ 6 53	H. C. O.
24	Cord. G. C. 17840	6	13 1.7	− 64 46	272 19	− 2 51	"
25	——	—	13 11.5	− 57 36	274 13	+ 4 8	C.
26	——	—	13 24.3	− 61 34	275 15	− 0 1	H. C. O.
27	——	—	13 36.3	− 66 55	275 30	− 5 31	"
28	Cord. Z. C. 15^h934	9	15 15.9	− 62 20	287 4	− 5 34	"
29	——	—	15 55.0	− 62 28	290 34	− 8 28	"
30	Cord. Z. C. 15^h4129	8½	16 0.6	− 25 57	290 39	+ 17 39	"
31	" G. C. 22748	5.9	16 44.5	− 41 4	311 20	+ 0 44	"
32	" " 22763	7.5	16 45.3	− 41 41	310 56	+ 0 14	"
33	" " 22827	7	16 47.3	− 41 40	311 11	− 0 3	C.
34	" " 22838	8.2	16 47.9	− 44 50	308 47	− 2 7	H. C. O.
35	" " 22843	6.4	16 48.0	− 41 0	311 47	+ 0 17	"
36	" " 23072	6.5	16 57.0	− 38 0	315 13	+ 0 45	"
37	" " 23073	7.1	16 57.2	− 37 42	315 29	+ 0 55	"
38	" " 23416	7.2	17 12.1	− 45 32	310 47	− 5 57	"
39	" Z. C. 17^h3612	9	17 55.1	− 32 42	326 8	− 6 15	"
40	SDM. − 19°4854	9.6	18 2.1	− 19 25	338 33	− 1 54	"
41	" − 21°4864	7.8	18 2.5	− 21 16	336 56	− 2 5	P.
42	" − 11°4593	8.7	18 13.5	− 11 40	347 36	+ 0 35	H. C. O.
43	DM. + 30°3639	9.3	19 30.9	+ 30 19	32 26	+ 3 57	"
44	" + 35°3953	7.0	20 2.2	+ 35 31	40 20	+ 1 10	"
45	" + 35°4001	8.5	20 6.5	+ 35 53	41 8	+ 0 39	W.-R.1
46	" + 35°4013	8.0	20 8.2	+ 35 54	41 20	+ 0 24	" 2
47	" + 37°3821	7.1	20 8.4	+ 38 3	43 7	+ 1 35	C.
48	" + 36°3956	8.0	20 10.8	+ 36 21	42 1	+ 0 13	W.-R.3
49	" + 36°3987	8.1	20 13.3	+ 37 7	42 56	+ 0 15	P.
50	" + 38°4010	8.7	20 15.8	+ 38 25	44 16	+ 0 37	H. C. O.
51	" + 43°3571	7.5	20 17.1	+ 43 32	48 32	+ 3 27	"
52	" + 36°4028	9.5	20 17.8	+ 36 36	43 2	− 0 46	"
53	" + 55°2721	8.9	22 15.0	+ 55 37	70 29	− 0 50	"
54	——	10	22 23.7	+ 55 46	71 38	− 1 20	"
55	DM. + 56°2818	8.9	22 32.9	+ 56 23	73 3	− 1 25	"

The fifty-five Wolf-Rayet stars now known are arranged in the order of their right accensions in Table I, with their catalogue numbers and magnitudes in the second and third columns. The anonymous star No. 54 was observed here, and I estimated its magnitude at 10. The discoverer of each star is noted in the last column. (W.-R. = Wolf-Rayet; R. = Respighi; P. = Pickering; C. = Copeland; H. C. O. = Harvard College Observatory.)

Professor Pickering has pointed out* that all these stars are near the central line of the Milky Way, and have a tendency to occur in groups. By assuming the position of the North Pole of the Galaxy at the year 1900 to be in Right Ascension $12^h 40^m$ and declination $+28°$, and measuring the longitudes northward from the ascending node (in right ascension $18^h 40^m$ and declination 0°) he found for the galactic longitudes and latitudes of these stars the results given in the 6th and 7th columns. It will be seen from the column of latitudes that all but six are within 6° of the central line of the Milky Way; only one, No. 30, is more than 9° from the central line; and that one is, moreover, in a projecting spur of the Milky

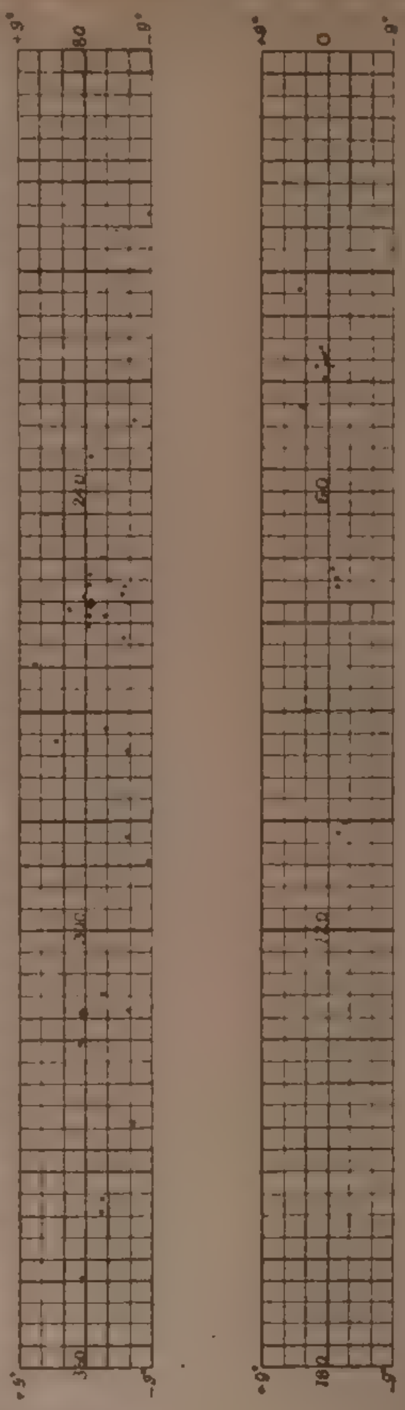


Fig. 1.—Distribution of the Wolf Rayet Stars in the Milky Way

* *Astronomische Nachrichten*, No. 3035.

Way. But the remarkable distribution of these stars is shown best by the accompanying chart (Fig. 1), on which the black dots representing them have been plotted by means of their galactic coordinates. The upper half represents the Milky Way south of the equator, and the lower half the Milky Way north of the equator. It extends only 9° each way from the galactic circle, and star No. 30 is necessarily omitted. In addition to the three well defined groups, made up of the stars numbered 31–38, 7–22 and 43–52, there are a few minor groups. An unduly large number, thirty-nine, are in the southern hemisphere, and only sixteen in the northern. Attention should also be called to the preference of these stars for two large and nearly opposite regions of the Milky Way, and their avoidance of the two equatorial regions, which can not be attributed to incompleteness in the observations for detecting them.

Thirty-two stars of this type are visible at Mt. Hamilton; but many of these are near the southern horizon at meridian passage and can be observed only at a great disadvantage. γ Argus, of the 3d magnitude, is the only bright one. A few other stars very far south are of the sixth magnitude. But unfortunately all those in good position for observing with the 36-inch telescope are between the 7th and 10th magnitudes; so that practically this investigation has been limited to the spectra of the faint stars of this class.

The visual determinations of the positions of the star lines are based upon micrometer comparisons with near lines in the artificial spectra of copper, hydrogen, iron, lead, magnesium, mercury, zinc, etc. Rowland's wave-lengths of the artificial lines were employed, in so far as he has given them; and the positions of the others I determined by direct micrometer comparison with Rowland's lines in the solar spectrum, using second and third order spectra. The wave-lengths are therefore based upon Rowland's scale.

Each night's observation of a line consisted in making, on an average, five readings each on the star line and artificial line. The positions of all the lines and bands visible in the thirty-two stars have been measured as accurately as possible, and nearly all have been observed on two or more nights. For the most part the observations were made with a 60° dense flint prism dispersing $5\frac{1}{2}°$ between B and $H\epsilon$, and a magnifying power of 13. With this prism and eye-piece I am just able to separate the two components of the double solar line at λ 4891, which are about 0.7 tenth-metre apart. Occasionally a strongly dispersing compound

prism, or a first or second order grating, or a magnifying power of 26, would be employed. Very often, even in the case of the brightest lines, no gain of accuracy was made by using the higher dispersion, on account of the great width of the lines and the consequent difficulty of estimating their centres.

The photographic observations were made with the 60° prism and $10\frac{1}{2}$-inch camera. The hydrogen comparison spectrum was photographed on each plate, on both sides of the star spectrum, at about the time when the exposure on the star was half completed. Stronger dispersion would have been desirable in studying some parts of a few of the spectra; but in many cases it would have been a positive disadvantage, owing to the great width of the bright bands: the increased width of the bands, due to higher dispersion, would require increased exposure-times, and would not yield corresponding increase of accuracy in measurements. Further, the Observatory does not yet possess a spectroscope of rigid construction for photographic work. Our present instrument lies almost wholly in one plane; and while serving its original purpose admirably, the flexure during the long exposures required for these faint stars is sufficient to introduce slight errors of wave-length, broaden the lines, and injure the definition. For this reason, a number of bands suspected of being double and multiple are left in doubt. Nevertheless, it has been considered profitable to secure one negative of each spectrum, and twenty-four have thus far been photographed. As is well known, the chromatic aberration in a large visual telescope is very marked in the blue and violet. Such is the case with the 36-inch equatorial. Light of only one given wave-length enters the spectroscope properly for any given position of the slit. I am able, therefore, to photograph only a very limited part of a stellar spectrum at one time. A few of the photographs of these spectra extend from $H\beta$ to near $H\epsilon$; but the greater number do not extend above λ 420.

The photographic results must be considered, for the above reasons, as incomplete and provisional, and I hope to secure another set at some future time with a suitable spectroscope.

The thirty-two stars observed are arranged in the order of their right ascensions in the following tables. Under each star are the collected results for the several bright lines and bands in its spectrum, both visual and photographic. Each wave-length recorded to four or five places is the result for one night's work on that line; and on the average depends, as stated above, upon five micrometer comparisons. The visual determinations for the

several nights appear in vertical columns, and their means are taken.

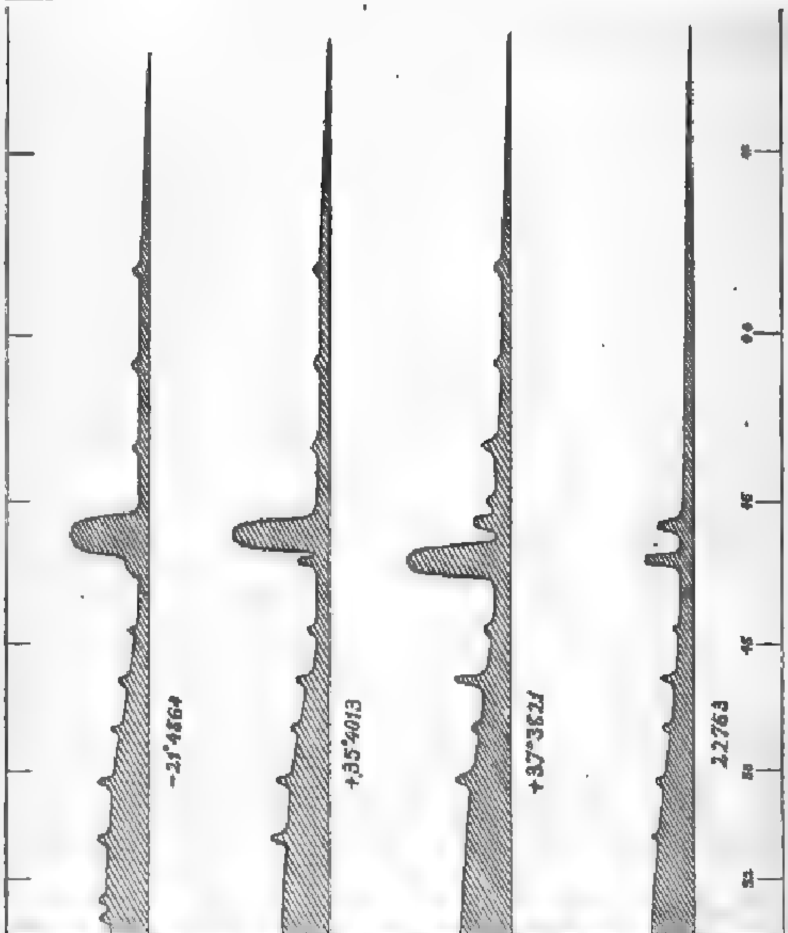
Very few of the lines are monochromatic images of the slit. They are, with few exceptions, more or less broad and ill-defined bands, brightest at the centre, and shading off sometimes rapidly, again gradually, in both directions. In a few cases the bands are unsymmetrical. Two extreme cases are found in the stars D.M. + 30° 3639 and + 43° 3571. The lines in the former are nearly all narrow and some are monochromatic; while in the latter star they are abnormally broad and somewhat unsymmetrical.

The intensities of the lines are indicated in a general way by means of symbols under the numbers representing their wave-lengths. Thus, ++ signifies "very bright"; + signifies "bright;" those "easily measurable" are unmarked; the "difficult" lines are marked thus —; while for those too faint to justify micrometer measures, and for some "measurable" or "difficult" lines which are "very bright" in a number of other stars, the results are given in three figures.

The visual intensity curves of these thirty-two stars were constructed in my note-books twice, on the average, for each star. It is impracticable and unnecessary to reproduce them all, and I have selected for reproduction the four shown on Figure 2. They are typical of nearly all the spectra, but of course are far from representing all the variations in the relative intensities of the bands. The photographic intensity curves of four stars are shown on Figure 3, but they are seriously modified, it must be remembered, by chromatic aberration.

The spectra of these stars did not seem to me to be relatively faint in the blue and violet. The continuous spectra were usually visible up to Hδ; which, considering the magnitudes of the stars, is as far as we would expect to follow them. Some of the spectra, notably that of γ Argus, seemed unusually strong in the blue and violet.

The dark absorption bands were observed by noting them in the intensity curves. A large number of the stars show no traces of the broad absorption bands, several others show them slightly; in a few the absorption is plainly visible, and in a few it is marked. On the whole, the absorption is much less than I had expected to find it. While much of the effect is real, some of it is undoubtedly due to contrast between faint continuous spectrum and very bright bands. Nearly all the strong absorption bands noted by me lie on one or both sides of the strong bright bands λ 581 and λ 569 in the yellow, and of the very strong bright



SUAL INTENSITY CURVE OF THE WOLF-RAYET STARS.

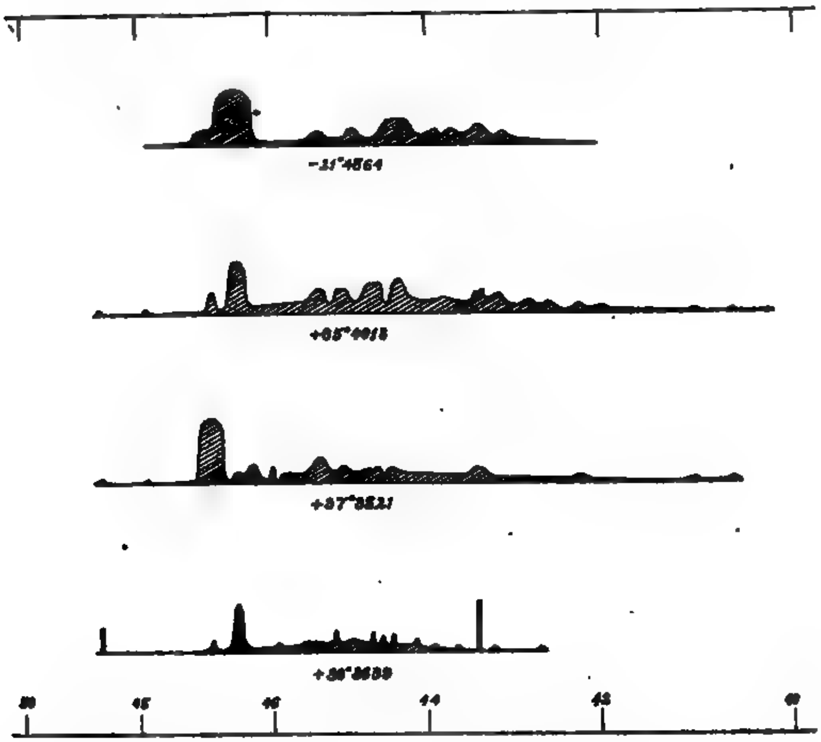


FIG. 3.—PHOTOGRAPHIC INTENSITY CURVE OF THE WOLF-RAYET STARS.

bands λ 469 and λ 465 in the blue. They do not admit of accurate observation.

No. 1. DM. + 63° 83.

				Hβ			
581	5413.0	5023	4938	4861	4688.2	4615	454
......	5413.5	....	....	....	4687.9	4614	......
......	5411.9	....	....	....	4687.5	4615	......
......	5412.6	....	....	....	4686.4	....	......
......	....	....	....	....	4685.3	....	......
581	5413	5023	4938	4861	4687	4615	454
−	+	−	−	−	++	−	−

Photographic: 4688, 4615, 454.
++ +

Negative under-exposed. The band 469 is about 40 tenth-metres broad.

No. 2. DM. + 56° 686.

								Hβ				
5814.2	5690.0	5590.8	5472	5414	526	5126	5020	4861	4787	469	4653.6	4534
5813.4	5692.4	5598.0	......	5411	.....	5125	5020	......	......	.....	4653.2	4535
....	....	....	....	......	.....	5121	5021	......	......	.....	4653.2	......
5814	5691	5594	5472	5412	526	5124	5020	4861	4787	469	4653	4534
++			−	−		−		−	−		++	−

No photograph taken. The band 465 unites with and overlaps a fainter band at 469, as in the star No. 41, S DM. $-21°$ 4864. [See Fig 2].

No. 3. DM. $+56°731$.

5810.1	5690	5594	5470	5411.2	469	4654	454
5812.3	5692	5594	....	5410.8	......	4652	......
5813.8	....	....	....	....	......	4656	......
5811.8	....	....	....	....	......	....	......
5812.8	....	....	....	....	......	....	......
5812	5691	5594	5470	5411	469	4654	454
+ +			−			+ +	

No photograph taken. The band at 465 unites with and overlaps the fainter band 469, as in the preeeding star.

No. 4. Cord. G. C. 8631.

Hα	D_3							Hβ					Hγ
6563	588	5807	569	559	5471	5418	4944	4860	4789	4688	4614	4545	4343
....	......	5803	......	......	....	5413	4944	4863	....	4688	4618	4544	4342
....	......	5809	......	......	....	5416	4940	4863	....	4687	4614	....	....
....	......	5808	......	......	....	5415	....	....	....	....	....	....	....
....	......	....	......	......	....	5416	....	....	....	....	....	....	....
....	......	....	......	......	....	5415	....	....	....	....	....	....	....
6563	588	5807	569	559	5471	5416	4943	4862	4789	4688	4614	4544	4342
−					−	++		+		++	+	+	

Photographic: 4688, 4628, 460, 454, 451, Hγ, 420, Hδ. Exposure too short.
++ + + ++ ++ +

The band observed visually at 4614 is shown by the photograph to consist of two, at 4628 and 460. This spectrum is almost identical with that of $+37°3821$ and $+35°4001$.

Pickering, with the 15-inch Cambridge refractor in 1881, observed three bright lines in this spectrum at 545, 486 and 466 —

Photographic: Exposures on ordinary dry plates show bright lines at

4787	4688	4651	454	4317	4504	4480	4467	4442	432	4270	4227
	+	++				+	+	++			

They show Hγ, Hδ, Hϵ to be dark, with no signs of bright edges, such as would exist if the lines were doubly reversed. The visual observations show Hα to be quite bright, with no signs of reversal. Two negatives show what seems to be a very faint band at Hβ, with a fine partially dark line through its center at 4861; but the contrasts are very slight indeed. Photographs on isochromatic plates show all the bright bands observed visually except Hα, and additional ones at 5470, 5135, 5020, 494, 4787. There appears to be a dark line at 4473, between the bright bands 4480 and 4467, possibly coincident with the bright chromosphere line 4472.

I believe that this is the first spectrum in which the Hα line has been observed to be bright and the other hydrogen lines dark. The blue and violet regions of this spectrum are relatively very bright, and the continuous spectrum is visible to K. The altitude of γ Argus when on our meridian is less than 6°.

A visual intensity curve of this spectrum was published by me in ASTRONOMY AND ASTRO-PHYSICS for June, 1893.

Respighi observed a few bright lines in this star at Madras in 1871, but did not identify them.—*Comptes Rendus*, Vol. 74, p. 516.

Ellery, at Melbourne in 1879, observed bright lines at 5760, 5648, 4682.—*Observatory*, Vol. 2, p. 418.

Copeland, at Puno, Peru, in 1883, observed three intensely bright and strong lines at 5809, 5668, 4646, and a slender bright line at 590.—*Copernicus*, Vol. 3, p. 205.

No. 6. Cord. Z. C. 8^h 4141.

				Hβ.			
5812.0	5690	559	542	4862	4690.1	4649.3	454
5814.5	....	......	......	....	4690.3	4652.4	......
5813.1	....	......	......	....	....	....	......
5813.	5690	559	542	4862	4690	4651	454
+						++	

No photograph taken. The altitude of this star on the meridian is less than 6°. I have identified it as Cord. Z. C. 8^h 4141, 9th magnitude.

Copeland, at Puno in 1883, observed a fairly bright line at 573 and an intense one at 4636.—*Copernicus*, Vol. 3, p. 206.

No. 25. Anonymous.

Copeland, at Puno in 1883, observed this to be a 9th magnitude star, with bright lines in its spectrum at 5753, 4643.—*Copernicus,* Vol. 3, p. 206.

No. 30. Cord Z. C. 15^h 4129.

5133	4942	4687
—	—	

These lines very faint and broad. No photograph taken.

No. 31. Cord G. C. 22748.

4690
—

Photographic: 4689, 4637.
+ +

The Hγ and 4473 lines are narrow, dark, with borders slightly brightened, probably double-reversed. There are dark lines at 454 and 451.

No. 32. Cord G. C. 22763.

					Hβ			
5573.2	5411.8	5132	5022	4941	4861.1	4785.4	4685.4	4637.2
5472.3	5413.4	5134	......	......	4860.1	4784.4	4686.1	4637.3
......	......	......	......	......	4861.6	......	4686.8	4638.7
......	......	......	......	......	......	......	4688.0	4637.5
5473	5412	5133	5022	4941	4861	4785	4687	4638
—	+	—	—	—	+	—	++	++

Photographic: 4690, 4638, 4543, 4515, 4473, 4340.
+ + +

The band at 4638 is more prominent than in any of the other stars containing it, and admitted of very accurate observation.

probably one of the borders of a doubly reversed Hβ. The band 469 is too faint to observe accurately visually.

Copeland, at Puno in 1883, observed bright lines at 5809, 5692, 4651.—*Copernicus*, Vol. 3, p. 205.

No. 34. Cord. G. C. 22838.

4693
—

Extremely faint band. No photograph taken. Its altitude on the meridian is only 8°.

No. 35. Cord. G. C. 22843.

Hβ	
4863	4686.7
......	4687.2
(4863)	4687
—	—

Photographic: 4688, 4637. The Hγ line is dark, with bright border, on red side, possibly double-reversed.

The difficult line measured visually at (4863) is possibly a similar bright border on the red side of Hβ.

No. 36. Cord. G. C. 23072.

No bright lines observable.

Photographic: The Hγ line is dark. There is a possible trace of bright 469, but it is very doubtful. Is this star of the Wolf-Rayet type?

No. 37. Cord G. C. 23073.

4689.5
4690.2
4690
—

Photographic: 4688, 4637. Hγ is dark.

No. 38. Cord. G. C. 23416.

										Hβ				
5814.4	5690.9	5590.4	5471	5413	5286	5252	5133	5017.1	4936	4862	4785	4691.7	4652.2	454
5812.8	5693.8	5592.7	......	......	......	......	......	5019.8	4940	......	......	4692.7	4651.4	....
......	......	5590.9	......	......	......	......	......	5021.6	......	......	......	......	......	....
5813	5692	5591	5471	5413	5286	5252	5133	5019	4938	4862	4785	4692	4652	454
++	+	+	+	+	—				—	+	+		++	—

Photographic: 4688, 4651, 4544, 449, 4444, 432.

Photographic:	4688,	4651,	4544,	449,	4444,	432.
	+	++				

Hγ is probably bright; but if so, it is shifted to the violet considerably. 4444 is very broad and possibly double. The band 469

is very faint visually. The greatest altitude of this star is about 7°.

No. 39. Cord. Z. C. 17^h 3612.

5816.0	5695.5	5591.1	5474	5410.8	5249	5132	5021.8	4939	4862	4789	4691.0	4653.7
5815.5	5695.4	5589.4	......	....	......	......	5021.8	4935	......	......	4691.1	4653.2
5818.5	5692.9	....	......	....	......	......	....	......	......	......	....	....
5817	5694	5590	5474	5411	5249	5132	5022	4937	4862	4789	4691	4653
	++	−	−		−			−		−		++

No photograph taken.

No. 40. SDM. — 19° 4854.

	Hβ		
5413	4862	4686	434
	−	++	−

No photograph taken.

No. 41. SDM. — 21° 4864.

										Hβ					Hγ
5814.8	5692.6	5596.1	5471.0	5410.5	5282.0	5241.0	5121.0	5023.0	4940.4	4861.1	4789.0	4655	4543	4443	4338
5810.6	5691.8	5591.0	5470.1	5412.5	5282.4	5248.6	5124.8	5022.5	4939.1	......	4786.5	4646	4546	4442	......
5813.6	5690.6	5592.9	5473.1	5409.9	5289.0	5249.2	5123.0	5022.0	4937.4	......	4785.5	4658	4535	4440	......
......	......	5594.4	......	......	5282.4	......	5122.7	5023.2	......	......	......	......	......	......	......
5813	5692	5594	5471	5411	5284	5246	5123	5023	4939	4861	4786	4653	4541	4442	4338
++	+	+	−	+						+		+−			

Photographic:	469,	465,	454,	4493,	4442,	4389,	4369,	4341.
	+	++	[illegible]	+	++			+

The band at 4442 is very wide and possibly double. The bands at 469 and 465 are unusually wide and overlap considerably.

The visual intensity curve for this spectrum is given on Fig. 2; and the photographic intensity curve on Fig. 3.

Pickering, at Cambridge in 1881, observed bright lines at 582, 470.—*Nature*, Vol. 23, p. 604.

[redacted]

No. 43. DM. + 30° 3639.

Hα	D_3										Hβ					Hγ
6563	588	581	5691.5	559	5412.2	529	5252	5131.3	5022	4941	4861.1	4786	4691.4	4651.5	4444	4340.3
			5693.6		5410.1		5263	5138.5	5017	4938	4861.9	4787	4689.4	4252.8		
			5693.4				5244	5141.0	5020			4786	4687.6	4653.1		
			5695.9				5260	5130.0	5021					4652.6		
			5694.7				5258	5133.3						4649.3		
			5694.3				5249	5137.7								
			5693.0					5130.7								
			5695.0					5133.3								
			5695.3													
			5693.9													
			5693.5													
6563	588	581	5694.0	559	5411	520	5255	5135	5020	4939	4861.5	4786	4689	4652	4444	4340
—			+		—					—	++			+	—	+

Photographic: 4862, 4689, 4653, 4621, 4592, 4555, 4544, 4517, 4474, 4457
++ + ++ + + +

4444, 4416, 4389, 4369, 4341, 4323, 4267.
+ + ++

The lines in this spectrum are better defined than those in any of the other spectra. The brightest lines are the hydrogen lines Hβ and Hγ. They are true monochromatic images of the slit, and the second measure of the Hβ line was made with a second order grating. The line at λ 5694 is well defined, and indeed appears to be monochromatic. I have for that reason determined its wave-length a large number of times. Several of the lines have not been found in any of the other stars; and in many respects, as Mrs. Fleming pointed out* at the time of discovery, its appearance differs from that of the other stars of this class.

I have observed this star to be surrounded by an incandescent hydrogen envelope. This observation was made spectroscopically, and presented no difficulties. The principal features of this spectrum, aside from the continuous spectrum, consist of the bright line at λ 5694, the bright band at λ 4652 and the very bright Hβ line. When the spectroscope was adjusted for the various parts of the spectrum the line at λ 5694 appeared essentially as a stellar point; the band at λ 4652 was broad but short and lay wholly upon the continuous spectrum; but the hydrogen line Hβ *was narrow and long, extending a very appreciable distance each side of the continuous spectrum.* When the slit was

* *Astronomische Nachrichten*, No. 2986.

opened wide the Hβ line became *a circular disc*, while the line λ 5694 and the band λ 4652 remained unchanged. The appearances of the Hβ line with open slit and with narrow slit are shown in the accompanying cut. The diameter of the disc was measured with the micrometer and found to be about 5″. The Hγ line and the very faint Hα line were seen, with open slit, to have similar forms. There can be no doubt that the observed appearances are due to an envelope of incandescent hydrogen surrounding the nucleus which yields the continuous spectrum and the other bright lines. The large size of the envelope may be due in part to the nearness of this star to the solar system; but as to that, no statement can now be made.

The other stars of this type were examined for discs, spectroscopically, but none were discovered.

The superiority of the spectroscopic method for observations of this kind is at once evident. Indeed, the above observation could not be made with the 36-inch refractor, using an ordinary eyepiece. While it was seen that the light in this star is unusually distributed, the effects of the large chromatic aberration could not be eliminated sufficiently to permit the detection of the disc.

A photographic intensity curve of this spectrum is given on Fig. 3.

No. 44. DM. + 35°3953.

4684.5
4689 5
4687.9
4687.4

4687
+

The band observed visually at 4616 is shown by the photograph to consist of two, at 4627 and 4598. This spectrum is almost identical with that of DM. + 37° 3821. [See Fig. 2].

Wolf and Rayet observed bright lines in this star in 1867, but did not identify them.—*Comptes Rendus*, Vol. 65, p. 292.

Vogel, at Vienna in 1883, constructed a visual intensity curve for this spectrum, and observed the following bright lines: 583, very faint; 571, very faint, only suspected; 541, bright line; 486, fairly bright ($H\beta$); 486, brightest place in bright band.—*Publ. Potsdam*, Vol. 4, pp. 18, 21.

Copeland, using the 15-inch Dun Echt refractor, observed a bright line at 5412, a faint line at 5220, and a large blue band at 4695.—*Proc. Roy. Soc.*, Vol. 49, p. 43.

Pickering states that the Draper spectrum photographs show a blue band at 469, and bright lines at 462, 455, 434, 420, 410, 406, 402, 397, 395, 389, 388.—*Proc. Roy. Soc.*; Vol, 49, p. 36.

Dr. and Mrs. Huggins' intensity curve of the band near 469 places the maximum brightness at about 4687.—*Proc. Roy. Soc.*, Vol. 49, p. 34.

No. 46. DM. + 35° 4013.

									Hβ						Hγ
5881.6	5813.8	5691.5	5590.8	5468.3	5412.4	5130	5022	4934	4862	4785	4690.4	4652.1	4546	4440	4340
5876.7	5812.3	5693.5	5594.2	5469.2	5411.7	5127	5015	4939	.....	4787	4689.5	4650.7	4540	4438	.....
5875.6	5812.8	5695.4	5593.8	5468.4	5412.0	5132	5020	4937	.....	4786	4689.7	4652.5	.....	.....	.....
5873.7	.....	.....	.....	5469.8	5412.5	5133	5019	.....	.....	.....	4688.0	4652.9	.....	.....	.....
.....	.....	.....	.....	5469.5	5410.4	.....	.....	.....	.....	.....	4690.4	4652.0	.....	.....	.....
.....	.....	.....	.....	5468.0	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....
5877	5813	5693	5593	5469	5412	5130	5019	4937	4862	4786	4690	4652	4543	4439	4340
	+	+		+	+			—				++		+	

Photographic:

4862	4788	4688	4652	4543	4518	4504	4480	4465	4441
	+	+	++	++	+	+	+	+	++

4343	4334	4316	4277	4260	4227	420	4102	4064	4023
+	+	+	+	+	+		+		

The line at 4510 is pretty certainly double, with components at 4518 and 4504. The lines at 4480 and 4465 may possibly be the very bright borders of a doubly reversed line at 4473, but that is hardly probable. All the lines in this star are really bands of different widths, except possibly those at 4343 and 4334, near $H\gamma$. The appearance at $H\gamma$ is very peculiar; neither of the fine bright lines coincides with the artificial hydrogen line, nor is there any basis for classing it as a double reversal.

The visual intensity curve of this star is given on Fig. 2; and the photographic curve on Fig. 3.

Wolf and Rayet observed four bright lines in this star, and made micrometer readings upon them. Their lines are probably those at 581, 569, 541, 465.—*Comptes Rendus*, Vol. 65, p. 292.

Vogel, at Vienna in 1883, observed very bright lines at 582, 570, and the brightest place in a broad weak band at 464. He also constructed a visual intensity curve.—*Publ. Potsdam*, Vol. 4, pp. 19, 21.

Copeland, at Dun Echt in 1884, observed bright lines at 5824, 5689, 5410 and a large band at 4654.—*Proc. Roy. Soc.*, Vol. 49, p. 43.

Dr. and Mrs. Huggins in 1890 observed a bright band whose maximum brightness is near 4650, and a faint band with maximum near 4690. They likewise observed a group of bright lines at about λ 5690 and a faint line near D_3.—*Proc. Roy. Soc.*, Vol. 49, pp. 34, 44.

No. 47. D.M. + 37° 3821.

Hα	D_3						Hβ					Hγ
6562.8	5876.6	581	569	5411.8	5022.3	4942.3	4861.3	4786.0	4685.2	4628.9	4539.6	4341
....	....	.....	.....	5411.5	5022.3	4940.9	4862.1	4785.7	4685.3	4627.8	4541.3	....
....	....	.....	.....	5410.5	....	4938.4	....	....	4686.8	4628.4	4541.1	....
....	....	.....	.....	5410.7	....	....	....	....	4687.2	4630.8	....	....
....	....	.....	.....	....	....	....	....	....	4686.4	4629.7	....	....
....	....	.....	.....	....	....	....	....	....	....	4631.2	....	....
6563	5877	581	569	5411	5022	4941	4862	4786	4686	4629	4541	4341
				+	+		+	−	++	+		

Photographic:	4862	4787	4687	4650	4627	4598	4541	4508	4481	4465	4442
			++		+	+	++	+			
	4341	420	4102	4062							
	++	+	+								

All these lines are broad except possibly 4598. There is an absorption line at about 4457. The bright Hα line is easily visible. This spectrum is almost identical with that of D.M. + 35° 4001.

The visual intensity curve for this star is given on Fig. 2;

This spectrum strongly resembles that of DM. + 35° 4013.

Wolf and Rayet observed bright lines in this star in 1867, but give no wave-lengths.—*Comptes Rendus*, Vol. 65, p. 292.

Vogel, at Vienna in 1883, observed bright lines 582, 569, and the brightest place of a band at 464. He constructed a visual intensity curve also.—*Publ. Potsdam*, Vol. 4, pp. 20, 21.

Copeland, at Dun Echt in 1884, observed bright lines at 5810. 5704, a faint line at 5233, and a large blue band at 4649.—*Proc, Roy Soc.*, Vol. 49, p. 43.

Dr. and Mrs. Huggins, in 1890, observed the maximum of the bright blue band to be at about 4650, and a faint band at about 4688.—*Proc. Roy. Soc.*, Vol. 49, p. 34.

No. 49. DM. + 36° 3987.

	Hβ				
5414.8	4862	4786	4684.8	4633	454
5411.8	......	......	4687.6	4635	......
......	......	......	4686.1	......	......
......	......	......	4686.6	......	......
......	......	......	4685.4	......	......
5413	4862	4786	4686	4634	454
		−	+	−	

Photographic: 4688, 4636, 4615, 454, 4341.
++

No. 50. DM. + 38° 4010.

581	569	5411.6	4854	4689.2	4631	454
......	......	5410.7	4856	4682.9	4636	......
......	......	5414.3	......	4686.5	......	......
581	569	5412	4855	4686	4633	454
				++	−	

Photographic: 4689, 4637, 454.
++

Hγ is dark with edges slightly bright. It is probably a doubly reversal.

The line observed visually at 4855 was seen to be more refrangible than the hydrogen artificial Hβ, and my notes say "Probably Hβ not present." It may be the bright margin of a doubly reversed Hβ.

No. 51. DM. + 43° 3571.

Hα						Wide Band.
6564.7	5850	5806	569	559	5135	4712 — 4610
......	5848	5801	......	......	5135	4719 — 4612
......	5846	5806	......	......	......	4710 — 4610
......	5849	5803	......	......	......	....
6565	5848	5804	569	559	5135	4714 — 4611
−	+	++			−	+ +

Photographic: 469, 465.
\+ ++

The Hβ, Hγ, and Hδ lines are dark, with slightly brightened broad borders: they are probably double-reversed. The bands at 469 and 465 are broader than in any of the other stars observed. The brighter one, 465, overlaps 469 and is itself about 80 tenth-meters broad and nearly uniform throughout. The line observed as Hα is very faint, rather broad, and may be the bright borders of a dark Hα. It may be, however, that Hα is bright and the other hydrogen lines are dark, as in γ Argus.

The lines in this star are all very broad. Some are also unsymmetrical, and these wave-lengths should have little weight when the results for all the stars are combined.

No. 52. DM. + 36° 4028.

				Hβ			
581	569	5412.7	4938	4861.5	4684.5	463	454
......	......	5413.0	....	4861.2	4688.9	......	......
......	......	5411.5	....	....	4686.4	......	......
......	......	....	....	....	4685.6	......	......
581	569	5412	4938	4861	4686	463	544
			−		++		

Photographic: 4686, 465, 4627, 460, 454
++

No. 53. DM. + 55° 2721.

D_3			Hβ			
5879	581	5413	4860	4689.6	4630	454
....	......	....	....	4689.2	....	......
....	......	....	....	4685.8	4633	......
....	......	....	....	4688.4	....	......
5879	581	5413	4860	4688	4631	454

No. 55. DM + 56°.2818.

	Hβ			
5414.5	4862	4686 4	463	454
5412.5	......	4689 3	......	......
......	......	4688.2	......	......
......	......	4685.6	......	......
......	......	4687.7	......	......
5413	4862	4687	463	454
	−	++		

Photographic: 4688, 4637, 454.
++ +

The wave-lengths of the bright bands observed visually in these stars are brought together in Table II, and those observed photographically (including a few dark lines) in Table III. The means for the several lines have been formed with due regard to the character and relative number of observations of the lines in the different stars. In Table III the dark lines with brightened borders are marked "rev.," signifying possible double reversal.

I wish to emphasize the fact that the lines in these spectra have a great variety of forms and intensities. The hydrogen lines are especially noteworthy: they have nearly every known character. In many of the stars they are dark. Again, they are dark with bright borders, and suggest strongly that they are doubly reversed. The bright hydrogen lines vary from faint to very bright, from monochromatic lines to very broad bands, and from those clearly single to those apparently multiple. The other prominent bright bands are of very different widths and intensities in the different stars. Many of the bands are always faint, as will be seen from the symbols written below their wave-lengths. These symbols and the notes given with the tables of measures for each star make further descriptions unnecessary here.

It will be noticed in Table III that several of the lines do not appear in many stars. That is due, in some measure, to the different degrees of exposure of the various plates; and further to the fact that the star + 30° 3639 appears to be of a somewhat different type from the others. Many apparent lines have been omitted because a second plate has not yet been taken to test their reality.

The photographs show, even better than the visual observations, that the blue bands at 4688 and 4652 are often associated in the same spectrum. The band at 4636 is distinctly separate from those at 4652 and 4626, and is found in several stars.

No	Star.	Hα	D_3										
1	DM. + 63°83				581 −				5413 +				5023 −
2	" + 56°686				5814 ++	5691	5594	5472 −	5412 −		526	5124 −	5020
3	" + 56°731				5812 ++	5691	5594	5470 −	5411				
4	Cord. G. C. 8631	6563 −	588		5807	569	559	5471 −	5416 ++				
5	γ Argus	6565 +	5874 +		5813 ++	5694 ++	5596 −	5470	5411 −			5135 −	5020 −
6	Cord. Z. C. 8h4141				5813 +	5690	559		542				
30	" " 15h4129											5133 −	
31	" G. C. 22748												
32	" " 22763							5473 −	5412 +			5133 −	5022 −
33	" " 22827		588		5812 ++	5693 +	559	5474 −	5414				
34	" " 22838												
35	" " 28843												
37	" " 23073												
38	" " 23416				5813 ++	5692 +	5591 +	5471 +	5413 +	5286 −	5252	5133	5019
39	" Z. C. 17h3612				5817	5694 ++	5590 −	5474 −	5411		5249 −	5132	5022
40	SDM. − 19°4854								5413				
41	" − 21°4864				5813 ++	5692 +	5594 +	5471 +	5411 +	5284	5246	5123	5023
42	" − 11°4593				5815 +	5695 ++	5590 −	5471 −	5410 −		5251 −	5132 −	5022 −
43	DM. + 30°3639	6563 −	588		581	5694 ++	559		5411 −	529	5255	5135	5020
44	" + 35°3953												
45	" + 35°4001		5878		581				5412			5135	5022

No.		Hβ								Hγ
1	4938 —	4861 —		4687 ++			4615 —	454 —		
2		4861 —	4787 —	469	4653 ++			4534 —		
3				469	4654 ++			454		
4	4943	4862 +	4789	4688 ++			4614 +	4544 +		4342
5	494		4787	4689 +	4651 ++				4441 +	
6		4862		4690	4651 ++			454		
30	4942 —			4687						
31				4690 —						
32	4941 —	4861 +	4785 —	4687 ++		4638 ++				
33		(4863) —	4785 —	469	4653 ++					
34				4693 —						
35		(4863) —		4687 —						
37				4690 —						
38	4938 —	4862 +	4785 +	4692	4652 ++			454 —		
39	4937 —	4862	4789 —	4691	4653 ++					
40		4862 —		4686 ++				454 —		
41	4939	4861 +	4786	469	4653 ++			4541	4442	4338
42	4943 —				4653 ++					
43	4939 —	4862 ++	4786	4689	4652 +				4444 —	4340 +
44				4687 +						
45	4939	4862 +	4788 —	4686 ++			4616 +	4538		4341
46	4937 —	4862	4786	4690	4652 ++			4543	4439 +	4340
47	4941	4862 +	4786 —	4686 ++		4629 +		4541		4341
48	4940	4862 —	4785	4691	4652 ++			454	444	
49		4862	4786 —	4686		4634 —		454		
50		(4855)		4686 ++		4633 —		454		
51				{4714 +	broad ++	band	4611}			
52	4938 —	4861		4686 ++		463		454		
53		4860		4688 ++		4631 —		454		
54		4865 —		{469	4651 ++	broad	band}			
55		4862 —		4687 ++		463		454		
	4940	4862	4786	4688	4652	4633	4615	4540	4441	4340

TABLE III.—BRIGHT BANDS OBSERVED PHOTOGRAPHICALLY

No.	Star.	Hβ													
1	DM. + 63°83			4688 ++				461 +5			454				
4	Cord. G. C. 8631			4688 +			4628 +		460		454 +	451			
5	γ Argus.		4787	4688 +	4651 ++							4517	4504		4480 +
31	Cord. G. C. 22748			4689 +		4637 +					dark	dark			
32	" " 22763			4690 +		4638 +					4543	4515			
33	" " 22827			469 +	4652 ++										
35	" " 22843			4688		4637									
36	" " 23072														
37	" " 23073			4688		4637									
38	" " 23416			4688 +	4651 ++						4544			449	
41	SDM. − 21°4864			469 +	465 ++						454 +			4493 +	
42	" − 11°4593			4687 +	4650 ++										4480
43	DM. + 30°3639	4862 ++		4689 +	4653 ++		4621		4592	4555	4544	4517 +			
44	" + 35°3953			469											
45	" + 35°4001	4862	4787	4687 ++	4650		4627 +		4598		4541 +	4508			4481
46	" + 35°4013	4862	4788 +	4688 +	4652 ++						4543 ++	4518 +	4504 +		4480 +
47	" + 37°3821	4862	4787	4687 ++	4650		4627 +		4598 +		4541 ++	4508 +			4481
48	" + 36°3956		4790	4690 +	4651 ++						4540	4510			
[illegible]	[illegible]			[illegible]		[illegible]		[illegible]			[illegible]				

IN THE WOLF-RAYET STARS.

No.								Hγ							Hδ			Hε
1																		
4								4341 ++						420 ++	4102 +			
5	dark	4467 +		4442 ++				dark		432	4270		4227		dark			dark
31	dark							dark rev?										
32	4473							4340 +										
33								dark rev.										
35								dark										
36								dark										
37								dark rev.										
38				4444						432								
41				4442 ++		4389	4369	4341 +										
42				4442 +														
43	4474 +		4457 +	4444 +	4416 +	4389	4369	4341 ++		4323	4267							
44	dark				dark	dark		dark		dark								
45		4465		4442			4369	4341 +						420 +	4102 +	406		
46		4465 +		4441 ++				4343 +	4334 +	4316 +	4277 +	4260 +	4227 +	420	4102 +	4064	4023	
47		4465		4442			4369	4341 ++						420 +	4102 +	4062		
48				4442 ++				4340		4314	4277	4260	4229					
49								4341										
50								dark										
51								dark rev.							dark rev.			
52																		
53																		
55																		
	4473	4466	4457	4443	4416	4389	4369	4341	4334	4318	4273	4260	4228	420	4102	4063	4023	

Without going further into details, it is now a question of identifying the lines and bands with the lines of known elements, and of assigning to these stars their place along with other types of celestial objects. A most perplexing question!

The hydrogen lines $H\alpha$, $H\beta$, $H\gamma$, $H\delta$, are present, but the other lines do not admit of certain identification. Prominent iron and other lines may coincide with a few of the star lines, and the line at 4480 suggests a magnesium origin; but there are not enough points of identity with well-known artificial or stellar spectra to enable us to draw any safe conclusions.

The presence of bright lines in these stars suggests a comparison with other bright-line spectra. Professor Pickering has pointed out* that a probable close relation exists between them and the "Orion type" stars, four-fifths of which are in the Milky Way, and the planetary nebulæ, nearly all of which are in the Milky Way. It has also been suggested that the Wolf-Rayet stars are closely related to the "new stars." In order to facilitate the comparison of these and other spectra, I have constructed Table IV. The wave-lengths of the forty-five bright bands observed in the Wolf-Rayet stars are given in the first column. Opposite to them are placed possible corresponding bright lines in the solar chromosphere, Nova Aurigæ (February, 1892),† and the dark lines in the "Orion type" stars and β Lyræ.

The hydrogen lines are prominent in all the six spectra. The D_3 and 4472 chromosphere lines appear, within the limits of error of the measures, to exist pretty prominently in all these types of spectra except that of Nova Aurigæ. D_3 was not observed at all in the latter spectrum, and 4473 was far from prominent (*schwer aufzufassen*).

If we omit the hydrogen, D_3 and 4472 lines, the remaining possible coincidences with solar chromosphere lines are of extremely small weight. The chromosphere lines 5285 and 5019 are prominent, but they coincide with two of the faintest Wolf-Rayet lines. The other chromosphere lines are comparatively faint.

If we compare columns one and three we shall find that the coincidences are equally unsatisfactory. The prominent Wolf-Rayet lines are conspicuously absent from the spectrum of Nova Aurigæ, of Nova Normæ, and probably also of Nova Cygni; and the converse is likewise true. There are several possible coincidences, but on the whole they relate to faint lines in one or both of the spectra; and the great majority of the lines in the two

* *Astronomische Nachrichten*, No. 3025.

† Using the Potsdam and Lick lists of bright lines.

Table IV.—Comparison of Wolf-Rayet and other Spectra.

Wolf-Rayet Stars.	Solar Chromosphere.	Nova Aurigæ.	Gaseous Nebulæ.	Orion Stars Dark Lines	β Lyræ Dark Lines.
6564	6563 Hα	6563	6562		
5877	5876 D_3		5876	5876	5876
5848					
5813					
5698					
5593					
5472					
5412			5412		
5284	5285 Fe	5285			
5250					
5131					
5020	5019 Fe	5016		5021	[5014]
4940					
4862	4861 Hβ	4862	4861	4861	4861
4787					
4688			4687	4688	
4652				4652	4652
4636					
4626	[4630 Fe]	[4629]			4630
4615					
4596					
4555	4556 Fe	4556			4556
4541				454	4544
4517					
4509		4507			4510
4504					
4493	4490 Fe	4490			
4480	4481 Mg	4481		4481	4481
4473	4472	4473	4472	4472	4471
4466					
4457					

Wolf-Rayet Stars.	Solar Chromosphere.	Nova Aurigæ.	Gaseous Nebulæ.	Orion Stars. Dark Lines.	β Lyræ Dark Lines.
4442	4444 Fe	4445			
+ +		+			
4416		4418		4416	
		+			
4389			4389	4389	4388
+			+	+	
4369					
4341	4341 Hγ	4341	4341	4341	4340
+ +	+ +	+ +	+ +	+ +	
4334					
4318		4316			
		+			
4278					
4260		4262			
		+			
4228		4228	423	423	
420				4203	
4102	4102 Hδ	4102	4102	4102	4102
+ +	+ +	+ +	+ +	+ +	
4063		4067	4067	4067	4062
+		+	+	+	
4023			4026	4026	4026
+			+	+	

spectra do not coincide with each other. Aside from the fact that the lines in the "new stars" and Wolf-Rayet stars are broad, these two types do not seem to have many points in common.

The second and third columns of the table are nearly identical, and the comparisons with the first column lead to similar conclusions. That was to be expected since, it will be remembered, the February, 1892, spectrum of *Nova Aurigæ* was largely chromo-

say that if any relation exists between these spectra, it is not clearly established and its nature is not apparent.

A comparison with the dark lines in the Orion type stars gives substantially the same results as obtained for the nebulæ, as would be expected from the close relationship of the nebular and Orion type spectra.* The presence of the lines 4688, 4652, 454, 4389, 4067 and 4026 in the two spectra must be given great significance. However, numerous strong lines in the Orion stars have not been found in the Wolf-Rayet stars and *vice-versa*, though careful search was made for them. The Orion star lines in column five are taken from my photographs of β, δ, ϵ, Orionis and six stars in the dense parts of the Orion Nebula. I have not included lines contained in the spectrum of α Cygni: they are so numerous that, within the limits of observation errors, lines could be selected to match nearly all the Wolf-Rayet lines; but the same could be said of many other dark-line spectra. In looking for points of correspondence with the Orion type of spectrum, we should begin with the simpler types, such as that of δ Orionis, and work up gradually to the complex types, such as that of α Cygni. Until such observations are obtained, with wave-lengths accurate to four places, it does not seem advisable to hold that a closer relation exists between the Wolf-Rayet and Orion stars than between the Wolf-Rayet stars and the nebulæ.

The spectrum of β Lyræ as observed by Dr. Belopolsky, Professor Vogel, Father Sidgreaves and others, shows considerable resemblance, in a general way, to the Orion type; and comparison with the Wolf-Rayet spectra leads to substantially the same results as were obtained for the Orion stars.

In conclusion, I think we can say, from the foregoing observations, that the spectra of the Wolf-Rayet stars are not closely related to any other known type. They appear to have several points in common with the nebular and Orion type spectra; but the last two appear to be much more closely related to each other than to the Wolf-Rayet spectra. It is therefore difficult to place these stars between the nebulæ and Orion stars. They certainly do not come *after* the Orion stars, and one does not like to place them *before* the nebulæ. We can probably say that the bright lines are chromospheric, owing their origin to very extensive and highly heated atmospheres, but showing very little relation, in constitution and physical condition, to that of our own Sun. For the present, at least, this type of spectrum must be consid-

* See A. AND A.-P. for May, 1894, pp. 393-7.

ered as distinct from every other known type, just as the nebular spectrum is distinct, and like the nebular spectrum containing lines whose origin cannot now be assigned.

I wish to acknowledge having received a great deal of valuable assistance during the progress of the observations from Professor Holden, who was always ready to advance the work in every possible way. Likewise the large number of observations secured in the relatively short time devoted to the work is due to the efficient assistance received from Mr. H. D. Stearns of Stanford University, Dr. Mary Cunningham of Swarthmore College, Mr. S. D. Townley of Detroit Observatory, Secretary C. D. Perrine and Assistant Astronomer A. L. Colton of this Observatory. The last three have each assisted on from ten to twenty nights.

LICK OBSERVATORY, 1894, April 18.

ON THE SPECTRA OF THE ORION NEBULA AND THE ORION STARS.

JAMES E. KEELER.*

Notwithstanding the attention that spectroscopists have bestowed on the great nebula of Orion, there are still many unsolved questions relating to this most interesting object which may be dealt with by instruments of moderate power. The apparent simplicity of the spectrum first observed by Huggins in 1864 has been shown by the same eminent investigator to be due merely to the faintness of its light, the spectrum revealed by photography being one of considerable complexity. It is not yet

to which it can be traced by visual observation, and that many of the brighter stars in the same constellation, even those remotely situated, are involved in its outlying streamers. The inference is that the Orion stars occupy a place near the beginning in the scale of stellar development, and a study of the spectra of these stars, and particularly of any analogies that they exhibit with the spectrum of the nebula from which they have presumably been evolved, becomes of special importance. Some analogies of this kind have been traced, but our knowledge in this direction is very fragmentary, and, as I shall show, some conclusions that have been generally accepted arecertainly erroneous.

For the purpose of throwing some light, if possible, on the interesting questions briefly alluded to above, I photographed the spectrum of the Orion nebula many times during the past winter with the spectroscope and 13-inch equatorial of the Allegheny Observatory, and although the results are far short of what I hoped to obtain, and are not at all in proportion to the considerable amount of time which has been expended on them, some facts of importance have been clearly brought out. The slowness of the work of observation is mainly due to the smoky atmosphere which surrounds this unfortunately situated Observatory, and which is particularly opaque to the photographically active rays. Nights on which photographic work in the upper spectrum can be pursued to advantage are of infrequent occurrence.

The spectroscope used in these observations was described in the January (1893) number of ASTRONOMY AND ASTRO-PHYSICS. The arrangement for photographing with a single prism is not illustrated in the plates; it is the same as the visual arrangement, except that the camera, of sixteen inches focus, is inserted in the place of the short observing telescope. In order to increase the stability, some light brass tubes, extending from the clamps in the frame near the slit to the clamps near the outer end of the camera, were added to the instrument. They are like the braces used with the prism train, except that they are very much longer, and they are secured in the same manner. By these rods the stability of the instrument was greatly increased; still the camera when used with a single prism is in a position unfavorable to rigidity, and care was therefore taken not to greatly change the position of the instrument during an exposure. By confining the observations to an hour angle of two hours on each side of the meridian, (and in the case of the Orion nebula an exposure of four hours was possible without reversing the telescope), the effects of flexure were

prevented from becoming sensible. The effects of rapid changes of temperature were much more annoying.

All photographs of the spectrum of the nebula were made with a 60° prism of white flint glass. Some photographs of star spectra were made with a dense prism with considerably greater dispersion, and on one occasion the prism train was employed. The spectroscope has in all its essential features shown itself to be entirely satisfactory, and well adapted to the various purposes for which it is used. Unfortunately the same statement cannot be made with regard to the telescope. The figure of the objective is good, but the glass is quite yellow, the crown lens in particular being so yellow that I cannot help thinking that it must have undergone a change in color since it was made, some twenty-five years ago. For most kinds of visual observations this yellow tint of the objective is rather advantageous than otherwise, as it cuts down the secondary spectrum, but it is obviously anything but an advantage in photographic work The mounting of the telescope has been brought up to modern requirements as nearly as circumstances permit, and is quite satisfactory.

The spectrum of the nebula on the photographic plate was about an inch long (23 mm. from the chief nebular line to the farthest line in the ultra-violet); its breadth was varied to suit the requirements of observation.

The object of my earlier observations, some of which were made in the winter of 1892-93, was to reproduce, if possible, the anomalous spectrum obtained by Huggins in 1889, and to determine the position and extent of the regions in which this spectrum might be found. In the spectrum photographed by Huggins, to which reference is made above, no trace of the hydrogen lines Hδ and Hϵ could be found, although Hγ was strong, and a large number of fine lines, apparently connected with the spectra of the trapezium stars, were visible; the strong line previously photographed, at λ 373, was absent. In 1890 the spectrum of nearly the same region was of the usual character. Although this sudden termination of the hydrogen series, evidently not the result of increasing faintness of the lines, seems very remarkable, the extraordinary fact discovered by Campbell, that both bright and dark hydrogen lines can exist in the same spectrum and in the same series, as well as less striking anomalies which have been remarked by other observers, show that *a priori* reasoning on the probability of such an occurrence must be used with great caution. I therefore tried to obtain the same result by repeating the observations of Huggins.

In the photographs which I took for this purpose, the slit was always placed in an east-and-west direction, and it was opened to its full length, so that it extended entirely across all the brighter part of the nebula. The width was .005 inch. With the aid of the finder of the equatorial, the center of the slit was kept in the hour circle passing through the trapezium, and in a parallel of declination which had been previously decided upon. The declination was altered at each exposure, and thus by successive steps the whole of the brighter region in the vicinity of the trapezium was fairly well covered. On account of bad weather many of the photographs were failures, and had to be repeated.

I expected that the region in question, if it fell upon the slit during one of these exposures, would reveal itself on the plate by the presence of breaks or gaps in the hydrogen lines above Hγ and in the line λ 373, and from these gaps the position of the region could be determined. No such appearances were, however, to be found on any of the photographs; the spectra were essentially all of one type, and the differences which they exhibited were only such as would naturally seem to accompany the varying brightness of the source. The type referred to is a spectrum in which the hydrogen lines are very strong, the series toward the violet falling off gradually to invisibility; the first and second nebular lines of the visual spectrum are also strong, especially with orthochromatic plates, and the ultra-violet line at λ 373 is distinct, although, (with my apparatus), not nearly equal to the lower hydrogen lines in strength. These lines represent by far the greater part of the radiation from the nebula.

It appears then from these observations, that the area of the region in question is very small, or that the spectrum of the nebula is subject to rapid and considerable changes. There remains the possibility that the abnormal spectrum of 1889 was an idiosyncrasy of the particular plate on which it was obtained —an explanation which is suggested by various puzzling phenomthat I have come across in my own experience, but one which I should be much more ready to adopt if the photograph had been one of my own making.

The photographs made in connection with this investigation do not show any remarkable differences in the spectrum east and west of the trapezium; on the preceding side the lines are stronger, corresponding to the greater brightness of the nebula in that region, and fainter lines appear, but the differences seem to be merely such as depend upon brightness. The variations in the density of the lines, found by Dr. Huggins to be due to the

irregular brightness or mottled surface of the nebula, is not very noticeable in my photographs, probably because the telescope was not directed in right ascension with sufficient accuracy by means of the finder to prevent a drift of the image; moreover, the slit-plate was in the focal plane of the $H\gamma$ line, and the images of the nebula corresponding to the other hydrogen lines were somewhat out of focus.

One of the objects of these observations was to determine as accurately as possible the positions of the fainter nebular lines, which are still subject to considerable uncertainty.* For this purpose the same arrangement of apparatus was employed; but the slit was shortened to about one millimetre, and after the exposure to the brightest part of the nebula, the telescope was either turned to the Moon, or allowed to remain untouched until morning, and the solar spectrum was then photographed with a longer slit, the central millimetre being covered by the metal tongue described in a previous account of the Allegheny spectroscope. The result was two parallel solar spectra about a millimetre wide and the same distance apart, connected by the sharp, black lines of the spectrum of the nebula.

The plates obtained in this way were measured with a small comparator by Zeiss, which had been exhibited at the Chicago Exposition, and which I purchased for the Observatory. It consists essentially of a strong stand, holding two fixed microscopes provided with eye-piece micrometers, and a sliding stage with a range of one decimeter. The photograph to be measured is mounted on one end of the sliding stage and viewed with one of the microscopes; the other microscope serves to read the scale, which is cut in a strip of silver let into the stage. One revolution

The nebular lines were observed by bringing them between the wires; the solar lines by bisecting them with each wire in succession.

This instrument is well adapted to the measurement of small photographs. As compared with such a measuring instrument as that used at Potsdam, it has the disadvantage of requiring two settings,—one with the viewing and one with the scale microscope; on the other hand the photograph can be moved rapidly through the field of the viewing microscope, and this motion, in addition to its convenience, is of advantage for detecting faint lines, which would otherwise escape notice.

Wave-lengths were determined from the photographs in the following manner: one of the plates, on which the solar spectrum was particularly sharp, was chosen as a standard, and from measurements of a large number of solar lines between F and λ 370 a wave-length curve was platted, on a scale large enough to bring out the errors of measurement. As plates taken on different nights and at different temperatures were generally made with slightly different adjustments of the instrument, their scales did not exactly correspond with that of the standard plate, moreover the nebular lines were slightly displaced relatively to the solar spectra, owing principally to changes of temperature in the interval between the exposures, and to motion in the line of sight. For any plate, measures of a considerable number of the standard lines gave the reductions to the scale of the standard plate, and the measured displacements of the sharpest hydrogen lines of the nebula gave the small corrections, nearly constant throughout the spectrum, which were to be applied to the positions of the nebular lines, and in which were included the effects of flexure, changes of temperature, curvature of the lines, and motion in the line of sight.

Notwithstanding the small scale of the photographs, the wave-lengths could be determined in this manner with considerable accuracy. For sharply defined lines the results of different plates differed by but a small fraction of a tenth-metre; for faint lines they were greater, but the different measures were generally within one tenth-metre; in some cases the discrepancies exceed this limit. At the end of the present article I have indicated the manner in which I think extreme accuracy will finally be reached.

On account of the unfavorable conditions already referred to, it was difficult to obtain in one evening a photograph sufficiently intense to show the fainter nebular lines, and I therefore tried to see how far it might be possible to compensate for these disad-

vantageous circumstances by continuing an exposure from night to night, in the manner that has become familiar in ordinary stellar photography. The experiment was not a success. Several cloudy nights often intervened between the exposures, the temperature meanwhile varying greatly, and the result was a series of partly overlapping images or confused lines, the intensity of which was not specially greater than that of any single image, while the photograph was unfit for accurate measurement. The longest exposure given to any plate was 14^h 45^m on four different nights in February, the slit was wide, and the lines are strong but very much blurred. One plate, exposed 7^h on two consecutive nights which happened to be of nearly the same temperature, is fairly good, but the sky was quite smoky at times, and a considerable part of the exposure certainly counted for very little. On the former of these occasions an orthochromatic plate was used, in the hope of catching the D_3 line, or some of the lines in the lower spectrum described by Taylor,* but without result in this respect, although the chief nebular line is very black and strong and the maximum sensitiveness of the plate falls at some distance below this point, at about midway between *b* and D. It is to be noted that the absorptive action of the atmosphere and of the glass of the objective is far less marked in this region than near Hγ. While the D_3 line falls too near the lower limit of sensitiveness to make its non-appearance worthy of remark, it would seem that the other lines, if they really exist, should be seen in this photograph, although it certainly is not easy to estimate their relative photographic and visual values.

As I have not been able to find any data regarding the slit-widths used by different observers, some experiments which I made in this connection may not be without interest. According to the principles of the undulatory theory, the full illumination of bright line is reached when the slit, as seen from the optical centre of the collimator, subtends the same angle as one wave-length of light at a distance equal to the (effective) collimator aperture. With my instrument this slit width is about .0003 inch. Experiment showed, however, that with the same exposure the density of the photographed lines began to fall off sensibly when the slit-width was reduced below .001 inch, or when it was still three times the theoretical width. In these photographs the exposures were comparatively short and and there was no evidence of shifting of the image. The discrepancy is, I suppose, to be attributed

* *Monthly Notices*, R. A. S., vol. XLIX, p. 126.

to the effect of spreading of the photographic image, each particle of silver being deposited partly by the influence of neighboring illuminated particles, which are fewer in number when the slit is narrow; the physiological effect on the retina which makes a line appear brighter when the slit is widened in visual observations, seems to be analogous to this photographic action. The slit-width which gave the best results was about .0015 inch.

In the following table are given the results of my measurements. The estimated intensities are given for an orthochromatic plate obtained on January 25, 1894, 100 representing complete opacity of the silver deposit, and 1 a scarcely visible trace. While these numbers have no special physical significance, depending as they do upon the atmospheric conditions, the absorption of the instrument, and many other factors, they serve to convey an idea of the appearance of the plate, and they illustrate the very great differences in the intensities of the lines. It is evident that almost all the light of the nebula is derived from hydrogen and the substance or substances yielding the two principal lines of the visual spectrum.

On one of the best negatives taken on February 10, 1894, the ultra-violet line at λ 373 is distinctly double, the upper component being somewhat the stronger. I have been unable to account for this circumstance, as the other hydrogen lines are single and sharp, and so are the solar lines of the comparison spectrum in the same region. On all other photographs the line is single, and I have therefore ascribed its appearance on the negative of February 10 to some accidental cause, perhaps a shred on the plate. According to my experience, faint details on a single negative are to be regarded with some suspicion, particularly if the plate has received insufficient exposure. In the table I have, however, given the measures of the two components of this line on the negative referred to, as well as the position of the single line from the mean of other negatives. The hydrogen lines up to and including $H\epsilon$ have been used in determining the corrections for displacement, and of course their wave-lengths have been assumed as known; above $H\epsilon$ they are determined from the measurements. The wave-lengths of the two lowest nebular lines are also assumed, as the accuracy of my former visual measurements with high dispersion much exceeds that of the present photographs.

POSITIONS OF LINES IN THE SPECTRUM OF THE NEBULA OF ORION.

Wave-length.	Estimated Intensity	Remarks
3728.91		Photograph of Feb. 10
3725.51		
3726.5	35	Other photographs.
3800	1	Hθ
3814	1	Very faint line, possibly accidental
3836	6	Hη
3868.9	20	
3889.2	20	Hζ
3970.0	40	Hε
4026	4	
4069	5	
4102.0	60	Hδ
4340.6	100	Hγ
4363	5	
4471.2	10	
4650 ±	3	Position estimated
4716	4	
4861.5	40	Hβ
4959.0	30	Second nebular line.
5007.1	100	Chief nebular line

The line at λ 446 appears upon only one of the photographs, which was taken with a rather wide slit, and with long exposure. The lines are confused, and unsuitable for measurement. There is a streakiness in the spectrum at several places in this region, on the background of continuous spectrum from one of the trapezium stars, which seems to indicate that other lines might be brought out under more favorable conditions.

On several of the negatives made, in the course of the investigation first mentioned, with a rather wide slit, into which the stars of the trapezium had occasionally wandered, there was a mottled appearance in the spectrum just below the Hγ line, which I should have attributed to photographic defects if it had appeared on a single negative; but as it was evidently real, I concluded that it was caused by the presence of both dark and bright lines, blended into a confused image by the wide slit and imperfect guiding. This seemed to be the only satisfactory explanation, notwithstanding the fact that the trapezium stars have hitherto been supposed to give spectra in which the nebular lines are bright. Another plate, taken with a narrow slit, (.0015 in.), accurately directed to the brightest star in the trapezium, justified this conclusion; broad diffuse dark lines were seen in the star spectrum, some of which were traversed centrally by the bright lines of the nebula, so that they presented much the same appearance as the hydrogen lines in the spectrum of γ Cassiopeiæ or Pleione.

The following table contains a list of these lines. As they are very broad and diffuse, their wave-lengths could not in general be measured with much accuracy.

DARK LINES IN THE SPECTRUM OF THE TRAPEZIUM STAR BOND 628.

Wave-length.	Remarks.
3970	Hε. Crossed by bright nebular line.
4102	Hδ. " " " " "
4201	Doubtful.
4341	Hγ Crossed by bright nebular line.
4369	Suspected.
4388	Strong dark line.
4471	Crossed by bright nebular line.
4542	Strong dark line.
4650	" " "
4685	" " "
4716	" " "
4862	Hβ. Crossed by bright nebular line.

The spectra of other stars in the trapezium were also photographed, and while the results were imperfect, on account of the faintness of the stars, and for other reasons mentioned below, they sufficed to show that the spectra are all of essentially the same type.

In 1890 I had already found dark hydrogen lines in the spectrum of ϑ_2 Orionis (Bond 685) by visual observation with the Lick telescope. Hence it seems to me that Espin's observation of bright lines in the spectra of ϑ_1 and ϑ_2 Orionis, announced in Wolsingham Observatory Circular, No. 26*, must be a mistake.

The lines in the above table are prominent lines in the spectra of other Orion stars, and their evident relation to the bright lines of the nebular spectrum led me to examine other stars in the same constellation, with the purpose of tracing these spectral analogies still farther. Professor Scheiner had already pointed out† that a line in the nebular spectrum measured by Copeland at λ 4476 was probably identical in position with a strong line which is characteristic of the Orion stars, and which according to the Potsdam photographs has a wave-length of 4471.36. According to my measurements the wave-length of the nebular line is 4471.2, Campbell‡ makes it 4473 in the Nebula of Orion and 4472 in some other nebulæ. There is no doubt therefore that Scheiner is right in his conjecture, and that the lines are identical in origin. But in the case of Bond 628, the coincidence of these lines is shown more beautifully by inspection

* A. N. 2963.

† *Spectralanalyse der Gestirne*, p. 267.

‡ ASTRONOMY AND ASTRO-PHYSICS, October, 1893.

of one of my photographs than by any process of measurement. The sharp nebular line fades away toward the star spectrum, and stops just before reaching it, pointing like an arrow exactly at the center of the gap in the continuous spectrum.

In photographing star-spectra with a visually corrected telescope, difficulties are met with which do not occur when the object is an extended surface like the nebula of Orion. In the upper part of the spectrum the focus changes rapidly with the wave-length, and only one set of rays can be accurately focused on the slit. Hence the proper exposure can be given to only a very limited range of spectrum, all other parts being under-exposed and weak. The only remedy is to make a number of different exposures with the slit-plate in different positions; but this piece-meal representation of the spectrum by a series of negatives is a very imperfect substitute for the single plate which would be obtained with a photographically corrected telescope. Estimates of the relative intensities of lines are also seriously embarrassed. These remarks do not apply to the lower part of the spectrum which can be photographed with orthochromatic plates. Here the case is reversed, and the visual telescope has the same advantage that the photographic telescope has in the blue and violet regions.

In photographing star-spectra I followed the same method as in the case of the nebula, a solar comparison spectrum being photographed on each plate. Both prisms were used at different times, and the small dispersion and other circumstances already referred to gave rise to some differences in the appearance of the spectra, as compared with those obtained with higher dispersion. In the main they are no doubt purely photographic. Thus, with a single prism the various intensities of lines in the spectrum of Rigel appear as differences in breadth; while with the prism train, which was used on some occasions, the lines are of the same breadth but differ in intensity, having the appearance described by Professor Scheiner.* The hydrogen lines in the spectrum of ζ Orionis are, according to Scheiner, so diffuse and faint that they are barely visible; on my plates, taken with small dispersion, they are very conspicuous.

It was obviously a matter of the greatest interest to determine whether the chief nebular lines at λ 4959 and λ 5007 are reversed in the spectrum of any of the Orion stars. On all the plates which were good enough to be of real value in deciding this point

* *A. N.* 2923.

—namely, those of β, γ, ϵ and ζ Orionis,—no traces of dark lines at these places could be found; the star spectra were continuous.

On photographs of the lower spectrum of Rigel a strong line was shown just above the D lines of the comparison spectrum. The analogies with the nebular spectrum already discovered, together with the fact that the D_3 line has been observed in the spectrum of the Orion nebula, led me at once to suspect that this line was D_3, and on account of the interest that attaches to everything connected with this remarkable line, I took some pains to measure its position with all possible accuracy. It is true there is a general belief that D_3 is never reversed in star-spectra* but our knowledge of the lower spectra of the stars is very imperfect. I have recently shown that there is reason to suppose that D_3 is dark, as well as bright, in the compound spectrum of β Lyræ.†

The method of determining wave-lengths hitherto employed was inapplicable in this case, for the lower spectrum of Rigel is almost blank, and when the D lines were brought to the centre of the field, and the instrument was specially adjusted for this part of the spectrum, only the F line was common to both spectra, the other hydrogen lines being off the plate, hence the relative displacement of the spectra could not be determined with sufficient exactness. The lines of sodium were therefore photographed on the plate as a comparison spectrum, and in reducing the measures, allowance was made for the effect of motion in the line of sight. I also made some visual measures with the single dense prism and with the prism train, and finally photographed the spectrum with the prism train, with the sodium lines for comparison. With this last arrangement the stability of the instrument is very great, as I think is apparent from the construction as shown on plate VII in No. 111 of this journal. It is shown by the great sharpness of the lines in the lower spectrum of a star like Arcturus when photographed with the same arrangement, and I have tested it directly by experiments with the solar spectrum, showing that the displacement caused by *twice* the *maximum* flexure is all but invisible under the microscope. The scale of the photographs is a little greater than that of the Potsdam photographs in the same part of the spectrum. The distance between tke D lines is 0.153mm.

An excellent photograph was obtained with this arrangement on the evening of March 23. The exposure was $1^h\ 30^m$ with a

* *Spectralanalyse der Gestirne*, p. 277

† ASTRONOMY AND ASTRO-PHYSICS, April 1893.

slit-width of .001 inch. A feeble sodium flame was held in front of the slit for 3 minutes before the exposure and again for 3 minutes after the exposure was completed. Both the star and the sodium lines were sharp, and suitable for exact measurement. The reduction was effected with the aid of a solar spectrum taken on the following morning.

In allowing for the effect of motion in the line of sight, I have used the motion of the star relatively to the Sun as determined by the Potsdam observers. The total motion to be allowed for is then as follows:

Earth's orbital motion..............................	+ 15.11	miles.
Diurnal rotation (hour angle + 3^h)........	+ 0.16	"
Motion relative to Sun (Vogel)	+ 10.14	"
	+ 25.41	"

This velocity corresponds at D_3 to a displacement of 0.80 tenth-metres toward the red. The apparent wave-length of the star-line, from the measurements, was 5876.78; hence the true wave-length is 5875.98.

The results of the different measures are given below:

March 16, 1894.	Single prism, visual observations; weight 1............	λ 5875.9
March 20, "	Single prism, photograph, weight 2........................	5875.8
March 21, "	Single prism, photograph; weight 2	5876.4
March 23, "	Prism-train, visual observations; weight 10.......	5875.98
March 26, "	Prism-train, visual observations, weight 2.	5876.1
	Mean by weights	5876.02

The assignment of weights is somewhat arbitrary. There is no doubt, however, that the determination with the prism train greatly exceeds the others in accuracy, and with this fact taken into account, any other system of weights gives practically the same result.

According to Rowland, the wave-length of the D_3 line is 5875.98; hence I think there can be no doubt that the dark line in the spectrum of Rigel is actually D_3.

Some other lines below D_3 were seen in the visual observations, and appear on the photographs; they are much fainter than D_3. I believe that the D lines of sodium are present, but very faint, although I am not certain on this point; in my best photographs they are obscured by the intense lines of the artificial sodium spectrum. As no list of lines in the lower spectrum of Rigel has been published, I give below the results of my photographic measures. The wave-lengths are generally subject to some uncer-

tainty, and the relative strengths given for the lines have little meaning, for reasons which have already been stated. Above Hγ I have not thought it worth while to give any measures, as this region has been covered by the Potsdam observations, the accuracy of which is much greater than that of mine. In the future I hope to amend the results in the table by repeating the observations with the prism train.

LINES IN THE LOWER SPECTRUM OF RIGEL.

Wave-length.	Remarks.	Wave-length.	Remarks.
4341	Hγ. Very strong. Neb.	4924	Fairly strong.
4252	Very weak.	5016	Strong.
4389	Neb. Fairly strong.	5033	Very weak.
4418	Very weak.	5056	Weak.
4425	Very weak.	5168	Fe? Fairly strong.
4439	Weak.	5316	Weak.
4448	Weak.	5454	Weak.
4471	Neb. } Strong equal pair	5876	D_3. Neb. Very strong.
4481	Mg } Strong equal pair	5890?	D_2 } Suspected.
4509	Very weak.	5896?	D_1 } Suspected.
4548	Weak.	5902	Weak.
4583	Weak.	5925	Weak.
4714	Neb. Strong.	5950?	Suspected; weak.
4861	Hβ. Neb. Very strong.		

In this table lines coinciding in position with the bright lines of the nebula are marked "neb." A line at λ 4389 does not appear in my photographs of the spectrum of the nebula, but a line in this position has been photographed by Campbell.* The strong line λ 4481 is also strong in the spectrum of Sirius. Scheiner, who makes its wave-length 4481.4, identifies it with the solar line 4481.41, and regards it as probably due to magnesium. No traces of the characteristic magnesium group *b* are however to be found on my photographs. The only line in the region is nearly coincident with b_4; it is perhaps the iron line λ 5167.69, which is relatively stronger in the arc specrum than in the Sun; but I am inclined to doubt whether any of the lines in the spectrum of Rigel are due to iron.

The relationship between the bright nebular lines and dark lines in the spectra of the Orion stars is more apparent when the spectra are exhibited in a drawing, with proper attention given to the relative intensities of the lines, than it is when the comparison is made by means of tables. I shall however content myself with a review of the nebular lines, pointing out for each line the coincidences with dark lines, as determined from the photographs.

* ASTRONOMY AND ASTRO-PHYSICS, Oct. 1893, p. 723.

Comparison of Nebular Lines with Dark Lines in the Spectra of the Orion Stars.

λ 3726.5. This is the most refrangible line that appears on my photographs, and at the same time it is a strong one. Observers whose instruments are specially free from selective absorption in the violet have described it as the strongest line in the photographic spectrum. The same line is found in planetary nebulæ. It would be of great interest to determine whether there is a corresponding dark absorption line in the star spectra; but unfortunately the difficulties of photographing at so short a wave-length with a a visual telescope are very great, and I have not succeeded in obtaining any negatives that are good enough to decide this point, nor have I found any published record of measurements in this part of the spectrum of Orion stars.

λ 3800, Hϑ (3798.1). In the nebular spectrum the hydrogen series fades off gradually toward the violet, and no doubt still higher members would appear with longer exposures under more favorable circumstances. As the hydrogen lines are found in practically all star spectra, their presence in the Orion stars has no special significance in this connection. No mention will therefore be made of the remaining hydrogen lines.

λ 3814. (?). This is a very faint line on one of my photographs, and it may be accidental. If real, it is probably identical with Pickering's "Orion line" λ 382

λ 3868.9. A strong line forming an equal pair with Hζ. It is above the limit of my star photographs.

λ 4026. A strong isolated dark line is found at this place in the spectrum of Rigel (λ 4026.6, Scheiner). The same line is very strong and broad in ζ Orionis, and strong in ε Orionis, no other strong lines being in the vicinity in either case. It is also strong in γ Orionis, the nearest line at all approaching it in intensity being 17 tenth-metres above.

λ 4069. This nebular line is represented by a fairly strong dark line in ζ Orionis, many weaker lines being in the vicinity. It is strong and diffuse in ε Orionis, weak in γ Orionis, although stronger than neighboring lines; in Rigel it does not appear on my photographs, and it is not in Scheiner's catalogue of lines in the upper spectrum of this star.

λ 4130. Professor Lockyer* has photographed a number of presumably very faint lines in the spectrum of the Orion nebula,

* Phil. Trans., Vol. 184.

having the wave-lengths 4045, 4130, 4142, 4155, 4167, 4188, 4200, 4268, 4690, 4735. With the exception of λ 4142, these lines do not seem to be uniformly represented in the spectra of the Orion stars, unless the lines are too weak to appear on my photographs. There is a fairly strong line in ζ Orionis at λ 4143.5, and a broad weak line at λ 4187. λ 4200 (4201, 4201.5) seems to run through the series with the exception of Rigel. λ 4131.0 is strong in the spectrum of Rigel, but I do not find it in other stars. The remaining lines are represented in some doubtful cases. For this reason and on account of their faintness they are not considered separately.

λ 4365. This is represented by a diffuse line in the spectrum of ε Orionis, and probably by a line in the brightest trapezium star, although the measured wave-length (4369) of the latter is not in very good agreement. I do not find it in β, ζ and γ Orionis.

λ 4390. Professor Campbell has photographed a line at this place which does not appear on my plates. It corresponds with strong absorption lines in β, ε, ζ and γ Orionis; in γ it is particularly strong and sharp. There is also a strong absorption line at this place in Bond 628. The wave-length given above is somewhat too great, according to my measures of the stellar lines; in Rigel, Scheiner makes it 4388.5.

λ 4471.2, (4471.36). This corresponds with one of the characteristic Orion lines, and its exact coincidence with the dark star-line in the case of Bond 628 has already been mentioned. It is a strong line in all the Orion stars I have examined.

λ 466 ±, (4661, Campbell). The nebular line, on the only plate of mine on which it appears, is too faint and blurred for measurement. I do not find star-lines at this place, but there is a very strong line at λ 4650 in ε and ζ Orionis and Bond 628.

λ 4716. A prominent line at this place seems to run through all the Orion stars. It is represented in β, γ, δ, ε, ζ Orionis and Bond 628.

λ 4959.02, λ 5007.05. The two principal lines of the visual spectrum are not represented by absorption lines in any of the stars.

λ 5875.98 The D_3 line is dark in the spectrum of Rigel. I have not yet been able to ascertain whether it is present in other Orion stars.

The comparisons which have been made above demonstrate the intimacy of the relation which exists betwen the Orion nebula and the neighboring stars; indeed, taking into account the rela-

tive intensities of the lines, the spectrum of Rigel may almost be regarded as the nebular spectrum reversed. Even D_3 is represented by an absorption line, but an exception must be made of the two chief lines in the visual spectrum, which do not seem to be reversed under any circumstances. It is not improbable that other nebular lines will be found at places corresponding to strong lines in other Orion stars, as for instance at λ 4650.

Professor Lockyer has given a table of lines common to the Orion nebula and α Andromedæ, but it seems to me that such identifications, in the case of a star like α Andromedæ, whose spectrum is full of fine lines of nearly equal intensity, and with uncertainties of at least one tenth-metre in the positions of most of the nebular lines, are little better than guesswork. In the case of the Orion stars the conditions are very different. The spectra are nearly blank, and the lines corresponding with the nebular lines are in general isolated and conspicuous; an error of one tenth meter can hardly lead to an erroneous conclusion.

As to the chemical significance of these lines, it seems to me that at present little can be said, for with a few exceptions their wave-lengths have not yet been determined with the necessary accuracy. The exact wave-lengths can however be ascertained by means of the Orion stars. Although in these stars the known lines capable of serving as reference points are few in number, measurements of considerable precision can be made with their aid, as in the case of Rigel at the Potsdam Observatory, with the positions obtained in this way the lines can be certainly identified in the spectra of stars which also contain a large number of solar lines, and in these spectra the lines in question can be measured with the requisite precision.

Some of the observations which I have described have an important bearing on theories of stellar development. It has been shown that contrary to the belief which has been held up to the present time, the trapezium stars have spectra marked by strong absoption bands, they have not the direct connection with the nebula that would be indicated by a bright line spectrum, but are in fact on precisely the same footing (spectroscopically) as other stars in the constellation of Orion. While their relation to the nebula is more certain than ever, they can no longer be regarded as necessarily situated *in* the nebula, but within indefinite limits they may be placed anywhere in the line of sight. It should be noted that the broadness of the absorption lines precludes the supposition that the absorption is due merely to the nebula, the stars being situated on the opposite

side from the earth. In that case the dark lines would or would not appear, according to the preponderance of the stellar or nebular radiations as seen from the earth, but in any case they could not exceed the nebular lines in width; hence the absorption has its origin in the atmosphere of the star itself.

It might be supposed that the star lines are doubly reversed, as in Pleione, in which case the bright lines would not be seen, on account of the nebular lines superposed on them. A brightening of the nebular lines would however result, which is not found on the photographs; moreover no central brightening is seen in those dark lines which are not traversed by the lines of the nebula. The supposed condition is not in itself very probable.

With regard to the appearances that have led to the belief that the nebular lines are bright in the spectra of these stars, I believe that they are of physiological and photographic origin. To me, as well as to other observers, the nebular lines (including $H\gamma$) have always appeared brighter where they were crossed by the star spectra, but this appearance is certainly an illusion, for the star line at $H\gamma$ is dark, although so broad and diffuse that it would easily escape detection by visual means. Possibly it is this unnoticed darkening of the background that causes the nebular line in its center to look unusually bright, while the spreading of the photographic action from the star spectrum outward along the nebular lines, may be the cause of the similar appearance that has been observed on photographs.

Finally I may observe that the discovery of the true nature of the spectra of these stars removes a certain difficulty that existed in connecting the Orion nebula with the system of stellar evolution. According to Professor Pickering* the spectra of ninety-nine one-hundredths of the stars can be imitated by combining in different proportions four sets of lines, one set being the characteristic lines of the bright stars in Orion. Hence the Orion stars are in the direct line of promotion. But the spectrum formerly ascribed to the stars of the trapezium differed from that of bright-line stars of the usual type, and seemed to indicate that the first stage of condensation was not the same as in other gaseous nebulæ. This difficulty is therefore removed. The first step in the formation of the Orion stars may be a bright-line star of a type already known; it has however not been observed.

* Astronomy and Astro-Physics, October 1893, p. 719.

SPECTRA OF THE GREAT NEBULA IN ORION AND OTHER WELL-KNOWN NEBULÆ.*

W. W. CAMPBELL.

THE PLANETARY NEBULA, SDM. − 12°, 1172.

This nebula was discovered by Mrs. Fleming on the Harvard College Observatory plates by means of its spectrum of bright lines, and attention was called to the fact that its H_β hydrogen line is unusually bright.†

It is a beautiful object as seen in the 36-inch telescope, consisting of a 9th magnitude star surrounded by a circular disc of blue light nearly 15″ in diameter. In the spectroscope, with open slit, the well-known nebular lines at wave-lengths 5007, 4959 and 4861 are seen as circular discs, of which the last is considerably the largest in diameter. The diameters of the three discs were measured with the micrometer, on Nov. 2, 1893, and found to be about 11″, 9″ and 14″ respectively. The relative intensities of the light in the three discs were estimated at 10 : 3 : 7. A wedge photometer of increasing darkness was moved over the eye-piece at right angles to the line joining the three discs; and the fact that the disc at 4861 disappeared before the disc at 5007 did, proves that the latter is the brighter. All of the discs are brightest at the centers and fade away gradually as the edges are approached, as would be expected; but the hydrogen disc 4861 is of much more uniform brightness throughout than the other two.

The relative diameters and intensities of these discs are very important, in that they make it almost certain that the incandescent hydrogen which furnishes the disc 4861 forms the outer shell or layer of this nebula, and that the unknown gases which furnish the discs 5007 and 4959 must for the most part lie within the hydrogen exterior.

It should be pointed out that this nebula is very near the nebulous regions of Orion. The hydrogen lines are relatively very strong in the Orion nebula spectrum, also, and a possible common origin of the two objects is suggested.

The other hydrogen lines H_γ, H_δ and H_α are visible in this spectrum, but the last two are very difficult. No other lines were seen with certainty, and no photographs were secured. No bright lines were seen in the stellar spectrum.

* Communicated by the author. [Continued from page 398.]

† *Astronomische Nachrichten*, No. 3049.

THE PLANETARY NEBULA IN DRACO, G. C. 4373.

Great interest attaches to this object, both from its remarkable form,* and from the fact that this was the first nebula to be studied spectroscopically, by Dr. Huggins† in August, 1864.

I obtained visual observations of its spectrum in Sept. and Oct., 1893. In addition to the three well-known lines, I was able to observe six other bright lines at $H\alpha$, D_3, λ 5750, λ 4688, λ 4472, and $H\gamma$, of which all but the last one are difficult. The "traces of lines at 527, 518, 509 and 479" observed by Vogel‡ were not visible to me. $H\alpha$ is extremely faint. D_3 and λ 575 were observable only when the bright central star was just outside the wide slit, and were not visible at all when the star spectrum was in view. Those bright lines are due, therefore, to the nebula proper, as indeed are all the lines observed, and there is no evidence to show that they exist at all in the central star. When the spectrum is observed with an open slit, there are three well-defined, bright, monochromatic images of the nebula formed at λ 501, λ 496, λ 486. The continuous spectrum of the star crosses these images, but no brightenings are visible at their centres, as would be the case if the nebular lines were present in the star.

Further, when the slit is made extremely narrow, the middle of each narrow nebular line is only very slightly brighter when the star is in the slit than when it is just outside. When the slit is wide and the star is just outside the slit the continuous spectrum of the nebula is very easily seen.

The estimated relative intensities of the principal bright lines are 10 : 3 : 2.

Two photographs were obtained, in May and September, 1893. A list of the bright lines observed is as follows, the first three having been obtained visually.

TABLE IV.—BRIGHT LINES OBSERVED IN G. C. 4373.

	6560	$H\alpha$, extremely faint.
	5879	D_3, very faint.
	5750	Very faint.
5007	5007	1st nebular line, very bright.
4959	4959	2d nebular line, very bright.
4861	4861	$H\beta$, very bright.
	4716	Very faint.
	4688	Faint.
	464	Very faint.
4472	4472	Bright.

* Described by Professors Holden and Schaeberle in *Mon. Not. Roy. Ast. Soc.* vol. 48, pp. 388-91.

† *Philosophical Transactions*, 1864, p. 438.

‡ *Astronomische Nachrichten*, vol. 78, p. 246.

4363	4363	Faint.
4341	4341	Hγ, very bright.
4102	4102	Hδ, very bright.
4067	4067	Bright.
4026	4026	Bright.
3969		Hε, very bright.
3888		Hζ, bright.
3867		Bright.

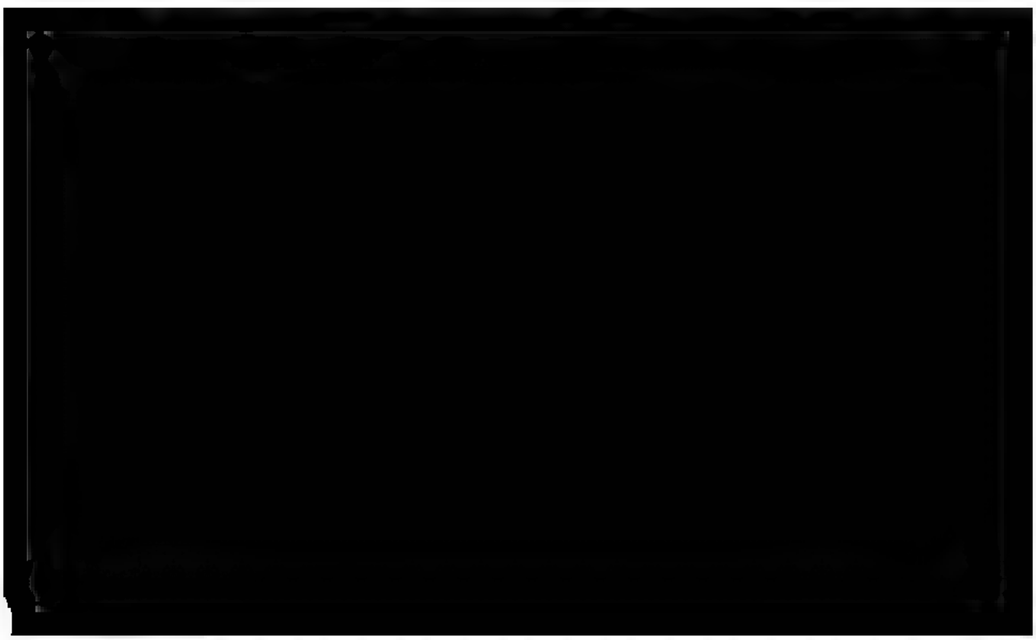
Eleven of these bright lines appear not to have been observed by others. The lines at λ 5007, λ 4959, λ 4861 are those first observed by Huggins in 1864. Von Gothard* photographed lines at 434, 410, 3965, 3865, 373, in October, 1892.

THE PLANETARY NEBULA, G. C. 4390, (Σ6).

This interesting nebula is nearly elliptical in outline, and near its centre the nebulosity is exceedingly condensed. With low powers this condensation appears considerably like a star, but high powers disperse it and leave only a very faint stellar nucleus.

Its spectrum has been investigated as thoroughly as the Lick apparatus would permit, though the chromatic aberration of the 36-inch lenses prevented me from carrying the work far into the ultra-violet. The visual observations were made mostly in September and October, 1893, and the photographic in September and October, 1892, and in June and July, 1893.

Visually, I was able to observe the lines Hα, D_3, 575, 541, 518, 501, 496, Hβ, 471, 469, 464, 447, 436, Hγ, Hδ; or fifteen in all. Lists of the lines shown on five negatives are given in the following columns, beginning with λ5007. All the lines preceding λ 5007 were measured visually. The relative intensities of the three principal lines are about as 5:2:1.



			4574		Faint.
4473	4473	4473	4473	4473	Very bright.
4390	4390	4390	4390		Faint.
4365	4364	4364	4364	4363	Bright.
4341	4341	4341	4341	4341	$H\gamma$, very bright.
				4265	Very faint.
				4145	" "
				4122	" "
	4102	4102		4102	$H\delta$, very bright.
				4067	Very faint.
				4026	" "
		3969		3969	$H\epsilon$, bright.
				3889	$H\zeta$, faint.
				3868	Bright.

Of these twenty-nine bright lines, eighteen at least appear to be new. The lines at λ 5007, λ 4959, λ 4861 were among the first nebular lines discovered by Dr. Huggins, in 1864. Professor Vogel observed the line λ 518 in 1871. Dr. Keeler, in 1890, detected other bright lines $H\alpha$, D_3, $H\gamma$, and three others placed by eye-estimates at about λ 5680, λ 5400 and λ 4450. It is probable that the last three are identical with the lines observed by me at 575, 541 and 447.

The line D_3 is sufficiently bright for its form to be observed. When it is in focus, it does not reduce to a point: it extends out a perceptible distance from the continuous spectrum of the nucleus; and, as is the case with the principal visual lines, belongs to the nebula proper. The continuous spectrum is quite strong; but how much of it is due to the nebula and how much to the stellar nucleus cannot be said, as they are superposed.

It is nearly certain that a very faint line exists in this nebula at λ 4924.

The Ring Nebula in Lyra, G. C. 4447.

In addition to the three well-known bright lines at λ 501, λ 496, λ 486 already observed in this nebula, there is a fourth bright line at λ 469. The fourth line is relatively as bright in this nebula as it is in N. G. C. 7027 and G. C. 4964, and these three spectra are probably essentially identical. The relative visual intensities of the four lines in the ring nebula were estimated at

$$10(+) : 3 : 1(-) : 1/8.$$

The third line $H\beta$ appears to be relatively fainter than in the other nebulæ.

The continuous spectrum is visible from λ 55 to λ 46. The spectrum of the central star could not be seen. No photographs were secured.

Von Gothard, in September 1892, photographed lines at 434, 411, 3965, 3865, 373, none of which had been previously observed.

The Planetary Nebula, N. G. C. 7027.

This object contains two nuclei of nebulous matter, in which there are apparently no stars. One of the nuclei is especially bright and condensed. In the spectrum of the principal nucleus I was able to observe, visually, the bright lines Hα, D_3, 575, 541, 531, 518, 501, 496, Hβ, 474, 472, 469, 463, Hγ, Hδ. The condensed parts of the nebula give a fairly bright continuous spectrum. The relative intensities of the four principal lines, 5007, 4959, 4861, 4688, were estimated at

$$10 : 3 : 1 : \tfrac{1}{2}.$$

I have not been able to complete the photographic investigation of this spectrum. The first column below contains the wave-lengths of the bright lines obtained by a short exposure in September, 1892. The results in the last three columns were obtained visually in 1893.

Table VI.—Bright Lines Observed in N. G. C. 7027.

			6562	Hα, extremely faint.
			5877	D_3, very faint.
	5752	5750	5753	Very faint.
	5412	5413	5412	Faint.
	533		5313	Very faint.
		5178	5186	" "
5007				1st nebular line, very bright.
4959				2d nebular line, very bright.

The Planetary Nebula, G. C. 4964.

This nebula consists of two nearly concentric rings more or less broken up, with a 14 ± magnitude stellar nucleus near its center. No part of the nebula is especially dense. The spectrum of the central star is too faint to be seen, but the nebula, with wide slit, gives a fairly strong continuous spectrum.

Visually, I am able to see in this spectrum the bright lines 541, 532, 501, 496, Hβ, 474, 469, Hγ. The relative intensities of the four principal lines, 5007, 4959, 4861, and 4687 were estimated at 10 : 3 : 1 : ½.

A list of the measured wave-lengths of the bright lines follows. Those in the first column were made visually; those in the last two columns, photographically in 1893.

Table VII.—Bright Lines Observed in G. C. 4964.

540			Very faint, difficult.
532			" " "
	5007	5007	1st nebular line, very bright.
	4959	4959	2d " " " "
	4861	4861	Hβ, very bright.
	4744	4744	Faint.
	4715	4713	"
4684	4687	4688	Very bright.
	466	4663	Very faint.
	4643	4647	Faint.
	4473	4472	Very faint.
	4364	4364	Bright.
	4341	4341	Hγ, very bright.
	4102	4102	Hδ, " "
		4067	Very faint.
		4026	" "
		3969	Hε, very bright.
		3868	Very bright.

Of these eighteen lines, Dr. Huggins in 1864 observed those at 5007, 4959, 4861, 4688; Keeler added Hγ in 1890; Gothard in 1892 photographed additional lines at 409, 397, 3865; and ten of the lines appear to be new.

The wave-lengths of the thirty-six bright lines in the seven nebulæ observed are collected in Table VIII. The visual spectra have been observed qualitatively as completely as possible. The photographic investigations have been made as complete as possible for the Orion Nebula, G. C. 4373, G. C. 4390 and G. C. 4964. It is evident, however, that a reflecting telescope or a photographic telescope would have been much more efficient in the violet end of the spectrum.

TABLE VIII.—BRIGHT LINES OBSERVED IN THE NEBULÆ.

Orion Nebula.	*BDM −12°1172	G. C. 4373	G. C. 4390	*G. C. 4447	†N.G.C. 7027	G. C. 4964
	Hα	6560	6563		6562	
5874		5879	5876		5877	
		5750	5752		5752	
			5413		5412	540
					5313	532
			5185		5182	
5007	5007	5007	5007	5007	5007	5007
4959	4959	4959	4959	4959	4959	4959
4861	4861	4861	4861	4861	4861	4861
			4743		4743	4744
4714		4716	4714		4716	4714
		4688	4687	469	4688	4686
4662			4663			4663
		464	4643		463	4645
			461			
			459			
			4574			
4472		4472	4473			4472
4389			4390			
4364		4363	4364		4363	4364
4341	Hγ	4341	4341		4341	4341
4265			4265			
423						
4143			4145			
4121			4122			
4102	Hδ	4102	4102		4102	4102
4067		4067	4067			4067
4026		4026	4026			4026
3969		3969	3969			3969
3889		3888	3889			
3869		3867	3868			3868
3835						
3798						
3770						
3749						

nent in the present spectrum of Nova Aurigæ, and possibly of Nova Normæ. The faint line near λ 5313 is possibly the coronal" line at λ 5317.

There are a few points of correspondence between the nebular spectrum and that of the Wolf-Rayet stars, notably at D_3, λ 5412, λ 4687, λ 4472, λ 4389, and the hydrogen lines; but there are so many points where correspondence is lacking, that we cannot say the two types are closely related. However, this subject will be discussed in detail in another paper soon to be published.

The close connection of the nebular spectrum and that of the Orion stars was brought out in Table III. That table, constructed especially for the Orion Nebula, will serve equally well for the other nebulæ, and need not be reproduced here.

No evidence has been obtained to show that any of the stellar nuclei of the nebulæ contain bright lines, and no dark lines have been observable in them. The faint bright lines probably belong entirely to the nebulæ proper. It is possible, however, that some of the faint lines obtained photographically are due to the light of the stellar nuclei; but a comparison of the lines obtained in nebulæ *with* and *without* stellar nuclei shows that such an assumption is unnecessary. Nevertheless, the connection of the planetary nebulæ and their stellar nuclei is undoubtedly a close one. The relation of their spectra should be investigated as far as possible. Certainly, at no other point in astro-physical research is accurate knowledge more desirable. The Lick Observatory does not as yet possess suitable apparatus for the work.

I wish to acknowledge having received valuable assistance in making the above observations from Mr. C. D. Perrine, Secretary of the Observatory; from Mr. S. D. Townley, now Instructor in Astronomy in the University of Michigan; and especially from Assistant Astronomer A. L. Colton: without which many of the results would be much less complete.*

LICK OBSERVATORY, 1894, March 20.

STARS HAVING PECULIAR SPECTRA.†

M. FLEMING.

An examination of photographs of stellar spectra, taken at the Peruvian station of Harvard College Observatory under the

* CORRECTION: My statement in regard to the dark D_3 in β and ε Orionis, on page 395, line + 7, of the May A AND A.-P. requires modification. It is true of stars containing only dark lines, so far as I know. The dark component of the D_3 line in β Lyræ is of course well known.—Mt. Hamilton, 1894, May 7.

† Communicated by Edward C. Pickering Director of Harvard College Observatory.

direction of Professor S. I. Bailey, and forming part of the work of the Henry Draper Memorial, has added several faint objects to the list of stars having peculiar spectra. The designation of the star is followed by its approximate right ascension and declination for 1900, its catalogue magnitude and a brief description of its photographic spectrum.

Designation.	R. A. 1900 h m	Decl. 1900 ° ′	Magn.	Description.
BD. + 30° 591	3 49.1	+ 30 46	6.5	Hβ bright
A.G.C. 13539	9 51.3	− 41 7	7½	Type IV
A.G.C. 19254	14 7.4	− 53 28	7½	Type IV
......	16 24.7	− 40 2	...	Gaseous Nebula.
S.D. − 21° 4	16 55.6	− 21 40	9 5	Gaseous Nebula.
Z.C. 17^h 2657	17 40.8	− 35 40	8	Type IV
A.G.C. 24406	17 52.7	− 36 0	7.2	Hβ bright.
S.D. − 15° 4923	18 13.6	− 15 39	9.0	Type IV
S.D. − 13° 5083	18 38.7	− 13 20	9.1	Type V
BD. + 11° 4673	21 46.2	+ 12 9	7.7	Hβ bright.

BD. + 30° 591. The spectrum of this star also shows a bright line superposed on the edge of shorter wave-length of the line Hγ which is dark.

S.D. — 21° 4483. An observation of this object on May 10, 1894, with the 15-inch equatorial, by Mr. O. C. Wendell, confirms the photographic results. Its visual spectrum is the same as that of other gaseous nebulæ and it presents a hazy disk when seen with a high power.

A.G.C. 24406. The spectrum of this star shows a bright line superposed on the edge of greater wave-length of each of the lines Hγ and Hδ, which are dark.

S.D. — 13° 5083. This star like all others of the same class lies near the central line of the Milky Way, its Galactic longitude being 348° 1′ and its Galactic latitude − 3° 53′.

The first of these stars is A. G. C. 13624, Magn. 8.

It must be here understood that the stars in the above table are not here announced as "suspected." variables since each of them appears bright on several plates and faint on several other plates. The variation has also been confirmed in each case by Professor E. C. Pickering. This is also the case with all the variable stars discovered here from the presence of bright hydrogen lines in their photographic spectra, with the exception of the star whose position for 1900 is in R. A. 15^h 27.0^m, Decl. $-71°$ 32′. (ASTRONOMY AND ASTRO-PHYSICS Vol. XII, p. 546).

Photographs of the spectra of the variable stars S Orionis, U Puppis, V Leonis, S Boötis, and R Phoenicis have been examined and show bright hydrogen lines. S Orionis and S Boötis were obtained from an examination of photographs taken with the 24-inch Bruce photographic telescope.

HARVARD COLLEGE OBSERVATORY,
Cambridge Mass., May 11, 1894.

SOLAR PHENOMENA OBSERVED AT THE ROYAL ROMAN COLLEGE DURING THE THIRD AND FOURTH QUARTERS OF THE YEAR 1893.*

P. TACCHINI.

I give below a résumé of the results which I have obtained for the distribution in latitude of solar phenomena, from observations made at the Royal Roman College during the latter half of the year 1893. The observations refer to zones 10° wide in each hemisphere of the Sun.

1893.	Perturbations.		Faculæ.		Spots.	
Latitude.	3rd Quarter.	4th Quarter.	3rd. Quarter.	4th Quarter.	3rd Quarter.	4th Quarter.
90° + 80°	0.000	0.000				
80 + 70	0.011	0.000				
70 + 60	0.011	0.000				
60 + 50	0.011	0.011				
50 + 40	0.042 } 0.420	0.016 } 0.394	0.000	0.000	0.006	
40 + 30	0.111	0.069	0.017	0.020	0.006	0.000
30 + 20	0.105	0.149	0.068 } 0.442	0.106 } 0.463	0.061 } 0.400	0.070 } 0.421
20 + 10	0.069	0.091	0.184	0.192	0.200	0.219
10 + 0	0.066	0.058	0.173	0.145	0.127	0.132
0 — 10	0.057	0.061	0.109	0.184	0.127	0.228
10 — 20	0.076	0.066	0.177	0.208	0.279	0.237
20 — 30	0.107	0.121	0.184 } 0.558	0.106 } 0.537	0.176 } 0.600	0.105 } 0.579
30 — 40	0.061	0.088	0.085	0.031	0.018	0.009
40 — 50	0.021 } 0.574	0.028 } 0.606	0.003	0.008	0.000	
50 — 60	0.110	0.063				
60 — 70	0.115	0.168				
70 — 80	0.067	0.011				
80 — 90	0.000	0.000				

* Communicated by the author.

As in the preceding half year, all the phenomena have been more frequent in the southern zones, but it should be noted that the frequency of the faculæ and spots was almost the same in the two hemispheres during the months of November and December. The maximum frequency of the faculæ and the spots continued to be in the zones (± 10° ± 20°) while that of the protuberances was found in higher latitudes. The persistence of the maximum of the protuberances in latitude (−50° − 70°), which had already been observed in the second quarter of the year, and which was preceded by a well-marked minimum in the zone (− 40° − 50°), is very irregular. The greater activity in the southern hemisphere is further confirmed by the fact that the highest and most beautiful protuberances have almost always been observed south of the solar equator, thus demonstrating that outside of the solar rotation there are causes still unknown which produce a marked variation of the solar activity with the latitude and with the hemisphere.

During the third quarter we observed no metallic eruptions, and during the fourth quarter we found indications of eruption on December 25 and 26 only, in latitude + 21°.7 and + 22°.6 on the east limb. The photographs which we have obtained also show a greater activity south of the solar equator.

ASTRO-PHYSICAL NOTES.

All articles and correspondence relating to spectroscopy and other subjects, properly included in ASTRO-PHYSICS, should be addressed to George E. Hale, Kenwood Observatory of the University of Chicago, Chicago, U. S. A. Authors of papers are requested to refer to last page for information in regard to illustrations, reprint copies, etc.

The Spectrum of Comet *b*, 1894 (Gale).—The spectrum of Gale's comet was observed at Allegheny on April 9, and appeared to be of the ordinary carbon type; no photographs of the spectrum were attempted. At Mt. Hamilton Professor Campbell obtained some fine photographs, showing more than twenty lines. The spectrum was identical with that of Comet *b*, 1893.

Red Fringe to the Orion Nebula.—Professor Barnard describes in *Knowledge*, (May 1) a red fringe of light which he sees around the sharply defined southern edge of the great bright area in the Nebula of Orion. He is not prepared to say what the phenomenon is, but hardly thinks that it is a telescopic effect.

As the Nebula of Orion emits no red light, there is no doubt that the appearance is subjective, arising from the effect on the retina of the bluish-green light of the bright region to which reference is made. The production of these colored subjective images is a common experiment in physiological optics.

The Influence of Slit-Width on the Appearance of Comet-Spectra.—Professor Vogel contributes on article to *A. N.* 3222, in which he shows that in 1881 he had already called special attention to the influence of slit-with on the distribution of light in the bands of comet-spectra. In a monograph on the great comet of 1881, published by the Potsdam Observatory, Professor Vogel makes the following remarks:

"I do not wish to bring these considerations to an end without calling attention to the fact that the width of the slit of the spectroscope is capable of producing a very great effect on the *distribution* of light in bands which are diffuse on one side, while lines, or bands diffuse on both sides, are almost entirely unaffected by it. How great the influence of the slit width may be, is shown by the following observations; and I should recommend all observers not to overlook this circumstance but in all measurments and records of the relative intensities to assure them selves that the slit is made as narrow as possible. There is no doubt that the greatly discordant observations of faint comets have their origin in the very wide slit which was necessary in order that the faint bands of their spectra could be seen at all."

The observations referred to by Professor Vogel in the beginning of this paragraph were made with different slit-widths, the source of light being a Bunsen-flame placed at different distances from the instrument. They show the progressive shifting of the maximum brightness toward the violet as the slit-width is increased.

Professor Vogel still thinks that vary'ng slit-width is not a sufficient expalanation of the observed deviation of certain comet-spectra from the ordinary carbon type, and regards the coexistence of the CO spectrum in these cases as altogether probable.

Notwithstanding this previous discussion of the subject, the paper by Professor Kayser, translated in our last number, is of very great value, on account of its admirable clearness and thoroughness. Whether it explains all observed anomalies in comet-spectra, or whether, as Professor Vogel maintains, some of these anomalies have their origin in real differences of spectra, future observations with the aid of photography will probably decide. It can hardly be supposed that exceptional cases will be rarer in the future than they have been in the past.

Professor Kayser at Bonn.—Professor Heinrich Kayser of Hanover has been appointed Professor of Physics in the University of Bonn. The appointment to a chair which has been occupied by such intellectual giants as Clausius and Hertz is an honor which requires no comment, but it is one which Professor Kayser has well earned.

The Line Spectrum of Oxygen.—In *Wied. Ann.*, 4, 1894, there is an interesting article by Max Eisig, which gives the results of his measurements of the line spectrum of oxygen. The work was carried on in the laboratory of Kayser and Runge at Hanover; and their concave grating apparatus was used for the purpose. The oxygen was electolytically prepared from purified water, and was admitted to the spectrum tube after being dried over phosphoric acid. The discharge tube itself was an 'end-on" one, which had one end closed by a quartz plate fastened by water-glass. The connections of the various parts to the mercury pump were made by sealing-wax joints, and grease was used with the stop-cock; but in spite of these facts no impurity-lines appeared in the spectra, probably owing to the

fact that repeated refilling with oxygen removes the ordinary carbon impurities. The vacuum used was not very great; although various pressures and e. m. f's. were used; and so, although traces of water-vapor were always present, they produced little or no effect on the photographic plates. The grating was a concave one, 21 feet radius and having 20,000 lines to the inch; and photographs were taken throughout the entire spectrum as far as the plates would allow. Exposures of from 2½ to 4 hours were found necessary. On the same plates and directly over the oxygen lines, was photographed the arc-spectrum of iron, care being taken to avoid displacement; and then comparisons of the two spectra were made under a dividing engine. The measurements of the oxygen lines are thus reduced to the same scale as Kayser and Runge's wave-lengths, *i. e.*, one where the standards are $D_1 = 5896.16$ and $D_2 = 5890.19$ (Rowland's standards are $D_1 = 5896.156$ and $D_2 = 5890.182$) Eisig estimates his probable error at from 0.1 to 0.2 Angstrom unit. Of course in a spectrum so imperfectly studied in the past as that of oxygen, it is impossible to be sure that all the lines observed belong to the substance itself; but the cases of doubtful lines are rare. There is no grouping of the lines in series, as there is in the case of hydrogen and other substances; and careful examination shows that none of the lines measured occur in the solar spectrum. This is no evidence, of course, that oxygen is not present in the Sun. It may be there, and its spectrum may be too weak for observation; or the conditions under which it exists may be such that a different spectrum from the so-called "line-spectrum" is produced.

The lines measured by Eisig and attributed to oxygen have the following wave-lengths and intensities:

2433.6	5	3410.2	[illegible]	3912.3	3	4096.9	6	4144.0	6	4332.2	6	4448.7	[illegible]	4662.0	4
2445.6	4	3712.8	4	3919.6	[illegible]	4097.8	6	4146.3	5	4337.3	4	4452.8	5	4674.2	6
2478.8	3	3727.5	3	3945.3	4	4103.4	6	4153.7	3	4345.9	2	4465.8	6	4676.6	5
2512.3	5	3749.6	2	3954.5	3	4105.3	4	4156.8	6	4347.8	3	4466.7	[illegible]	4696.8	4
3134.9	5	3754.7	6	3973.4	1	4111.2	6	4169.5	6	4349.8	1	4468.4	6	4699.6	6
3138.6	6	3757.3	6	3983.0	4	4112.4	[illegible]	4185.8	4	4351.7	2	4469.9	6	4701.5	6
3271.2	6	3760.0	6	4070.1	1	4114.2	6	4190.0	4	4367.3	3	4591.4	3	4703.4	6
3273.9	6	3851.2	6	4072.5	1	4119.5	2	4317.4	3	4369.7	6	4596.5	4	4705.7	4
3287.8	6	3857.4	6	4076.2	1	4120.5	4	4319.9	3	4396.4	5	4639.2	4	4710.4	5
3377.1	4	3864.8	5	4079.1	5	4121.7	6	4326.2	6	4415.3	1	4642.1	2		
3390.2	3	3882.5	3	4085.5	4	4133.2	5	4327.8	6	4417.4	2	4649.5	1		
3407.7	5	3907.6	6	4093.2	[illegible]	4142.4	6	4329.0	6	4443.6	6	4651.2	4		

J. S. A.

CURRENT CELESTIAL PHENOMENA.

PLANET NOTES FOR JULY AND AUGUST.

H. C. WILSON.

Mercury, having been visible in the evening during the last days in June, will in July pass between us and the Sun, being hidden by the rays of the latter during the greater part of the month. He will be at inferior conjunction July 20 at 4^h 28^m P. M. central time.

Venus is slowly receding from us and moving around behind the Sun. Her disc will be 0.76 illuminated July 1 and 0.92 August 30. Venus will pass by Jupiter on the morning of July 20, the nearest approach of the two planets to each other occurring at about 2^h 30^m A. M. On the morning of July 28 at 6^h 13^m Venus will pass very close to the third magnitude star μ Geminorum, the difference of declination of the two bodies at the time of conjunction being only 3′. August 8 at 7^h 45^m A. M. Venus will pass 9′ to the south of another third magnitude star, δ Geminorum. Venus will be in conjunction with the Moon July 30 at 1^h 34^m A. M. and August 28 at 7^h 23^m P. M.

Mars will come into good position for observations after midnight by the first of August, and it is to be hoped that observers will begin early to study the markings on the surface of the planet. It is not necessary to have a great telescope, in order to see them to good advantage In fact there are some good observers who believe that planetary details can be seen better with small than with large telescopes. We do not subscribe to this belief, but do say that the difference in favor of the large telescope is not so great as to entirely discourage the possessor of a good small one from attempting to add to our knowledge of the planetary markings.

Jupiter and *Neptune* are coming around as morning planets but will not be in good position for observation during the summer. As already noted, Jupiter will be in conjunction with Venus, 51′ north of the latter, on the morning of July 20. Neptune will be still closer to Venus, only 9′ north, July 11, 11^h 54^m P. M.

Saturn will be visible in the early evening but will be pretty low in the west by the time twilight is over. Saturn and the Moon will be in conjunction July 9 at 9^h 11^m P. M. and August 6, 7^h 30^m A. M.

Uranus is making the turn of the loop in his apparent course among the stars and will be almost stationary during July. In August he will move eastward toward the star α Libræ. Uranus will be in conjunction with the Moon July 11 and August 7.

Planet Tables for July and August.

[The times given are local time for Northfield. To obtain Standard Times for Places in approximately the same latitude, add the difference between Standard and Local Time if west of the Standard Meridian or subtract if east].

MERCURY.

Date. 1894.		R. A. h m	Decl. ° ′	Rises. h m	Transits h m	Sets. h m.
July	5......	8 23.4	+16 59	6 16 A. M.	1 28 2 P. M.	8 41 P. M.
	15......	8 11.0	+15 25	5 30 "	12 36.5 "	7 43 "
	25......	7 44 8	+16 30	4 18 "	11 31.2 A. M.	6 42 "
Aug.	5......	7 46.0	+18 53	3 27 "	10 49.2 "	6 11 "
	15......	8 33.0	+19 04	3 33 "	10 56.7 "	6 02 "
	25......	9 47.4	+13 04	4 27 "	11 31.5 "	6 36 "

Date.		R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m
				VENUS.		
July	5......	4 21.5	+19 39	2 01 A. M.	9 27.0 A. M.	4 53 A. M.
	15......	5 11.0	+21 30	2 02 "	9 37.0 "	5 12 "
	25......	6 01.9	+22 26	2 09 "	9 48.4 "	5 28 "
Aug.	5......	6 58.8	+22 17	2 23 "	10 02.0 "	5 41 "
	15......	7 50.4	+21 02	2 41 "	10 14.1 "	5 47 "
	25......	8 41.2	+18 47	3 04 "	10 25.5 "	5 47 "
				MARS.		
July	5......	0 35.0	+ 0 27	11 32 P. M.	5 37.2 A. M.	11 42 A. M.
	15......	0 56.8	+ 2 34	11 06 "	5 19.5 "	11 33 "
	25......	1 17.1	+ 4 28	10 40 "	5 00.6 "	11 21 "
Aug.	5......	1 37.2	+ 6 18	10 09 "	4 37.4 "	11 06 "
	15......	1 52.8	+ 7 40	9 40 "	4 13.6 "	10 47 "
	25......	2 05.1	+ 8 44	9 08 "	3 46.6 "	10 25 "
				JUPITER.		
July	5......	5 20.9	+22 43	2 45 A. M.	10 26.2 A. M.	6 07 P. M.
	15......	5 30.4	+22 52	2 15 "	9 56.4 "	5 38 "
	25......	5 39.5	+22 58	1 44 "	9 26.2 "	5 08 "
Aug.	5......	5 49.1	+23 02	1 10 "	8 52.4 "	4 35 "
	15......	5 57.0	+23 04	12 38 "	8 21.1 "	4 04 "
	25......	6 04.4	+23 04	12 06 "	7 49.2 "	3 32 "
				SATURN.		
July	5......	13 12.5	− 4 57	12 33 P. M.	6 16.5 P. M.	12 00 midn
	15......	13 13.6	− 5 07	11 55 A. M.	5 38 3 "	11 22 P. M.
	25......	13 15.3	− 5 20	11 18 "	5 00.8 "	10 43 "
Aug.	5......	13 17.8	− 5 37	10 39 "	4 20.0 "	10 01 "
	15......	13 20.5	− 5 57	10 04 "	3 43.4 "	9 23 "
	25......	13 23.7	− 6 18	9 29 "	3 07.3 "	8 46 "
				URANUS.		
July	5......	14 36.2	−14 52	2 37 P. M.	7 40.0 P. M.	12 43 A. M.
	15......	14 35.8	−14 51	1 57 "	7 00.4 "	12 04 "
	25......	14 35.8	−14 51	1 18 "	6 21.1 "	11 24 P. M.
Aug.	5......	14 36.2	−14 54	12 35 "	5 38.4 "	10 41 "
	15......	14 37.0	−14 58	11 57 A. M.	4 59.7 "	10 02 "
	25......	14 38.0	−15 03	11 19 "	4 21.5 "	9 24 "
				NEPTUNE.		
July	5......	4 53.0	+21 06	2 26 A. M.	9 58.5 A. M.	5 31 P. M.
	15......	4 54.4	+21 08	1 47 "	9 20.6 "	4 54 "
	25......	4 55.6	+21 10	1 09 "	8 42.5 "	4 16 "
Aug.	5......	4 56.8	+21 11	12 27 "	8 00.5 "	3 34 "
	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

THE MOON.

Date.		R. A.	Decl.	Rises.	Transits.	Sets.
		h m	° ′	h m	h m	h m
July	23	0 51.4	+ 6 36	10 13 P. M.	4 42.8 A. M.	11 27 A. M.
	25	2 31.6	+18 15	10 53 "	6 14.8 "	1 53 P. M.
	27	4 31.7	+26 40	11 55 "	8 06.9 "	4 30 "
	30	6 51.2	+27 59	1 54 A. M.	10 18.0 "	6 49 "
Aug.	1	9 06.7	+20 36	4 33 "	12 25.7 P. M.	8 01 "
	3	11 03.8	+ 7 47	7 22 "	2 14.2 "	8 49 "
	5	12 46.8	− 6 04	9 56 "	3 49.0 "	9 29 "
	7	14 27.1	−17 54	12 22 P. M.	5 21.2 "	10 11 "
	9	16 12.6	−25 52	2 42 "	6 58.6 "	11 10 "
	11	18 03.8	−28 36	4 43 "	8 41.7 "	12 40 A. M.
	13	19 53.5	−25 42	6 11 "	10 23.1 "	2 41 "
	15	21 34.3	−18 04	7 07 "	11 55.9 "	4 54 "
	17	23 06.8	− 7 09	7 46 "	1 20.2 A. M.	7 07 "
	19	0 36.9	+ 4 52	8 20 "	2 42.2 "	9 19 "
	21	2 13.8	+16 40	8 57 "	4 11.0 "	11 41 "
	23	4 07.2	+25 40	9 52 "	5 56.3 "	2 13 P. M.
	25	6 19.6	+28 36	11 29 "	8 00.3 "	4 29 "
	28	8 34.9	+23 11	2 02 A. M.	10 07.4 "	5 57 "
	30	10 35.3	+11 18	4 52 "	12 00.0 M.	6 50 "

Phases and Aspects of the Moon.

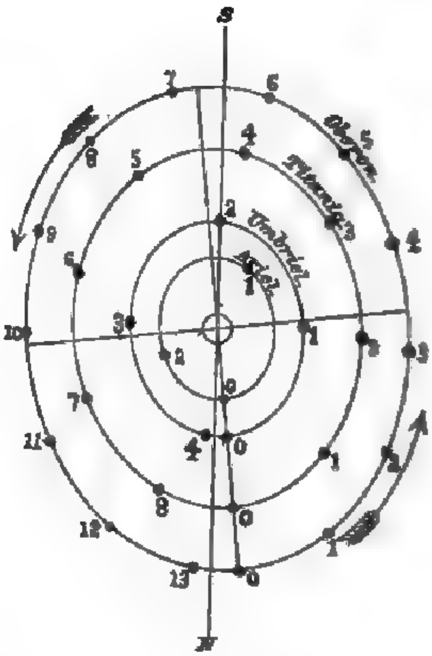
	Central Time.		
		d	h m
New Moon	July	2	11 45 P. M.
Perigee	"	3	7 40 A. M.
First Quarter	"	9	4 15 P. M.
Apogee	"	17	8 30 A. M.
Full Moon	"	17	4 03 P. M.
Last Quarter	"	25	3 07 P. M.
Perigee	"	31	5 06 P. M.
New Moon	Aug.	1	6 24 A. M.
First Quarter	"	8	4 05 A. M.
Apogee	"	13	1 30 P. M.
Full Moon	"	16	7 17 A. M.
Last Quarter	"	23	11 40 P. M.
Perigee	"	29	12 36 A. M.
New Moon	"	30	2 04 P. M.

Occultations Visible at Washington.

Date 1894		Star's Name.	Magnitude.	IMMERSION Washington M. T.		Angle f'm N p't.	EMERSION Washington M. T.		Angle f't N p't.	Duration.	
				h	m	°	h	m	°	h	m
July	9	58 Virginis	7	10	29	100	11	30	312	1	01
	16	B. A. C. 6628	6	12	58	111	14	00	209	1	02
	18	χ Capricorni	5½	14	58	67	16	14	227	1	16
	20	B. A. C. 7835	6	7	[illegible]	50	8	24	270	0	55
	23	B. A. C. 221	6	11	34	15	12	20	279	0	46
	26	ζ Arietis	5	10	14	67	11	00	252	0	46
	26	B. A. C. 1055	7	14	07	59	15	08	245	1	01
	27	χ Tauri	6	13	33	36	14	16	284	0	43
Aug.	13	ω Sagittarii	5	8	42	30	9	47	300	1	[illegible]
	13	A Sagittarii	5	10	44	61	12	12	252	1	28
	16	50 Aquarii	6	11	25	60	12	51	223	1	26
	18	20 Piscium	6	8	14	29	9	07	272	0	53
	26	47 Geminorum	6	12	04	97	12	49	263	0	45

Elongations of the Satellites of Uranus.

[The diagram shows the apparent paths of the satellites of Uranus during the summer of 1894. The black dots with the numerals indicate the positions of the satellites at intervals of 1 day after each northern elongation. The points marked 0 are those of northern elongation.]



ARIEL.

		h	
July	2	10.7 P. M.	N
	5	11.2 A. M.	N
	7	11.7 P. M.	N
	10	12.2 P. M.	N
	13	12.7 A. M.	N
	15	1.2 P. M.	N
	18	1.6 A. M.	N
	20	2.1 P. M.	N
	23	2.6 A. M.	N
	25	3.1 P. M.	N
	28	3.6 A. M.	N
	30	4.1 P. M.	N
Aug.	2	4.6 A. M.	N

UMBRIEL.

		h	
July	5	1.7 A. M.	N
	9	4.3 "	N
	13	7.8 "	N
	17	11.3 "	N
	21	2.8 P. M.	N
	25	6.3 "	N
	29	9 8 "	N
Aug.	3	1.2 A. M.	N

TITANIA.

		h	
July	2	6.7 P. M.	S
	7	3.2 A. M.	N
	11	11.7 "	S
	15	8.2 P. M.	N
	20	4.6 A. M.	S
	24	1.1 P. M.	N
	28	9 6 "	S
Aug.	2	6.1 A. M.	N

OBERON.

		h	
July	7	12.4 A. M.	N
	13	5.9 P. M.	S
	20	11.5 P. M.	N

OBERON, CONT.

		h	
July	27	5.0 A. M.	S
Aug.	3	10.6 P. M.	N

δ LIBRÆ.

Alternate Minima.

		h
July	2	2 P. M.
	7	5 A. M.
	11	9 P. M.
	16	1 "
	21	4 A. M.
	25	8 P. M.
	30	12 M.
Aug.	4	4 A. M.
	8	7 P. M.
	13	11 A. M.
	18	3 "
	22	6 P. M.
	27	10 A. M.

U CORONÆ.

Alternate Minima.

July	2	9 P. M.
	9	7 "
	16	4 "
	23	2 "
	30	12 M.

U CORONÆ Cont.

(Alternate Minima.)

		h
Aug.	6	9 A. M.
	13	7 "
	20	5 "
	27	3 "

U OPHIUCHI.

(Every fourth Minimum)

July	1	4 P. M.
	5	1 A. M.
	8	9 P. M.
	11	6 "
	15	2 A. M.
	18	11 "
	21	7 P. M.
	25	4 A. M.
	28	12 M.
	31	9 P. M.
Aug.	4	6 A. M.
	7	2 P. M.
	10	11 "

U OPHIUCHI Cont.

(Every fourth minimum)

		h
Aug.	14	7 A. M.
	17	4 P. M.
	20	12 midn.
	24	9 A. M.
	27	5 P. M.
	31	2 A. M.

Y CYGNI.

(Every fourth minimum.)

July	6	11 A. M.
	12	10 "
	18	10 "
	24	10 "
	30	10 "
Aug.	5	10 "
	11	10 "
	17	9 "
	23	9 "
	29	9 "

Maxima and Minima of Variable Stars.

[From ephemerides by Dr. Loewy in the "Companion to the Observatory," and by Dr. Hartwig in the "Vierteljahrsschrift der Astronomische Gesellschaft".]

Maxima

July	1	X Capricorni
	2	R Corvi
	2	R Boötis
	2	R Persei
	2	R Geminorum
	3	U Cassiopeiæ
	3	S Scorpii
	4	R Aquarii
	4	U Boötis
	6	S Sagittarii
	10	R Comæ
	10	R Sculptoris
	18	R Sagittarii
	24	R^2 Libræ
	26	S Vulpeculæ
	27	R Leonis Minoris
	27	Y Virginis
	28	Z Scorpii
	29	R Delphini
	30	R Scorpii
	30	V Ceti
Aug.	1	U Monocerotis
	2	W Cygni
	3	S Herculis
	4	R Lyræ
	5	Z Virginis
	5	T Hydræ

Maxima

Aug.	7	R Vulpeculæ
	7	R Ceti
	7	Y Libræ
	8	R Ursæ Majoris
	8	R Pegasi
	9	S Ursæ Majoris
	10	R Carini
	11	U Geminorum
	11	R Virginis
	13	R Arietis
	13	R Lacertæ
	13	U. Libræ
	14	T Libræ
	14	S Ophiuchi
	15	R Scuti
	15	S Arietis
	16	S Cephei
	16	R Sagittæ
	17	T Ursæ Majoris
	17	R Pegasi
	18	U Ceti
	19	V Tauri
	25	T Cephei
	28	S Cygni
	29	W Scorpii
	30	L^2 Puppis
	30	U Aquarii
	30	V Ceti

Minima

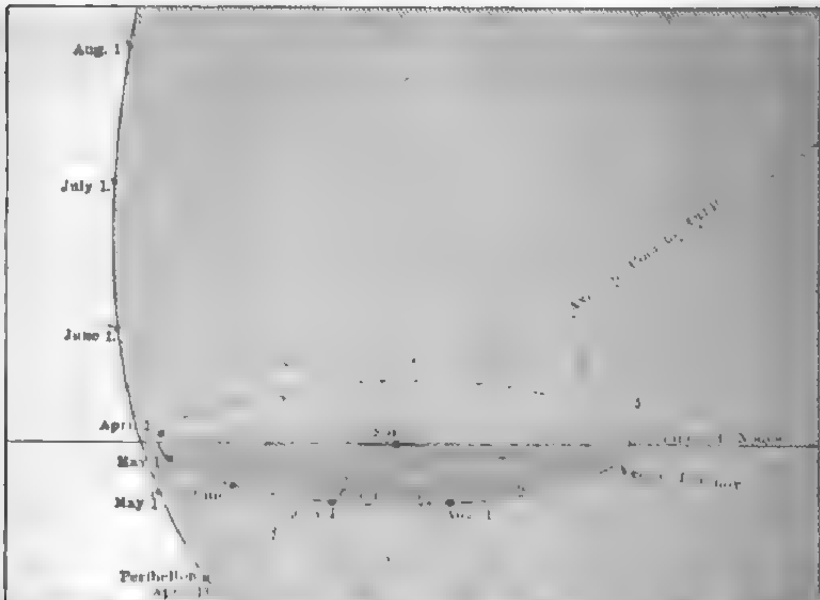
July	1	T Capricorni
	1	L^2 Puppis
	5	S Carini
	10	R Centauri
	11	R Scuti
	14	R Arietis
	14	U Monocerotis
	19	R Cancri
	20	R Lyræ
	25	S Ceti
	26	W Capricorni
	30	U Virginis
	30	V Cygni
	31	W Tauri
	31	R Sagittæ
Aug.	2	S Aquilæ
	5	X Boötis
	5	S Canis Minoris
	7	S Delphini
	16	T Herculis
	22	R Leporis
	29	U Monocerotis

COMET NOTES.

Denning's Comet a 1894.—Our last observation of this comet was obtained on the night of May 3. It was then exceedingly faint, and will probably not be visible, even in the largest telescopes, in June.

Gale's Comet *b* **1894.**—This comet which we reported last month as too far south for observation in our latitude has come rapidly north and is now almost out of view from the place of discovery. The orbit is almost perpendicular to that of the Earth, so that it is probably not a periodic comet, and the parabolic elements last received appear to represent the observations fairly well. Mr. Ellery, Government Astronomer, at Melbourne, Australia, has telegraphed the following elements:

$$
\begin{aligned}
T &= 1894 \text{ April } 13.75 \text{ Greenwich M. T.} \\
\left.\begin{aligned}
\omega &= 324^\circ\ 19' \\
\Omega &= 206\quad 15 \\
i &= \ \ 87\quad 15
\end{aligned}\right\} & \ 1894.0 \\
q &= 0.9849
\end{aligned}
$$



it was rapidly approaching the Earth, that it passed the point nearest to the latter about May 1, and that now it is rapidly receding.

The comet was first seen in northern latitudes by Mr. Douglass at Lowell Observatory, Flagstaff, Arizona, April 26. At Northfield cloudy weather prevented observations until May 3, when the comet was visible to the naked eye as a hazy star of the fifth magnitude. No tail was visible to the naked eye or with an opera glass, but with the 16-inch telescope the tail could be traced 20′ or 30′ from the nucleus. A photograph taken with our 6-inch Brashear camera on May 5 with an exposure of one hour shows very faint traces of a tail extending to the edge of the plate, a little over 6°. Unfortunately the 2¼-inch camera happened to be provided with a poor plate and showed the merest trace of a tail in the same time.

Mr. Barnard at Lick Observatory was more fortunate and succeeded in getting a very beautiful picture of the comet with an exposure of two hours and twenty minutes on the night of May 3. He obtained a second successful photograph, on May 5.

This comet was observed by Mr. Douglass at the Lowell Observatory, Arizona, on April 26th, 15^h G. M. T., in R. A. 6^h 30^m, and Decl. 33° 32′ south. He described it as of 5th magnitude brightness; circular, with a central nucleus, and a diameter approximately four minutes of arc, and a narrow tail eight minutes long. On April 28th he found it of the fourth magnitude, and from these and subsequent observations its positions showed it to be gaining somewhat on its ephemeris.

Ephemeris of Gale's Comet, *b* 1894.

Greenwich Midn.		R. A. h	m	Decl. °	′	log *r*.	log Δ	Br.
June	1	11	02.3	+38	39	0.1086	9.9869	0.48
	3		07.9	39	21			
	5		13.3	39	57			
	7		18.4	40	28			
	9		23.3	40	56	0.1366	0.0694	0.29
	11		28.0	41	20			
	13		32.5	41	41			
	15		36.9	41	59			
	17		41.1	42	14	0.1642	0.1369	0.19
	19		45.3	42	27			
	21		49.4	42	39			
	23		53.4	42	49			
	25	11	57.3	42	57	0.1912	0.1935	0.13
	27	12	01.1	43	04			
	29		04.8		10			
July	1		08.5		15			
	3		12.1		19	0.2172	0.2415	0.09
	5		15.8		22			
	7		19.4		24			
	9		23.1		25			
	11		26.7		25	0.2418	0.2825	0.07
	13		30.3		24			
	15		33.8		23			
	17		37.3		22			
	19		40.8		20	0.2684	0.3181	0.05
	21		44.4		18			
	23		47.9		16			
	25		51.4		13			
	27		54.9		10	0.2882	0.3497	0.04
	29	12	58.4		06			
	31	13	01.9	+43	02			

Tempel's Periodic Comet.—This comet was found very near the predicted place on the morning of May 9 by Mr. Finlay an astronomer at the Cape of Good Hope. The observed position of the comet was as follows:

May 8.6628 Gr. M. T.; R. A. $23^h\ 45^m\ 21^s.1$; Decl. $-4°\ 51'\ 18''$. This gives as the corrections to Mr. Schulhof's ephemeris $+47^s$ in R. A. and $-1'.9$ in Decl. remarkably small corrections considering the fact that the comet has not been observed since 1878. The comet is described as circular, less than 1′ in diameter, 11 magnitude or fainter, with some central condensation and no tail. It is now at its greatest theoretical brightness but in a quite unfavorable position for observation in the northern hemisphere, owing to the morning twilight in which it must be observed.

Ephemeris of Comet *a* 1894 (Denning).—From Dr. Krueger's elements as given in A. N., Vol. 135, p. 135, I have computed the following ephemeris:

Gr. M. T.	App. R. A. h m s	App. Decl. ° ′	Log r	Log Δ
June 1.5	12 17 29	+5 20	0.3030	0.1514
3.5	20 23	4 49		
5.5	23 16	4 18	0.3123	0.1744
7.5	26 7	3 48		
9.5	28 57	3 19	0.3214	0.1966
11.5	31 46	2 50		
13.5	34 35	2 21	0.3304	0.2183
15.5	37 22	1 53		
17.5	40 8	1 26	0.3391	0.2393
19.5	42 54	0 59		
21.5	45 40	0 33	0.3478	0.2552
23.5	48 25	+0 7		
25.5	51 9	−0 19	0.3563	0.2791
27.5	53 53	0 44		
29.5	12 56 36	−1 9	0.3647	0.2978

O. C. WENDELL.

Harvard College Observatory, May 15, 1894.

Gale's Comet was picked up here on the evening of April 30, at half past eight 75 meridian time, in R. A. $7^h\ 43^m\ 20^s$; $-18°\ 40'$. I should have seen it two or three nights before this, only that my residence, about one hundred feet distant from the Observatory, cuts off my horizon in that direction. In all but two di-

ever, that by using a high power eye-piece with its small field, the comet could be kept central in that small field very easily without cross wires. The resultant photograph was very satisfactory in this respect. The plate used was a Cramer "Crown," and the exposure one hour and twenty minutes.

WILLIAM R. BROOKS.

Smith Observatory, Geneva, N. Y., May 17th, 1894.

A Comet in the Corona of April 16, 1893.—To day the Lick Observatory received positive copies (on glass) of the eclipse negatives taken by the English expeditions to Brazil and Africa. I can now annouce that the object, to which particular attention was called in the October and April numbers of this journal,* is a comet.

On one of the negatives taken in Brazil identically the some form is shown as has already been described in the April number of ASTRONOMY AND ASTRO-PHYSICS except that the distance of the object from the Moon's outline is greater by ten minutes of arc. On two of the African plates the same object is very faintly shown at a still greater distance from the Moon's outline. The comet was apparently moving in the direction of the slender but conspicuous streamer (also shown on the English photographs) described in the April number of this journal. The following measures refer to a point on the sharply defined *inner* boundary of the tail which is nearest to the Moon's outline.

Place.	Distance Moon's diameter = 1.00	Remarks.
Mina Bronces, Chile.	0.88	Conspicuous and very certain.
Brazil.	1.19	" " " "
Africa.	1.50	Faint.

Allowing for the difference of absolute time at the three stations the resulting geocentric daily motion is three and one-quarter degrees. The object was apparently diminishing in brightness quite rapidly, which indicates that its actual distance from the Sun was small and not simply due to projection. To remove if possible all doubt as to the position of the object on the African plates the original negatives should be specially examined. By giving a slight vibratory motion to the plate in its own plane very faint contrasts can be more readily detected. *There is not the slightest doubt with reference to the Chile and Brazil negatives.*

Lick Observatory, May 7, 1894. J. M. SCHAEBERLE.

Elliptic Elements of Denning's Comet *a* 1894.—In No. 317 of the *Astronomical Journal* Professor Boss gives the following elements which indicate that this comet belongs to the Jupiter family, having a period of less than 8 years:

Epoch: 1894 April 27.5 Greenwich M. T.

$$M = 9^\circ\, 31'\, 45''.28$$
$$\left.\begin{aligned}\omega &= 46\;\; 56\;\; 20\;.2\\ \Omega &= 83\;\; 52\;\; 10\;.4\\ i &= \;\;5\;\; 34\;\; 33\;.3\end{aligned}\right\}1894.0$$
$$\varphi = 45\;\; 18\;\; 19\;.6$$
$$\mu = 446''.8386$$
$$\log a = 0.5999040;\ \text{period } 7.94 \text{ years.}$$

Mr. Schulhof gives similar elements in *Astronomische Nachrichten* No. 3227, making the eccentricity less, however, so that the period is only 6.74 years. He finds by Tisserand's criterion that the comet may be identical with that of Grischow (1743 I) or that of Blanpain (1819 IV). He gives the following comparison in which *n* is the test quantity, *l* is the longitude where the comet may approach nearest to Jupiter and the other letters represent the elements of the orbits:

	n	π °	Ω °	i °	e	a	l °
Comet Grischow	0.525	95	89	2	0.72	3.09	271
Comet Blanpain	0.517	69	79	9	0.71	3.11	248
Comet Denning	0.517	130	85	5	0.68	3.57	287

* 1893, page 732; 1894, page 307.

NEWS AND NOTES.

Subscribers will please remember that the next number of this publication will be issued for August. None will appear in July.

In view of the very superior articles of this number we have given larger space than usual to ASTRO-PHYSICS, and although we have gone beyond our usual limits we have still been obliged to omit much useful minor matter.

Double Star Near H 3950—The faint pair of stars, near H 3950, referred to by Mr. Sprague in the May number of *Astronomy and Astro-Physics*, (p 417) was first noted in the low-power sweeps at Madison) *Pub. Washburn* Observatory Vol II), and subsequently measured by Comstock in his review of those stars (Vol VI). It is much too wide in distance and too faint in magnitudes to be of any interest as a real double star. S. W. B.

Professor Glassenapp's Double-Star Measures in 1892.—The measures of double-stars made by Professor S. Glasenapp at Abastuman in 1892 have just appeared in a handsomely printed volume issued from the press of the Imperial Academy of Sciences of St. Petersburg. The observations include about six hundred pairs, principally from the Dorpat catalogue, each star being observed as a rule on two nights. The other pairs measured are from the OΣ and β catalogues. The measures are excellent, and the stars judiciously selected considering the aperture of the equatorial.

It is to be regretted that this work could not have been continued, as the atmospheric conditions were found to be very favorable both as to the steadiness of the air, and the number of working nights. The elevation is some three hundred feet higher than Mt. Hamilton, and in point of latitude is much more favorably placed for observing south of the equator than any of the other Russian Observatories. From the amount of work done in the comparatively short time the Observatory was in active operation, it is evident that Professor Glasenapp made good use of his opportunities. Visitors to the Russian exhibit at the World's Fair will recall the beautiful photographs contributed by Professor Glasenapp of the picturesque Mountain Observatory at Abastuman. S. W. B.

Eccentricity of the Orbit of the Companion to Algol.—In the case of Algol, the total duration of phase, *i. e.*, the interval between the time when the light begins to diminish, and its complete restoration, must, at least approximately, correspond to the interval between first and last contact, speaking of the phase as a partial eclipse. This must be the time taken by the companion to describe an arc equal in length to the sum of its own diameter and Algol's. If the orbit is circu-

lar, this time will be the same for all parts; but if, as is more proabable, it is eccentric, the time will vary owing to the varying velocity; and if it is as eccentric as is usual in the case of binary stars, the times for different parts must vary very considerably. If, then, there is a secular variation of periastron, the different minima will correspond to different parts of the orbit, and there ought to be a variation in the duration of the phase.

But there is an observed variation in the intervals between the phases; and from this fact Chandler has proved that there is a third invisible member of the system, and has calculated its distance and mass. This third body must perturb the close companion, and in particular, must cause a rotation of periastron.

Knowing the mass and distance, the amount of this perturbation could be calculated on different assumptions as to the form of the orbit, and in this way the eccentricity deduced, if there have been any observed irregularities in the duration of the phase; if none have been noticed, it seems to me that it would be well to observe the duration, in the hope that at some future time the eccentricity could be computed.

Of course the variation of periastron may be so slow that no observable effect would be produced, except in a very long space of time, but it seems to me that it is not unlikely to be fairly rapid.

There are two other perturbations which would cause a change in the duration of phase, *viz.*: (1) an alteration in eccentricity, (2) variation of the plane of the orbit, causing the companion to travel along a chord of the disc of the primary, more or less remote from a diameter, than the one it traverses now; this would alter the magnitude of the phase as well as its duration.

Changes due to these latter causes would, however, be much slower, and in comparison insignificant. J. R. HOLT.

6 Harrington Street, Dublin.

Shape of the Discs of the Satellites of Jupiter.—In Dr. Barnard's article on this subject in ASTRONOMY AND ASTRO-PHYSICS for April there is an assumption which will bear criticism. He considers that the ease of detecting any malformation is mainly a function of the aperture of the telescope, and consequently decides that the Arequipa observations on Jupiter's satellites must stand or fall by the verdict of the Lick instrument. Since the ellipticity of a disc does not vary with the aperture there can exist only the minor differences of size and brilliancy of image between the Arequipa and the Lick observations. The power used in Arequipa was 700 or more and the aperture 13 inches. Dr. Barnard used a power of 1,000 or more with an aperture of 36 inches. The discs he saw were therefore one and one-half times as large and their brilliancy nearly four times as great.

The most favorable conditions of light and size of image for detecting ellipticity must vary with the observer. According to my recollection of the appearance of the satellites at Arequipa, IV might have been brighter to advantage, while I, II, and III were bright enough to satisfy all the needs of observation; and it seems to me quite possible that much increase would have rendered their shape more difficult of observation through irradiation. In the matter of size of image the Lick telescope had the advantage, yet it was not very great. To express it roughly Dr. Barnard saw the discs about one-half of the apparent diameter of our Moon to the naked eye, and we saw them one-third of the same.

I desire also to call attention to the marked difference between individuals in their power of observing minute ellipticities After I had for some time seen I elliptical, I observed II many times before acknowledging that it was not per-

manently round, thinking Professor W. H. Pickering mistaken in his observations on it. Yet after a time my eye seemed to acquire the power of seeing small errors in shape and our position-angles, taken independently, rarely exhibited unreasonable disagreement.

I conclude therefore that thus far no very great advantage lies with the great aperture of the Lick telescope, and that atmospheric conditions (having regard to the diameter of the lens, because a larger lens requires a more steady atmosphere) and individuality of the observer, enter largely into this question. As Professor Holden says, "everything is not yet settled with respect to Jupiter's satellite system." A. E. DOUGLASS.

Lowell Observatory, Flagstaff, Arizona, May 6th, 1894.

Note Relative to β 101, 9 Argus, $\alpha = 7^h\ 46^m$, $\delta = -13°\ 35'$.—In No. 116 of ASTRONOMY AND ASTRO-PHYSICS, Mr. Burnham has derived for this star an orbit which indicates extraordinarily rapid motion of the components during the past two years. This motion is confirmed by the following observations which I have made with the 40 cm. equatorial of the Washburn Observatory.

	p	s	Power.	
	°	"		
1894.230	292.5	0.45 est.	792	Blurred.
246	284.8	0.35	792	Blurred but separated
258	282.6	0.29	1540	The quadrant is right.
1894.24	286.6	0.36		

The measured distance agrees well with the ephemeris given by Professor Glasenapp, *Monthly Notices*, March 1894, but the position-angle is eleven degrees greater than is given by the ephemeris. GEORGE C. COMSTOCK.

Comet Medals. In *English Mechanic* April 27, 1894 our excellent friend W. F. Denning, of England, appears to be guilty of scientific heresy. The charge is based on the following paragraph, page 220:

"I have written to Professor Holden of the Lick Observatory declining the Comet medal of the Astronomical Society of the Pacific. I quite fail to appre-

of Northwestern University, Evanston, Ill., Professor G. W. Hough, President, in the chair.

Professor Henry Crew read the first paper of the evening on "*The Concave Grating and its Relation to Spectroscopy, illustrated by Apparatus in the Laboratory.*" The speaker, in introducing his subject, sketched the work of Fraunhofer on Spectra, and discussed the early work of Rutherford and others in constructing gratings for spectroscopic purposes. He developed the principal formulæ employed in the theory of the grating, and showed how the whole theory of the spectroscope followed from one formula. He then spoke of the invention of the concave grating by Professor Rowland, who had also perfected his discovery both theoretically and practically. Dr. Crew explained by diagrams the construction and use of the apparatus, and pointed out its particular advantages, especially in securing a normal spectrum. After explaining the mathematical theory of the apparatus, the speaker invited the members of the Academy to the Laboratory, where he exhibited the practical working of a fine concave grating.

The second paper of the evening was read by Professor Malcolm McNeill on "*The Life and Works of Kepler.*" The speaker gave an interesting account of Kepler's youth, his education, and his early religious dissent, which influenced his whole career and caused him to be continually persecuted, although ever since leaving the University of Tubingen he had desired an official position in the church. Professor McNeill then gave an account of Kepler's relations with Tycho, and alluded to the great difficulties he afterwards encountered in securing the use of Tycho's manuscripts from his heirs. He then sketched the career of Kepler in discovering the laws of planetary motion, and pointed out the difficulties which were continually encountered and finally overcome.

The speaker called attention to the importance of Kepler's discovery that the planes of the orbits of the planets pass through the Sun, and remarked that the order of Kepler's laws as ordinarily given is not historically correct, the law of equal areas being the first discovered. He said that Kepler had worked twenty-two years on the harmonic law, the squares of the times of revolution are as the cubes of the major axes, and that he had regarded this as his principal discovery. Kepler's manuscripts had been preserved, and finally secured by the Empress Catherine of Russia, so that they are now preserved in the Observatory of Pulkowa. The Opera Omnia edited by Fritche gives the most authoritative information regarding Kepler and his splendid discoveries, which laid the foundation for Newton. Professor McNeill was of the opinion that while Kepler had the true spirit of a discoverer, he had also the character of a mystic, and that these two traits appear clearly in his writings.

After some discussion, the section withdrew to the Dearborn Observatory, where the 18-inch telescope was placed at the disposal of the members. *G.*

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These books can be bought of Messrs. Wm. Wesley & Son, Essex St., Strand, London, England.

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CONTENTS FOR MAY, 1894.

PLATE XVII.

ELECTRICAL CONTROL OF EQUATORIAL

G. W. HOUGH, DEARBORN OBSERVATORY, NORTHWESTERN UNIVERSITY, EVANSTON, ILL.

ASTRONOMY AND ASTRO-PHYSICS, August, 1894

Astronomy and Astro-Physics.

VOL. XIII, No. 7. AUGUST, 1894. WHOLE No 127.

General Astronomy.

PHOTOGRAPHIC DETERMINATION OF STELLAR MOTIONS*

EDWARD C. PICKERING.

A great advantage of photography as a means of studying astronomical phenomena is the ease with which a vast number of facts may be collected. These facts are recorded in permanent form and later may be deduced or verified. An attempt has accordingly been made at the Harvard College Observatory to detect by photography stars undergoing considerable change in position either owing to parallax or proper motion. Two methods have been employed for many years in comparing photographs of the same parts of the sky. First, by superposing two negatives of the same region, in which case the images of the stars will form curious sets of concentric circles, if the plates are not exactly oriented. Since the two films are separated by the the thickness of one plate of glass, small differences in position cannot readily be detected in this way. Secondly, if a contact print is made from one negative and the other negative is superposed on the positive thus formed, the dark images of one plate should fill the light spaces in the other, and give a nearly uniform surface. In actual practice this method was found here less satisfactory than might be anticipated, although highly recommended by Professor Barnard (*Astron. Nach.* CXXX, 77). A cluster appears to be the best object for this method. If the plates are not exactly superposed each star appears to project slightly from the background and to cast a shadow on one side.

A third method has accordingly been tried. A photographic plate is placed in the plate-holder with the film side away from the objective, the photograph being taken through the glass. The character of the images thus obtained does not seem to be affected. Theoretically, the plate-holder should be moved towards the objective by two-thirds the thickness of the plate, but this correction is scarcely perceptible, with a large telescope. When such a photograph is superposed upon a photograph taken in the us-

* Communicated by the Author.

ual way, the two films being placed together, all the images of one should appear to coincide with those of the other the difference in the planes of the two images not being noticeable even when viewed under a considerable magnifying power

This last method has been in use for the past three years at the Harvard College Observatory with the 11-inch Draper telescope at Cambridge and with the 13-inch Boyden telescope at Arequipa A number of objects, about a hundred and fifty in all, have been selected, including variable stars of long period, of short period, Algol stars, stars whose spectrum is of the third, fourth or fifth type, binary stars, stars having large proper motion, etc. The two dates are computed on which the longitude of the Sun should differ 90° from the longitude of each of these objects. Within a few days of each of these dates, two photographs are taken one with the plate in the usual position, the other with its film reversed Generally, an exposure of about ten minutes was given to each. The first of these plates was taken in August, 1891.

The examination is made by superposing one plate upon another taken six months later and in the opposite position. Thus a plate taken in January with the film turned toward the objective is placed upon the plate taken in July with the film turned from the objective. The other two plates are also superposed. Instead of making the images exactly coincide, one plate is moved so that all of its images shall be exactly north of those on the other plate by a very small amount, for example, 10″. The plates then appear to be covered with double stars having the same position angles and distances with components nearly equal in brightness. If now the two images of any star differ in brightness it may be variable, and if the position angle is different from that of the adjacent stars it may be suspected of proper motion, or of sensible parallax. In any case, confirmation may be obtained at once from the other pair of plates. A third pair of plates should always be taken one or more years after one of the first dates and thus serve to distinguish between parallax and proper motion. In the actual examination a microscope is used having a field rather more than a centimetre in diameter and traversed by a vertical cross-wire. Sweeps are made moving the plate in right ascension, and after each sweep changing the declination by moving the plate one centimetre at a time, until the whole plate has been examined. As each star in turn is brought past the cross-wire the direction of the line connecting its components is determined with much accuracy. The diameter of the images should not exceed two to four seconds of arc and they

may be placed about ten seconds apart. A parallax of half a second will then change the position angle by about five degrees, a very noticeable quantity. The conditions under which the plates are taken give nearly the maximum value of the displacement in right ascension due to parallax. This corresponds to a change in position angle to which the eye is much more sensitive than to changes in distance. Any suspected objects are marked upon the photographs and confirmed or not on the other plates. If the change in position is real, the same method may be used with great advantage for determining its amount. In the usual method, it is necessary to measure the position of each star from several adjacent companions which involves measurements of several hundred seconds and the application of various corrections for difference of scale, orientation, differential refraction, etc. The results are then brought together and the outstanding differences are discussed. In the present case the work is purely differential. We have only to measure the position angles and distances of what appear to be double stars whose components are nearly equal and to which any convenient position angle and distance may be given. Personal equation dependent on position angle is evidently eliminated. Such effects as differences in scale of the two plates, error in orientation and differential refraction then appear only as linear terms whose values are readily determined. Incidentally an inspection of these plates shows that no slipping of the film is sensible, but such a source of error, if it occurred, would affect all determinations of position from photographs. Although considerable progress has been made in taking the photographs much time has not yet been spent in studying them. An examination has been made by Miss L. D. Wells of 1436 stars on eight pairs of plates, and shows that probably none of these stars have a parallax of as much as half a second. Ordinarily, a pair of plates can be examined in about half an hour.

A preliminary measure of the positions of the images of eight stars in the vicinity of the variable star T Cassiopeæ, gave the average deviation of the uncorrected differences 0″.23 which would correspond to a probable error in the parallax of but little over a tenth of a second as derived from measures of a single pair of plates.

Evidently in a few years the value of these plates will be greatly increased as a means of measuring proper motions. In ten years the work should be repeated and a proper motion of one-tenth of a second would then give a displacement of a second, which, as shown above, would be readily detected by inspection and could be measured with accuracy.

The question has presented itself how far it may be best to photograph the entire sky with the Bruce telescope with plates in both positions, for determining the proper motion of large numbers of stars. The advantages of this method increase in many respects with the focal length of the telescope. The ease with which photographs suitable for this work may be taken with any photographic telescope is the reason for the present publication of a description of this method.

HARVARD COLLEGE OBSERVATORY, Cambridge, U. S.
JULY 8, 1894.

AN ELECTRICAL CONTROL FOR THE EQUATORIAL.*

G. W. HOUGH

The increased application of photography to astronomical work, makes it desirable to secure uniform motion for the telescope with which the photographic plate is connected. It is not only necessary, however, to secure uniform motion, but this motion must be precisely the same as the apparent diurnal motion of the heavens.

The use of electricity for controlling the equatorial is not new, during the past few years a number of instruments have been provided with electrical attachments of various kinds. In the application of the electro-magnet, to the telescope, however, no new principles are involved which were not worked out many years ago in connection with the chronograph.

More than ten years ago, Professor S. W. Burnham and myself discussed the feasibility of an electrical control for the $18\frac{1}{2}$-inch refractor of the Dearborn Observatory; but so long as the driving-clock ran fairly well, I did not appreciate the importance of anything better, and hence nothing was done in the matter.

The driving-clock of the $18\frac{1}{2}$-inch is a Bond spring-governor—this apparatus by means of occasional repairs and adjustments ran perhaps as well as a majority of such appliances. The rate, however, changed from night to night, and in cold weather the change of rate was so considerable as to become a source of great annoyance in making micrometer measures.

An electrical control for securing uniform circular motion, was first applied in 1849, to a disk chronograph, by Professor O. M. Mitchel, Director of the Cincinnati Observatory. Professor Mitch-

* Communicated by the author.

el applied the electro-magnet directly to the driving shaft of the chronograph and hence it was required to be of sufficient power to sustain the whole of the driving weight if necessary. During my use of this chronograph, it occurred to me that much less work would be required to be done by the electro-magnet, provided the controlling power was applied to a rapidly revolving shaft.

In 1869, I constructed a recording chronograph, in which the electrical control was applied to a shaft revolving once each second. I subsequently used the same method for my recording and printing chronographs.

A short time since I concluded to apply the same control to the 18½-inch equatorial of the Dearborn Observatory. As this method of control is exceedingly simple and is also positive in its action, a description of its salient features may be of value.

The pendulum and escapement arms of the driving-clock were removed and an electro-magnet substituted as shown in the diagram. The office of the arm, E, of the electro-magnet, M, is simply to hold the clock-work until unlocked by the operation of the electro-magnet. The clock-train, in this apparatus is controlled by a fan, and is regulated to always run fast. The rate of the telescope driving train, when controlled by the electro-magnet, must therefore be precisely the same as the rate of the clock which operates the electro-magnet, M. In other words the control is absolute; the only error being in the imperfection of the worm-gear. The shaft which is locked by the electro-magnet makes a revolution in about 1.016 seconds and hence the standard clock for operating the magnet must be rated to lose approximately 60 seconds hourly on sidereal time. The clock used in connection with the equatorial, has a compensated seconds' pendulum and Graham escapement. As this control has been in use only a short time, the error due to the worm has not been ascertained with great precision; it will, however, appromimate 10″ of arc.

For micrometer work a slight irregularity in the motion is a matter of no consequence, but for photography, the worm should be as perfect as possible.

The following is an average specimen of the performance of the driving-clock controlled electrically.

The first column is the sidereal time, the second, the displacement of a star as measured with the micrometer.

April 26.	Sid. T.	Error	Sid. T.	Error.
	10 30	− 0.0	10 55	− 9.0
	35	− 5.0	11.00	− 6.2
	40	− 8.7	05	− 6.2
	45	− 8.0	10	− 6.5
	50	− 8.0	15	− 2.5

From this table it is seen that the maximum due to the worm displacement of 45 minutes was 9″ of arc. The displacement would be no greater than this for any interval of time.

I have made stellar photographs of the cluster in Cancer, with the finder 47-inch focus, with an exposure of 20 or 30 minutes, without adjustment during the exposure.

The star disks appear practically round when examined under a microscope.

For short-focus star-cameras, the displacement due to imperfections in the worm would be a matter of no moment, and it seems feasible to make such photographs automatically, and without the constant attention of the astronomer.

There are in the United States a number of Clark telescopes provided with the Bond spring-governor, which may be electrically controlled at a very trifling expense. No change is required in the clock-work, but simply to lock the revolving arm on one side only, by means of an electro-magnet, in place of the escapement arms which are removed.

The advantage of the absolute control for ordinary micrometer work is so great, that any one who has used this method would not be satisfied with a spring-governor, or any other form of unstable control.

The spring-governor, as is well known, locks the train twice every second, and theoretically is better than a seconds' control, but practically, both for the chronograph and the equatorial, it is not necessary. The disturbance of the telescope, even with the highest magnifying power, is scarcely perceptible, when the locking is performed only once each second. By using a double locking arm, however, and a half-second pendulum for operating the electro-magnet, this objection is removed.

For a long focus telescope like the 36-inch Lick or the 40-inch Yerkes of Chicago, theoretically a half second control would be preferable. Owing, however, to the vibrations of a long tube by the wind, one cannot secure very great stability with the ordinary equatorial mounting, and hence the disturbance due to the control is hardly worth consideration.

In the January and April numbers of this journal, Professor W. H. Pickering and F. L. O. Wadsworth have proposed the use of

an electric-motor as the prime mover to carry an equatorial telescope. The use of an electric-motor for winding the clock and for shifting the telescope or other apparatus, in many cases is of great value and is to be commended; but its use as a prime mover for securing uniform motion at a fixed rate of speed, in my opinion is not a promising problem. The speed of the motor will depend on the electro-motive force of the circuit which is not constant.

An electric-motor has already been used as the prime mover for a chronograph.

About forty years ago it was thought that a true gravity pendulum could be secured by employing an electro-magnet to unlock the gravity arm. In neither of the cases above mentioned was the result satisfactory, for the reason that the strength of the electric current is not the same for any considerable period.

If a motor is used to drive the telescope, nothing would be saved in the way of clock gearing, and I imagine the control for rate would be more difficult than if gravity from the fall of a weight were employed.

In the practical use of an equatorial, the great desideratum is a driving-clock which will start instantly and carry the telescope at a uniform and fixed rate. In my experience, I have lost a good deal of time and occasionally important observations from the bad performance of the driving-clock. If a driving-clock is used which will take care of itself, then one's whole attention can be given to observation.

DEARBORN OBSERVATORY, Northwestern University,
May 14th, 1894.

SOME NEW FORMS OF DOUBLE MOTION MECHANISM.*

F. L. O. WADSWORTH.

I have used the term Double Motion Mechanism to designate that form of mechanical movement used in spectroscope slits, astronomical refractometers, and similar pieces of apparatus, for moving two jaws or carriages simultaneously in opposite directions, at the same speed. The usual mechanism employed for this purpose consists either of a right and left hand screw longitudinally fixed and working in nuts on the movable carriages, or a system of link work symmetrical about the central line from

* Communicated by the author.

which the motion takes place. Each form of mechanism has disadvantages peculiar to itself, and in order to bring out more clearly the nature of the improvements which it is the object of the present paper to describe, the principal points which will govern the choice of a mechanism for this particular purpose will be briefly considered. They are:

1. Accuracy of movement.
2. Ease and rapidity of movement.
3. Compactness of mechanism for a given range of movement.
4. Simplicity and cheapness.

As regards the first of these points, the screw system is mechanically superior to any form of link mechanism, because of the number of points at which errors due to imperfect fitting are possible in the latter. Indeed it is now possible by Rowland's method to make a single screw accurate, so far as uniformity of pitch is concerned, to almost any required degree. The slight difference in pitch between the right and left hand portions of the screw may, if great accuracy is required, be compensated by making the driving nuts independent and separate from the driven jaws or carriages (as in a dividing engine), and suitably inclining or curving the guides along which the tail pieces of these nuts slide. The main cause which tends to inaccuracy in the use of the screw is the necessity for placing it, in most cases, unsymmetrically with respect to the guides for the carriage, in order to avoid obstructing the central part of the field. Then the point of application of the driving force is much nearer one side of the carriages than the other, and there is in consequence a tendency to twist the latter in their ways, which can only be prevented by making the bearing surface very long, or by using spring gibs, weights, or equivalent mechanical means for bringing the center of resistance to motion into the line of application of the driving force. The first and better method increases the necessary size and bulk of the instrument, and the second is unmechanical, while both considerably increase the friction.

As regards ease of motion there is no question of the superiority of a well designed and a well constructed system of link work, for not only may the driving force be more directly and centrally applied, but the links which move the carriages may also be used to support and guide them, and all sliding friction thereby avoided. The motion too is more perfectly under control than with the screw, and may be made much more rapid without increasing the tendency to vibration or jar. Unfortunately, if any

considerable range of motion has to be provided for, the necessary link mechanism becomes bulky and cumbersome, and if compactness is essential the screw is almost a necessity. The last point is however of minor importance except in the case of large instruments when it needs to be carefully considered.

The fourth point is worthy of more consideration than it ordinarily receives. Upon the simplicity of the mechanical design depends, in the first place, its successful operation under the severe conditions of actual usage. It may be taken almost as a general principle that a change of design or construction which cheapens first cost without impairing efficiency, makes the instrument simpler and to that extent more reliable. To explain more clearly what is meant by dimishing cost without impairing efficiency, it may be stated that work which can be done on the lathe is in general cheaper and at the same time more accurate than work finished on the planer or milling machine, and that parts so shaped that they may be made up from standard stock material cost much less than they would if they required special castings to be made for them. Unfortunately the scientist who designs the instrument is not usually acquainted with the details of the mechanical work and is therefore unable to determine the best and cheapest construction for the whole or for any given part, while the mechanic on the other hand, while perfectly prepared to answer the latter question when he fully understands the purpose to be achieved, is ofttimes liable to make grave blunders if left to work out details by himself.

As to relative cheapness and simplicity of these two forms of mechanism much depends on the particular instrument in which it is to be used. In general the link form will be cheapest and simplest in those instruments not requiring great accuracy, nor involving a large range of movement, and the screw form in instruments where these latter qualities are of primary importance.

From what has preceded it is evident that the point of greatest advantage possessed by the link form of mechanism is the ease and smoothness of motion, and that its greatest point of disadvantage is its bulkiness and want of accuracy. The problem with it is to combine compactness with a large range of motion, and accuracy with ease of motion and simplicity. In the case of the screw the main problem is to secure a central driving action on the carriages. To show how these objects have been, to some degree, at any rate, accomplished in particular cases, I will briefly describe some instruments representative of those mentioned at the beginning of the article.

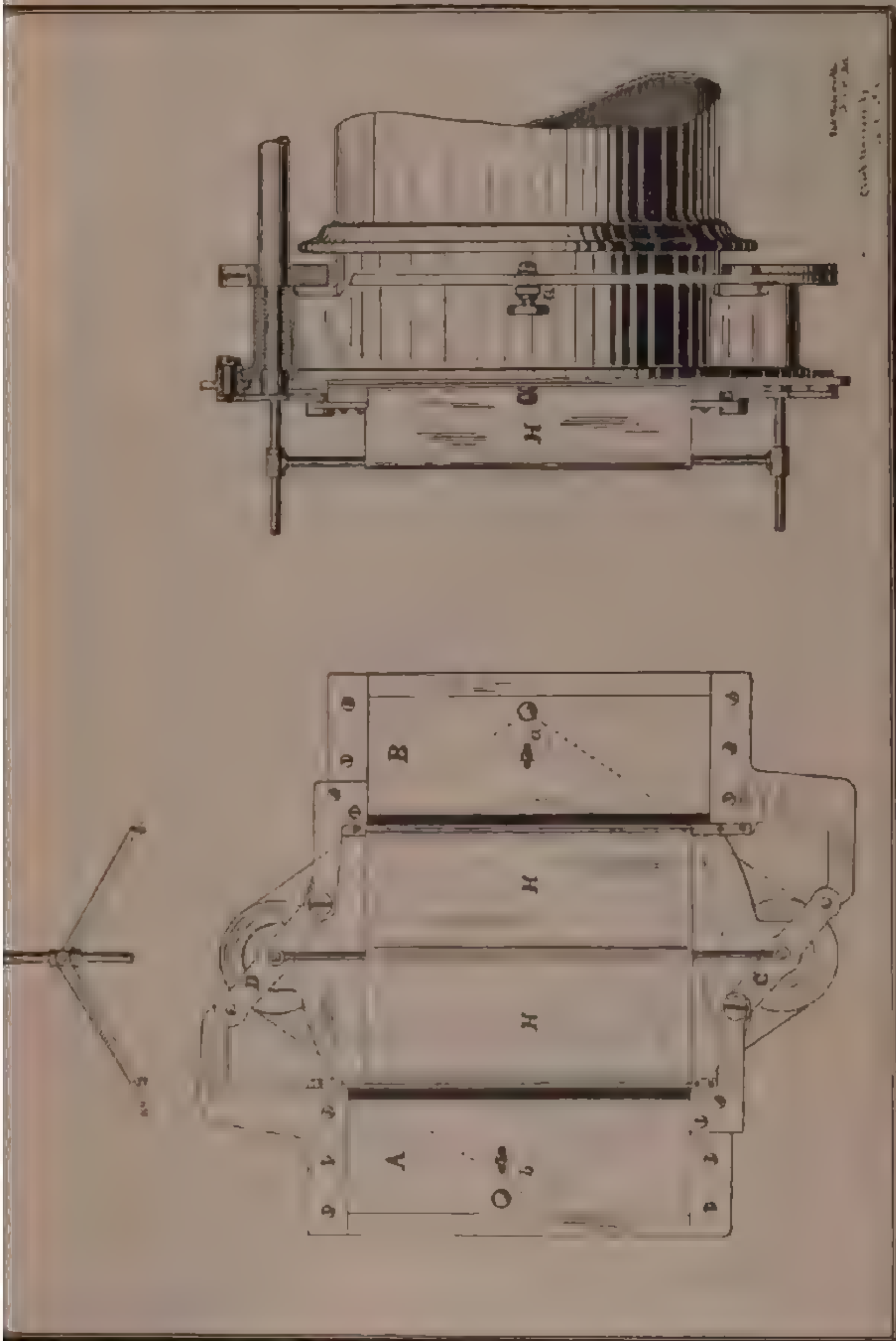
The first one is that form of astronomical refractometer recently invented be Michelson[*] which consists of two moveable slits placed in front of the objective of an ordinary telescope. In the first instrument of this kind constructed[†] the slit plates were moved in an out by means of a right and left hand screw geared to a rod running along the side of the telescope to the observer. Twisting of the slit plates by reason of the unsymmetrical position of the screw was in this case prevented by the use of spring gibs which held the jaws in close contact with the guide nearest the screw. This instrument was, however, unsatisfactory in its operation because of the slowness and stiffness of the motion. To overcome these difficulties in the second instrument a form of link work similar to that used in many spectroscope slits was tried. This instrument is shown in Plate I. The two slit jaws, A, B, are pivoted to and moved by the double-ended cranks, C, D, which are themselves journaled on the short tube which slides over the end of the telescope. If either one of these cranks are turned the other will of course turn with it, the two slit plates acting as connecting rods, while they themselves receive a lateral motion. In order to make the arrangement as compact as possible the parts are so arranged that the cranks have a motion through nearly 180 degrees, enabling their full throw to be utilized. To carry the driver-crank past its dead center it was necessary to gear the two together by means of two steel bands (shown by the dotted lines), which were carried around the cap by means of two adjustable idle pulleys, *a*, *b*, and fastened to two drums on the crank shafts. A folding screen, HH, whose function was to keep that part of the objective between the slit plates covered, completes the arrangement, whose methods of operation will be readily understood from the drawings and the description just given Although far better as regards ease of motion etc., than the first instrument, it was not entirely satisfactory. Unless the steel bands were quite tight, which caused considerable friction, there was more or less binding and sticking as the cranks passed through the dead center position Again there was nothing to support the slit plates at the outer sides, except the face of the mounting and there was in consequence considerable friction between them and this face.

In the third and still larger 12-inch instrument with which the diameters of Jupiter's satellites were measured by Prof Michel-

*Application of Interference Methods to astronomical instruments. A. A. Michelson, Phil. Mag. July, 1890.

† Ibid, Fig. 1, Plate II.

PLATE I.



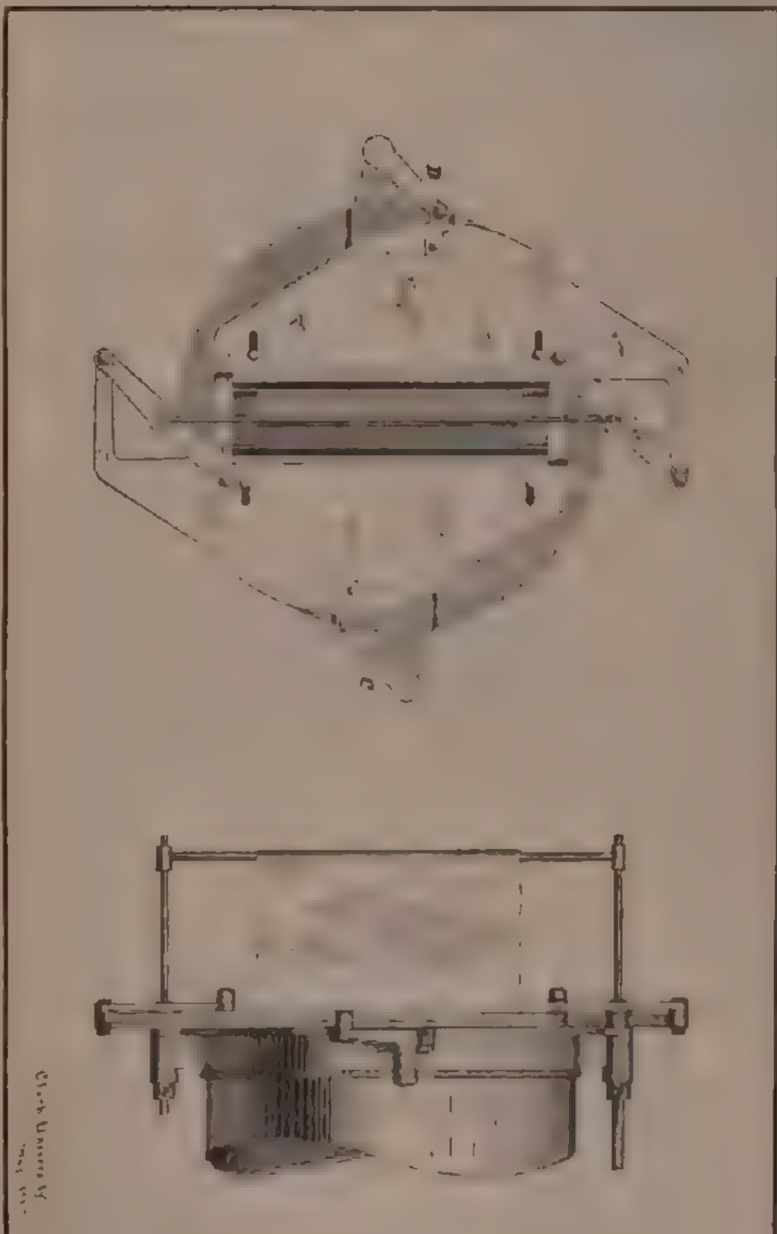
son* at Lick Observatory, these last difficulties were completely overcome by mounting each slit plate on three cranks instead of two as in the previous design. A rough sketch of the instrument is given in the paper referred to below but for the sake of comparison and discussion the more detailed original drawings are here reproduced (Plate II).

In general, as will be seen by an inspection of the drawing, the design is the same as in the instrument just described. The novelty and great improvement lies in the introduction of two additional cranks or links, D, C, placed about 90 degrees from the two double-ended driving cranks, A, B. Each slit plate therefore swings on three points of support, entirely eliminating any sliding friction between the plate and the face of the cap. But the main point of advantage which this arrangement possesses is that with it there is no longer a point of dead centers, and hence not only is the smoothness and ease of motion improved, but the accuracy is also increased, as the slit jaws must always remain rigidly parallel to each other and at equal distance from the line joining the centers of rotation of the two double-ended cranks. The only mechanical conditions which it is necessary to fulfil, are that the six crank elements shall be of equal length; that the two elements of the double-ended cranks, A, B, shall lie in the same straight line, and that the axes of rotation shall all be parallel; all of which conditions may be fulfilled to a degree of accuracy at least as great as would be reached, with the same degree of labor, with any form of screw movement. Accuracy, however, is not so important in this case as those qualities which have already been pointed out as characteristic of link mechanism, together with that one not usually possessed by it, viz., compactness. How far this latter feature has been secured in the present design may be judged from the fact that when closed no part of the mechanism, which has a range of motion equal to the full aperture of the telescope, in this case 12 inches, projects more than 3¼ inches beyond the side of the telescope tube at the objective end; an amount barely sufficient to allow the driving rod, which passes back to the observer at the eye-piece end, to clear the enlarged central portion of the telescope tube.

It is evident that the use of three-crank mechanism, if such it may be termed, is of wide application. The high efficiency of link-work trains, together with their cheapness and durability, render the use of them extremely desirable in mechanism, and

* Measurement of Jupiter's Satellites by Interference. A. A. Michelson. Publications of the Astronomical Society of the Pacific, 1891, Vol. III. p. 274.

PLATE II.



they would no doubt be more universally used were it not for the fact that special means are usually required to overcome the dead points. In the case of the two shafts which are to be driven, one from the other, at a constant velocity ratio (the driving shaft of a locomotive for example), this is done by placing on each shaft two cranks set at an angle to each other (usually a right angle), and using two connecting rods The three-crank mechanism accomplishes the desired result in a considerably simpler, more compact and less expensive manner, since it requires only one set of cranks and one connecting rod. Simple and efficient as this device is I have never seen it described in any book of mechanical movements nor in any previous publication; and if it has been before used it does not at any rate appear to have been commonly known.

The other form of double motion mechanism which I wish to describe is one of recent design and belongs to the screw variety. The object in view was to secure a mechanism which, while retaining all the advantages secured by the use of the screw in the way of accuracy, compactness, and convenience of operation and reading, obviated the single bad feature of dissymmetry already alluded to. The manner in which this was accomplished may best be described by reference to Figs. 1 and 2, and 3 and 4, Plate III, which represent respectively rear and side elevations of a large double-motion slit and double-motion micrometer recently constructed after my designs by Grunow; to whose excellence of workmanship the highly successful performance of both of these pieces of apparatus is largely due.

Referring first to the drawings of the slit (which is perhaps incidentally of interest as being the largest spectroscope slit ever constructed, it having a clear opening of ten cm.*) Fig 1 is a rear view, the back plate being removed to show the mechanism clearly. The two-slit jaws, *a*, *a*, slide between guides, *b*, *b*, screwed to the front of the slit plate, and each has a lug, C, extending through this plate and projecting about 1 cm., behind it. To the left hand jaw is screwed a ⊐ shaped bar, D, the side arms of which pass around the ends of the slit and the center of which is opposite the lug on the right hand jaw. The lug, C, is tapped to receive a screw, F, on which are two threaded portions, one engaging with the thread in the lug and the other of just one-half the pitch of the first, engaging in a nut, H, which is screwed to the slit plate.

* This giant slit is used with the great salt train recently completed by Brashear (described in this journal for April) in the bolometric investigation of the infra-red solar spectrum now being carried on at the Astro-Physical Observatory.

PLATE III.

Fig 1

Fig 2

Fig 3

Fig 4

When therefore the screw is turned, say to the right, the jaws are separated by an amount equal to the motion of the point of the screw with reference to the lug C, say a distance x, while the whole jaw system is drawn to the right by the action of the fixed nut H, a distance equal to $\frac{1}{2}x$. The center of the slit, therefore, remains fixed, the jaws opening out from it. A spring bearing against the lug on the left hand jaw provides for the return motion and takes up all back lash in the screw. A circular graduated head, M, gives by its motion over a longitudinally graduated drum, N, the whole number of turns and fractions of a turn enabling the exact width of the slit to be determined at a glance.

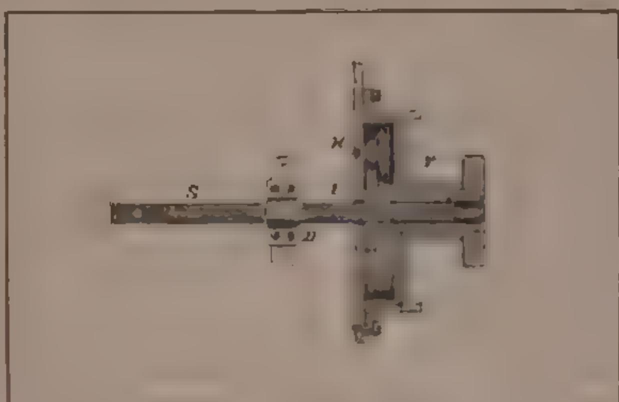
Aside from achieving the chief result aimed at, viz., a central driving action on both jaws, which avoids any tendency to twist and cramp the latter in their guides, and therefore permits shorter slit plates to be successfully used, this form of mechanism presents other advantages over the usual right and left hand screw form. It will be noted that the accuracy of separation depends only on the accuracy of the screw which works in C, the screw at N serving only to keep the jaws centered. In determining the distance between the two jaws we have to deal with the errors of only one screw instead of two as in the case of the right and left hand screw movement.* Still another advantage over the latter is that the motion is positive in one direction only (on opening) and there is therefore no danger of jamming the edges of the slit by turning the screw backward too far.

The drawings of the double-motion micrometer, Figs. 3 and 4, Plate III, are given to show another application of this mechanism and also to illustrate a very convenient form of micrometer for certain kinds of measurement in which it is desirable to have the two images, whose distance apart is to be determined, symmetrically situated in the field of the observing eye-piece. The general design is the same as in the case of the spectroscope slit. One set of micrometer wires is carried on the outer carriage, A, A, which slides between the sides of the micrometer case as in the usual construction. The other set is attached to a second carriage, B, B, which slides between guides on the first one. The separation of the two carriages is effected by the screw, C, working in a nut, D, on the outer carriage, and bearing against a lug on the inner one, the return motion being effected by the spiral springs. The centering of the system is effected as before by a second screw of one-half the pitch of C, which, for the purpose of

* This is of less importance in the case of a spectroscope slit than in the case of the double-motion micrometers next to be described.

securing compactness, is cut on the hub of the graduated drum, N. This method of construction makes the micrometer very compact, while securing great length for the slides. The whole is to be mounted as shown on a second slide on the end of the telescope simply for convenience in placing it at any part of the field of the latter.

One modification of the last form of mechanism which I have only recently designed is interesting, if only in showing how the required motion may be obtained by means of a single screw of the same pitch throughout. This may be accomplished by the same arrangements of parts as before except that the centering nut, instead of being fixed, revolves at one-half the speed of rotation of the screw. In the accompanying figure which shows one simple means for accomplishing it, S is the screw working in the nut, D, on the one carriage and bearing against the lug on the other, as before. In this case the graduated head is held longitudinally and drives the screw by means of a pin or key working in a deep groove cut in the hub of the head. Attached to the head is a fine tooth gear, F, which by means of the two idle gears carried on a fixed pin, H, drive the toothed nut, K, at just one



half the speed at which the screw itself revolves. The drawing itself shows details of construction. The particular advantages of this special arrangement are greater simplicity in making the screw and greater compactness when a considerable range of motion is necessary. If the two carriages to which the desired motion is to be imparted are at a considerable distance apart, as in the second form of astronomical interference instrument described in the paper to which reference has already been made, it

would be better to place the driving wheel and centering nut between the lug on one carriage and the nut on the other. The centering nut, it will be observed, obviates the necessity for any end-thrust bearings and thus eliminates another source of error in the determination of the position of the two carriages.

These two forms of mechanism; the link form, Plate II, and the screw form, Plate III, with these various modifications, which I have described, are only a few of many which I have designed for different purposes, but I have preferred to describe only those which have stood the test of actual usage and have been found more satisfactory than those forms previously in use.

Astro-Physical Observatory,
Washington, D. C., April, 1894.

MARS.*

PERCIVAL LOWELL.

On May 31st, the date at which these observations of mine began, Mars was about 98 millions of miles distant from the Earth, his south pole was tilted $23\frac{1}{2}$ degrees toward her, and he showed gibbous to the extent of nearly one-sixth of his disk. The phase axis lay slightly to the left of the polar one.

These conditions continued substantially unchanged throughout the period covered by the observations, that is, from May 31st to June 24th; the phase reaching its maximum of 47 degrees on June 16th and the tilt of the pole its greatest latitude of 24 degrees on June 22d (Marth). The phase axis meanwhile slowly shifted its position from the left to the right of the polar one. The planet passed through quadrature on June 16th.

The observations were thus made from four and a half to four months before opposition and about equally long before the time of the planet's nearest approach; inasmuch as opposition will occur on the 20th of next October, and the minimum distance between the two planets be reached on the 13th of the same month. For satisfactory views to be had so unprecedently long before, so to speak, the event, the seeing, to secure which as good as possible the Observatory site was chosen, is responsible. What that seeing was the accompanying drawings will show. It was such as to enable me to make out a dozen of Schiaparelli's canals *two months and a half before the summer solstice of Mars' southern hemisphere.*

* Communicated by the author.

Most of the observations and all of the drawings, except the first, were made with the 18-inch. I may say here, however, that in questions of planetary detail of the inner planets of the solar system, up to and including Mars, size of instrument is quite secondary to quality of atmosphere. Large objectives give more light than small ones; that is their chief advantage. For faint stars this light is invaluable, but for some of the planets such illumination were better away. Venus and Mercury are best studied in the daytime for this reason. Now want of light is not the difficulty with Mars. During the dark the planet's glare was too great in the 18-inch. Details invariably came out best about three-quarters of an hour after sunrise. Yet the seeing at that hour was on the whole less good than earlier. From these observations and from certain ones of M. Flammarion, I am convinced that the post-sunrise and pre-sunset hours are the best ones for studying the planet. At other times a very faint dark glass might prove advantageous, or better yet, diaphraming the instrument down; then its focal length will tell over a smaller one. Simply increasing the power will not do, for atmosphere, owing to its inevitable lack of homogeneity, prescribes a limit. With the 18-inch at Flagstaff, a power of 420 was about this limit on Mars, although I have used 1280 without sensible loss, though also without sensible gain. The greater number of my drawings were made with 370. On the six-inch 270 showed well, lake Tithonus and the Agathodæmon canal being very evident as a dark line, and Argyre and Pyrrhae coming out with singular prominence—the dark and light markings being more contrasted, it is worth mentioning, than in the 18-inch.

How little this matter of atmosphere is duly appreciated is evidenced by the way in which a large instrument is assumed to be necessarily superior to a small one, quite irrespective of what it is that is to be observed. Now the fact is that there are two quite different classes of celestial phenomena; those dependent on quantity of light and those dependent on quality of definition for their visibility, and the two means to these ends go anything but hand in hand. For the one, the illumination, the size of the instrument is the prime requisite; for the other, the definition, the atmosphere is the first essential. As an object-lesson in this it is worth noticing that the biggest instruments have not always given the best views of Mars. In matters of Martian detail it is amply evident from the results that observer, atmosphere, instrument is the order of weight to be given as the factors of an observation.

One preliminary result of the Arizona air may be worth noticing in the interest of astronomical concord, always so desirable. The quality of the seeing there suggests reconciliation between the very various drawings made generally of the planet. For Arizona seeing, too, had its ups and downs. When the seeing was exceptionally bad, Mars looked strikingly like his own photographs, as I remarked encouragingly to his photographer. A fact worthy of a word, for it seems to be overlooked in these days when celestial photography is the fashion—overlooked, not by the photographers but by the public, as I judge by more than one question put to me on the subject—that though photography will reveal what no eye can see, the eye will reveal what no photographic plate can show. Planetary detail falls under the latter head. For the difficulty of photographing such detail is not simply, as the inquirers suppose, question of a driving clock timed first to the Earth's rotation and then to the planet's pace, which alone would require more perfect apparatus and a more complicated one than any yet devised. The deep difficulty lies in our own atmosphere, which is never steady enough; what is disclosed one minute being swamped by light waves the next. The attentive eye registers each glimpse, the photographic plate only the aggregate, and in the composite picture thus obtained the bad obliterates the good. With faint stars there is no such loss and every gain. For light is all that is wanted, however it be got. Now the eye cannot add impressions; the camera can. If therefore the camera be exposed long enough it will reproduce what the eye could never detect. But, in the case of detail, the longer the plate is exposed the more certainly will the detail be lost. Until, therefore, we rise above our atmosphere or find an absolutely faultless spot, we shall never be able for planetary work, to match the eye by any film, however sensitive or accurately driven.

In the next higher stage toward atmospheric steadiness Mars assumed his old-fashioned, orthodox appearance, quite guiltless of canals and such-like inexplicabilities, an appearance which the admirable drawings of Mr Green have raised to the rank of portraiture, and to the finality of which the Royal Astronomical Society, by its skepticism of Schiaparelli, has done so much to commit itself. Under better air, however, the disc began to show symptoms of heresy to these established Anglican views; until finally in the best moments the planet's surface came out with all the clear-cut character of a steel engraving, as Schiaparelli and Terby have described it. From the manner in which these

MARS.

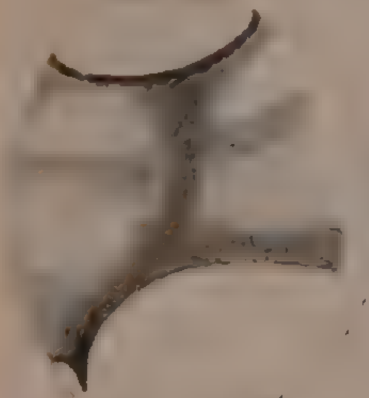


FIG. 1.—May 31, 17h 48m (Standard Mountain Time.) Longitude* of Center of Disk 310°; Power, 240. The Hour-glass Sea on the extreme left. Strait visible connecting it with the Polar Sea. Aëria much the brightest land on the disk. Deucalionis and Noachis very faint.

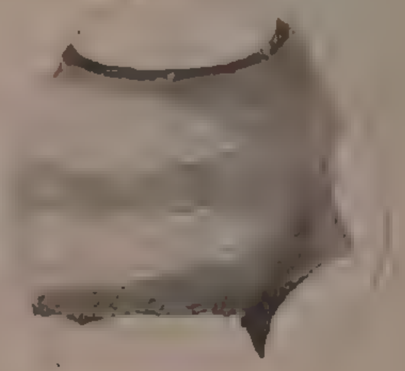


FIG. 2.—June 2, 16h 50m (S. M. T.) Longitude 276°; Power, 420. Lybia visible as a sort of snout. Having less sea next to the darkest marking on the disk. The Polar Sea the darkest.

FIG. 3.—June 3, 17h 40m (S. M. T.) Longitude 278°; Power, 320. The beginning of the Nilosyrtis visible as a pig-tail to the Hour-glass Sea. Strait between Polar Sea and Hour-glass Sea well seen.

FIG. 4.—June 7, 17h 45m–18h 15m (S. M. T.) Long. 244°; Power, 370. Star points on following end of snow cap. Preceding part dusky. Aeria on the limb, brightest part of the disk except the polar cap. Limb white utterly unlike land. Beginning of Lethes visible in center of disk. Cloven shape of Hellas shows where Peneas debouches.

* Longitudes are all from Marth. Details seen a few minutes before and after included in the drawings.

happy revelations played bo-peep with the eye, it was evident that their intermittent invisibility was not chargeable to Mars' atmosphere but to our own. A canal or a coast line would suddenly stand out perfectly distinct several hundred miles in length, and then as suddenly be waved, by our own air, out of existence. The northern coast of the Mare Sirenum came out in this manner on June 24th, and three canals, centering like the spokes of a cart-wheel about a lake for hub, probably the Araxes, the Eumenides and the Agathodæmon about the Lacus Phœnicis, did the same on June 23d

So much for the terrestrial conditions under which the observations were made. The Martian ones were such as to make the polar cap and its accompanying phenomena the centre of interest upon the planet.

In the matter of the compass points, I would say that I make use of the orientation proper to the planet, that is east is east to one on the surface of Mars. This is similar to our own terrestrial orientation. The south pole of Mars reached its maximum dip toward the Earth on June 22. It amounted then to 24 degrees, an inclination greater than it has shown us for fifteen years, or will show us for fifteen years to come. Its south polar regions were in consequence peculiarly well displayed, and this at the time when the snows there were in active process of melting.

At the beginning of the period included in these observations, the area covered by the snow-cap was still very large. On June 3 I made it 47 degrees across, that is, it covered nearly the whole frigid zone. During the month it decreased slowly in size. On June 19 it measured about 39 degrees. But I think the former of these determinations somewhat too large and the latter a trifle too small. Throughout all this time its outer edge looked to me to be perfectly elliptical, implying, that is, that it was in fact perfectly circular.

Girdling it was a narrow dark streak of nearly uniform width, as seen at any one observation, but which slowly narrowed as the longitudes grew less. The snow was continuously bordered by this belt, which was broadest between 320° and 220° of longitude. Here calculation showed it to be about 350 miles wide. Farther east, above the Mare Sirenum, it had contracted to the half of this. It was clearly water at the edge of the melting snow, a polar sea in short. And it is worth noting that it was widest where the dark markings, the seas, were greatest in extent. Between Hellas and the Chersonesus its surprising symmetry was broken by an expansion into a great gulf two or three times the

width of the rest of it. Under the best seeing the gulf showed a beautiful deep blue.

On the morning of June 8, at $1^h\ 17^m$ G. M. T., I observed suddenly a couple of extremely brilliant star-like points upon the snow-cap *n. t.* just above this great bay. They were very conspicuous, resembling exactly the starry dazzle from a distant surface, tilted at the proper angle for specular reflection. A few minutes later they had disappeared. Just such an effect would be produced by snow slopes suitably illumined turning through the glinting angle. We have here then, in all probability, evidence of differences of elevation in the snow-cap, of mountains there in short. Their position proved to have been in long. 291° 30′; lat. 75° 40′ south. This, together with the known direction of the Sun at the time, enables us, assuming specular reflection, to tell the tilt of their slopes. At that point upon the disk under the then illumination from the Sun, the tilt of a slope capable of sending specular reflection to the Earth would have been 54½°, and the slope must have faced 9¼° west of north. This is a surprising steepness, but fortunately absolute specular reflection is not needed to account for the phenomena. At so great a distance a surface tilted at a much lower angle if of high reflective power would produce a like dazzling effect, and for other reasons it is probable that the Martian slopes are not excessive. At the same time phenomena have been observed which seem to require some height in these hills, making them another of the puzzling points about this interesting planet. I shall resume discussion upon the snow cap in a later paper. Their position, it will be noticed, corresponds with the star-points seen by Green in 1877 and by Mitchel in 1845, only that they were then seen as distant snow islands, the polar cap having disintegrated further.

On the morning of June 10, at $0^h\ 10^m$ G. M. T., I saw similar star points appear in about the same relative position on the disk, less bright than the first ones and from the longitude then on the meridian, situated further to the east of them. I saw such again on the 11th, the 13th and the 14th, at hours which showed them to belong to a long range of heights on that side of the snow-cap. Sometimes they glittered like stars, sometimes they appeared simply as perfectly white spots.

Meanwhile on June 9, I had my first certain view of the great rift. It came out as a dark line roughly parallel to a parallel of latitude in the midst of the snow, about 15° from the lower edge

FIG. 5.

of the cap, and nearly half as long as the cap's diameter. After this I saw it under varying angles of rotation, as will appear from the drawings.

On the 13th it showed cornerwise on the following side of the cap, back of the great polar bay. The phenomena on this morning were particularly detailed. Above the bay the snow glittered with the bright points I have spoken of, and then back of them fell off shaded to the rift. The rift itself appeared in the form of a half cross or notch in the heart of the snow at the base of the hills.

FIG. 6.

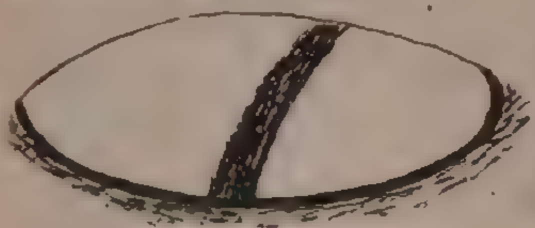


FIG. 7.

On the 14th I marked the rift debouch into the polar sea. At 17^h 30^m, mountain time, that is, 15d 0^h 30^m G. M. T., the entrance to it was on the meridian. The longitude of the rift's eastern end was therefore 160°. The rift was very broad, apparently equally so throughout, and looked like a huge cart-track coming down to one over the snow. On the 15th I estimated its breath at one-eighth of the cap's diameter. The same morning I also made a comparison of it with the scale of artificial canals. The values computed from the two determinations agreed perfectly, giving it a breadth of 220 miles.

On the 12th I noticed star-points on the preceding side of the cap. This was then to the east of the rift and indicated therefore, high land on that side of it, as well as on the other, tilted also to the west of north. Later the preceding end lost its lustre and the following end had once more become the brighter.

From these observations and others at this Observatory on

FIG. 8.—MAP OF MARTIAN SOUTH POLE.

The stars represent the brilliant points seen, indicating high land

the western portion of the rift, I have drawn a plan of the polar-cap and marked it for elevation, as given below. The length of the rift would therefore seem to be about 1200 miles.

Next to the snow-cap the most striking feature about the disc was the emphatic tripartite character of its markings. The surface of the planet was conspicuously divided off into three distinct portions, the continental area, the polar cap and the area of dark markings lying between the two. Though in no sense zones these corresponded in a general way with the equatorial, the south temperate and antarctic regions respectively. Of zones proper, there was, with the exception of the polar cap, no evidence whatever, which means of course that no cloud formations typical of them were visible; Mars being in this respect utterly unlike Jupiter or Saturn, where all that we see is cloud evidence of zone.

The three portions referred to may be recognized in any map of Mars it was their marked contrast that was striking. For next to the sharp manner in which the snow-cap stood out upon the disk was the definite character of the continental coast line and the equally indefinite character of all the markings between the two. The coast line was most salient and clear-cut on the western side of the Hour-glass Sea (Syrtis Major or Mer du Sablier).

To the eastward the coast lay in general direction straight, approaching the pole as it stretched eastward. It was indented by numerous bays but destitute of those comet-tail peninsulas so generally observed connecting it to the chain of islands south All of these islands Hellas, Ausonia and the rest, were vague, without definite contours and lapsed imperceptibly into the surrounding seas. Even in color they were less decided than, though of much the same tint as, the continental areas.

No connection appeared between the continent and the islands till it came to the eastern end of Mare Sirenum. Here Icaria and Phætontis made a bridge of singularly uniform width, preposterously straight and narrower than usually figured. Beyond this to the east, Thaumasia appeared, at times well defined, at other times not. Lack of contrast not lack of contour was responsible for this, as the glimpses of it showed.

The colors of these three portions of the planet's surface were as marked as their contours. In the best seeing the colors were exquisite. During the dark the polar cap was straw-color, the seas a limpid bluish green, and the lands a brilliant rose-orange With the sunrise the snow-cap turned to white, the seas to sky-blue and the land to a rosier red The sky-blue of the seas was almost exactly the same tint, only a trifle fainter, as the blue of the surrounding sky, a sky be it remembered, of over 7000 feet of altitude. The light of the Sun filtered through our atmosphere, added blue to all, thus wiping out the yellow A similar effect is visible in Venus and the Moon, which from pale yellow by night turn pale white by day.

It may be noted, in passing, that these before and after sunrise hues seem effectually to dispose of the suggestion that the seas owe their color to contrast. Were that so then when the changed light had made the lands less yellow, contrast should make the seas seem more yellow and not, as was the case, more blue.

For purposes of comparison with terrestrial objects, the post-sunrise tints are of course the ones to take. The resemblance in color therefore, between the so-called seas and water scarcely needs comment.

Next in point of general importance come the phenomena of the terminator and limb. The terminator differed alike from the terminator of the Moon and from the terminator of Venus, both of those bodies opportunely offering themselves for comparison in the course of the month The Martian terminator was more marked than the lunar one, less marked than that of Venus; that is the shading was greater in the one case, less in the other It

would thus seem to imply for Mars a depth of atmosphere between the two.

The softening of the light was evident the whole length of the lune that was lacking. Even the polar snows showed it, being distinctly shaded on their preceding portion, notably on May 31st and June 7th. The rest of the terminator was similarly darkened, the dark parts being darker and the light less light than the centre of the disk. Only one exception to this did I observe; on the 24th when a light band lay along the edge some twenty-five degrees wide over the sea area. This may have been caused by Argyre and Pyrrhae then upon that portion of the disk though not distinguishably visible.

I saw no irregularities upon the terminator although the terminator of Venus, with the same power, seemed not rigidly correct, either at the cusps or the fringe's inner boundary, and that of the Moon stood conspicuously notched to the naked eye. On the terminator of Mars the only projections were the bright areas which projected in a body, an effect palpably due to irradiation. It seems therefore, safe to say that few if any mountains relatively comparable to the lunar ones, exist on Mars. The elevations otherwise revealed are probably of no great height.

The aspect of the limb was more suggestive, if harder to explain. The limb was very luminous and yellow. This luminosity was not confined to the actual limb, but extended a long distance on to the disk. It was as if a wash of some lighter tint had been laid on around that side of the picture; at times it showed an inner edge of demarkation as abrupt as a wash of water-color. I saw it thus on June 15th. An estimation of its breadth relatively to the radius of the disk made it 29° in from the limb, or two hours in time. As the limb stood at nine o'clock of a Martian morning, this corresponded to 11 o'clock in the day. So abrupt an inner contour is doubtless an effect of contrast but it implies a pretty rapid falling off in the light.

With the exception of the polar cap and its surrounding polar sea, all detail near the limb was lost in this light, and to a distance in from the edge proportionate to the intrinsic contrast of the detail. The surpassing brilliancy of the snow-cap and the darkness of the water beneath it caused these to stand out up to the edge. The sea area below them sometimes showed to the limb, sometimes not; the dark patches nearer the equator, to my eye, never. Roughly speaking, the farther from the pole the point on the surface lay, the farther from the limb was it obliterated.

Reversely every bright portion of the planet was brighter when upon the limb. The most striking instances of this were the islands like Hellas. This when coming around the corner shone with a brilliancy almost rivalling the polar cap. Yet so soon as it was well in evidence it was content to sink almost out of sight during the rest of its journey across the disk. Such first putting in of an appearance and then lapsing into obscurity was a specialty of islands; but once I marked, for some singular reason, both the islands and the coast line—in this instance Electris and Zephyria—thus singled out for initial prominence. Whether islands and coasts are more covered by moist air is a question suggested by these phenomena.

Such inpartiality of obliteration shows, however, one thing: that the light along the limb is not due, as has been suggested for somewhat similar phenomena on the lunar limb, to reflection from mountain slopes or other irregularities of the surface. Nor does it seem attributable to cloud properly, so-called, since we can hardly suppose clouds to be so superior to latitude, or to be so idiosyncratic as to remain till 11 o'clock in the forenoon and no later. We are thus apparently forced to fall back on atmosphere pure and simple, with probably much aqueous vapor in suspension.

Collateral proof of some such homogeneous veil, rendered opaque only by its thickness, appeared on July 15th, when Hellas then on the limb, stood out embossed on the light surface preceding it. The light surface was in fact a dark strait, but the whole had been illuminated by the light of the limb.

We now come to what we have at present every reason to believe to be water, the dark patches. A part of them may be vegetation of which more later; but a part can hardly be anything but water and perhaps all may be.

These dark patches, or, as we may say provisionally, seas were of diverse degrees of darkness but each tint merged imperceptibly into the others. They looked to me to differ in degree, not in kind. The darkest of them was the polar sea with its great polar bay. In the best seeing this last appeared deep blue, its size enabling the color to come out. The next darkest was the Hour-glass Sea (Syrtis Major); then the Syrtis Minor and the Sabaeus Sinus. All these are small patches, irreverently suggesting puddles, or deeper spots in the general water waste. Next to them in tint was the strait between Hellas and Pyrrhae connecting the polar and Hour-glass seas.

To the eastward of the Syrtis Major the tint of the seas grew

lighter, and the same was true to the west. Indeed the Hourglass Sea seems to be the centre of the oceanic system of the planet. Not only does it lie at the apex of the largest water area on the planet; it is with the exception of the temporary polar sea, the darkest and therefore presumably the deepest body of water there; but it makes a sort of funnel to discharge these waters into the various canals through the Nilosyrtis, the largest of their number.

This brings us to the canals. The first one I saw was on June 7th, the Cerberus in all probability from its position. On June 9th it appeared persistently. I noted it as looking very broad. When on the meridian I glimpsed it double. I had only one glimpse of it as such and though this was definite enough, I put no very great faith in it. I mention its duplicity for future verification. On the 9th and more certainly on the 10th, another canal showed debouching into the same bay but turning more to the right, as it went northward, the Galaxias perhaps. I next thought I saw in the very middle of the continental area, a part of the Orcus parallel roughly to a parallel of latitude. I never familiarized myself beforehand with the region about to be seen. During the next week I made out Gigas, Titan, Gorgon, Sirenius and Eumenides. The darkest of these was the Eumenides, though it was the least well placed for observation. Up to this time the canals were of great difficulty, and were neither sharp nor phenomenally straight; fugitive glimpses, many of which flitted before me but of which I have recorded only the more certain ones. But when the region of the Lake of the Sun began to come round the canals in its neighborhood came out in much better shape: Phasis, Eumenides and Agathodæmon. Detailed explanation of the drawings will give a better idea than I can do in the text of the observations.

The general outcome of these early observations is suggestive. For the phenomena seem to point to a vast vernal freshet, now in process upon the planet. In the first place we see the great polar snow-cap vanishing under our eyes. Now this might take place either by evaporation or by melting. Doubtless it does take place in both ways, but what is of interest is that we have direct evidence of the latter process. Instead of simply growing beautifully less the snow-cap appeared persistently fringed by a dark line which steadily kept pace with its retreat. This can hardly be anything but water although it may be water in a very different condition from what we know it, neither fresh nor salt, for example, but something unlike either. The character of the wa-

ter is to certain extent a vital question, as irrigation with salt water does not commend itself to our notions.

Now so much water suddenly produced has got to be disposed of. For it is in a state of unstable equilibrium. So long as it was snow it stayed where it was. But the moment it melted gravity would pull it from the pole. Here step in the next observed phenomena. The bluish cast of the south temperate belt and the indistinctness of the contours of the islands there hint that this whole region is undergoing submersion and submersion of a sudden and temporary character. For in the first place we know that at other seasons these islands are both more conspicuous in color and more distinct in form, and that the comet-tail peninsulas now invisible appear with all grades of distinctness. In the second place if this submersion were not of a temporary character the contours of the red portions would be more definite than they are. For a marsh does not remain a marsh for long time; it becomes either land or water, by reason of the forces at work upon it.

Thirdly, the canals of the continent are well known to be less visible at this season of the Martian southern hemisphere. This is precisely what should happen if they are canals, especially if what we see are the effects of the water in them rather than the water itself. For it would take time for the effects of a flood, in the way of vegetation for example, to show themselves. Now it is rather significant in this connection, that the canals now best seen should be those nearest the south pole, namely, the ones about Thaumasia and Mare Sirenum.

So much for the positive evidence of the circulation of water as water upon the planet. The evidence of its circulation as cloud is negative. There is no sign of cloud apparently at this season. Although the air is probably charged with vapor, it does not condense into cloud or fall as rain or snow. Even occasional indistinctness of detail seems, as we have seen earlier in this paper, to be due to our atmosphere, not to that of Mars. At this spring season of the southern hemisphere the Martian aqueous circulation seems therefore to be chiefly a surface one of water.

Here we have a *raison d'etre* for the canals. In the absence of spring rains a system of irrigation seems an absolute necessity for Mars if the planet is to support any life upon its great continental areas.

It is specially important that phenomena like these spring phenomena should be reasoned on. For it is chiefly by collation of inference that we are likely to build up a true knowledge of

MARS.

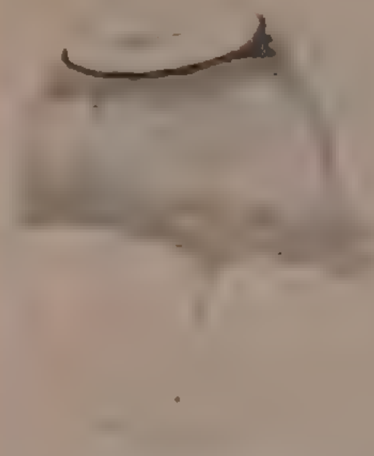


FIG. 9.—June 9. $17^h 20^m$ (S. M. T.) Long. 211°, Power, 370. Rift in snow-cap. Following part of snow-cap peculiarly brilliant. Cyclops Cerberus (together) and Galaxias in center of disk. The former ends in a black dot, possibly the Trivium Charontis. Great Polar Bay visible on following edge of snow-cap.

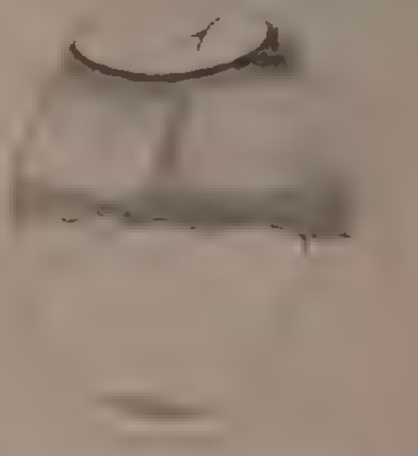


FIG. 10.—June 13. $18^h 10^m$ (S. M. T.) Long. 194°. Power 370. Rift well seen. Star points following it. South east of them, the snow-cap has sleuded to the rift. Cyclops and Cerberus on the right of disk. Suspected Trivium Charontis at bottom. Polar Bay well seen.

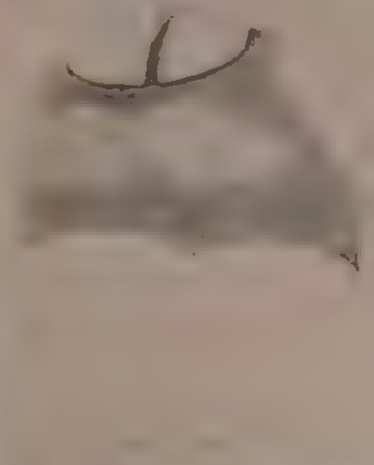


FIG. 11.—June 15. $17^h 30^m$–18^h (S. M. T.) Long. 161°. Power, 640. Rift on the meridian.

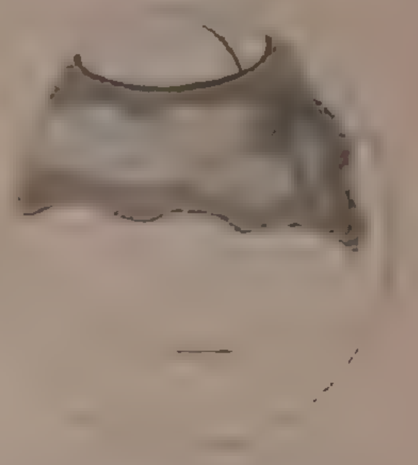


FIG. 12.—June 16. $17^h 20^m$ (S. M. T.) Long. 145°. Power, 370. Rift on following end of cap. Oasis at the Nodus Gordii in the center towards the bottom of the disk.

MARS.

Fig. 13.—June 17. 18h 38m (S. M. T.) Long. 155°. Power, 370. Titan, Gigas and Gorgon.

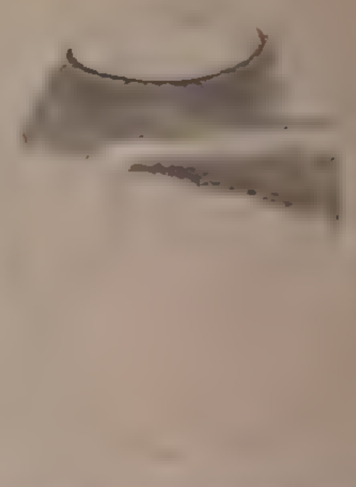


Fig. 14.—June 19. 17h 45m (S. M. T.) Long. 122°. Power, 370. Rift again Gigas and Sirenius. Icaria and Phaëtontis strangely straight.

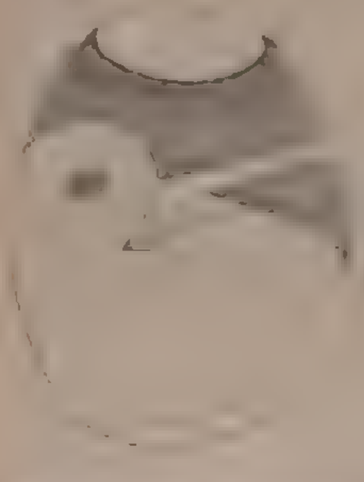


Fig. 15.—June 20. 16h 40m (S. M. T.) Long. 96°. Power, 370. Lake of the Sun, Phasis, Araxes Eumenides. Araxes, the boundary of a shaded area.

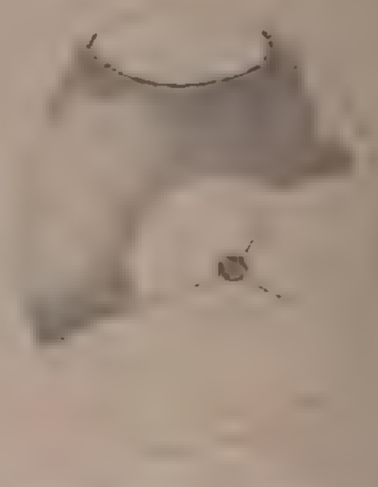


Fig. 16.—June 23. 18h 20m (S. M. T.) Long. 91°. Power, 370. Probably the Lucus Phœnicis with Agathodæmon, Araxes and Pyriphlegethon.

this our neighbor world. We shall best explain such enigmas as the canals by getting greater insight into the general physical conditions existing upon the planet. It is by no means inconceivable that we may first succeed in unravelling that net-work of seemingly incomprehensible lines by inevitable inference from comparatively simple data.

LOWELL OBSERVATORY, July, 1894

THE SEAS OF MARS.*

WILLIAM H PICKERING

The first observation made upon Mars at the Lowell Observatory with the 18-inch Brashear lens was upon June 1, 1894. Since then observations have been continued upon nearly every night. What appears to me be the most important conclusion deducible from our work so far is that Mars does not always present the same appearance at the corresponding time upon two successive Arean years. This remark does not apply merely to small details but to large and prominent features. Moreover this difference does not seem to be due simply to the fact that one season is a few weeks later than the other, but that the phenomena presented upon the two years are really different.

Thus the central branch of the Y, just north of *Noachis* which was so marked a phenomenon in 1892, was not visible to me early in June, as I had expected it to be. It is true that Mr. Lowell thought he saw it faintly marked, but although I looked for it upon the same evening, I could not satisfy myself of its existence. Nevertheless the definition was such that had it appeared as it did in 1892, it could not have been missed at the first glance. I looked for it again at the following presentation in July just passed, but no trace of it was to be seen. Two drawings made by Professor Campbell upon July 18 and 20, 1892, and published in the last number of the Publications of the *Astron. Soc. of the Pacific*, p. 171, show it very nicely indeed. These may be compared with some reproductions of my own work now originally published in ASTRONOMY AND ASTRO-PHYSICS 1892, p. 668, and now republished in the same number with the drawings of Professor Campbell. After the disappearance of the central branch in the latter part of July, 1892, a portion of it reappeared in August, and remained visible through September. A sketch

* Communicated by the author.

showing its appearance upon September 4, 1892, has been kindly forwarded to me by Mr. Russell of the Sydney Observatory, N. S. W. This branch may therefore be said to have been characteristic of the opposition of 1892. This same region was very carefully sketched by Mr. Douglass and myself a number of times between June 30 and July 6, 1894, but not a trace of the central branch could we detect. Upon these dates Mars held the same position in its orbit that it did upon August 12 and 18, 1892. A sketch made by myself August 13, 1892, shows the central branch very clearly. It will be interesting to hear if its appearance has been noted this year by the Australian observers, since in their longitude it would have been visible about the middle of June.

But not only has the central branch of the Y been invisible this year, but the large dark blue patch which it connected with the southern snow cap, and which we called the Northern Sea, has been very much less marked, and much smaller than was the case in 1892.

Again a large black gulf bounding the melting snow upon the north and situated very nearly due south of *Syrtis Minor* has been a very striking feature of our observations this year. This gulf was only observed once in 1892, upon July 27, and it was then by no means conspicuous. If these very dark regions are, as we suppose them to be, water, it would then seem that the water which did not reach the northern regions this year has appeared as an excess in the south.

Upon testing this black region upon June 4, with an Arago polariscope, made for me by Mr. Brashear, it was found to show clear traces of polarization, as did the canal running north from it. This would naturally be the case if it were water, since being situated near the limb, it would reflect to us largely the light of the Arean atmosphere. Upon the rest of the disc of the planet, the polarization was not very conspicuous. At the next presentation of this region, upon July 9, the observation was repeated but to my surprise no trace of the polarization in the dark spot could be detected. A close examination of the region was then made, and its color was found to have entirely changed,—whereas upon June 9, Mr. Lowell writes "Bay a deep blue, looks just as deep water does," it was now found to be of a rich chocolate brown tint, differing entirely in color from the bluish grey regions to the north of it. These grey regions showed no sign of polarization, and as I have before remarked I see no reason for supposing that their color is due to water. As far as my observations go, it appears to me that the permanent water area upon Mars, if it exists at all, is extremely limited in its dimensions.

These large grey regions were of a brilliant and decided green color in 1890, just before the vernal equinox. In the early part of 1892, also, large green areas were seen upon the planet, but as the season advanced the green regions changed almost entirely to grey. At the present time very little color is visible in the shaded regions. They are subject also to such large variations in area, as the season progresses, that unless we can persuade ourselves that gigantic floods, unaccompanied by clouds, form the normal condition of affairs upon Mars, we seem forced to adopt some other explanation of their existence. The theory that they owe their color to vegetation is perhaps the most plausible one, and some new facts bearing upon this matter have recently come to hand. Upon June 30 a distinct depression in the terminator where it was crossed by the stem of the Y was detected by Mr. Douglass. As the planet rotated, the position of the depression changed, and it was noted that it was not always found in portions of the terminator which was darkest. Since that date similar depressions more or less marked have been detected upon nearly every evening. Upon looking over our observations for 1892, I find under date of September 20, 8^h 06^m a drawing showing a flattened terminator, and a statement that "the planet seems somewhat of this shape." Further investigation shows that the long narrow strip known as *Ceraunius* was lying upon the terminator at about this time. These notches in the terminator can be most readily explained by actual depressions in the surface of the the planet, and as Professor Campbell has shown (Pub. Astro. Soc. Pac. 1894, p 110) a difference of elevation of the surface amounting to two miles ought to be readily visible to us on the Earth at certain seasons provided the elevation or depression involved occurred upon the terminator. It thus appears that we are perhaps on the eve of being able to construct a contour map of the planet. The observations involved are however very difficult, and no great accuracy in the results can as yet be expected.

Strictly speaking the notches in the terminator correspond to variations in the inclination of the surface of the planet rather than to variations in its level, but if we could determine the inclination and knew the distance through which it extended, we should have all the data required for our work.

There is one conclusion however to which these observations lead us at once. Since these notches in the terminator do not necessarily occur in the darkest parts of the grey regions, and since different portions of them are notched to different depths

when on the terminator, it follows that all portions of the grey regions are not on the same level. In other words hills and valleys occur in them, and consequently the grey regions do not represent the surface of an ocean.

LOWELL OBSERVATORY, Flagstaff, Arizona.
July 13, 1894.

ON THE PERIODIC TIME AND DISTANCE OF THE FIFTH SATELLITE OF JUPITER.*

E. E. BARNARD.

This satellite has been observed at every favorable opportunity during the opposition of 1893. On that occasion it was first seen September 3d and was last observed 1894, January 28.

These observations, combined with those of 1892, now give a very accurate determination of the periodic time of the satellite.

The following values of the period have been obtained by comparison with the east elongation of 1892, September 10*d* 12^h $48^m.2$ Standard Pacific Time.

STANDARD PACIFIC TIME.

		East Elongation.		
1893 Oct.	1	$16^h\ 11^m.84$	Period	$11^h\ 57^m\ 22^s.615$
	2	16 5 .61		22 .554
Nov.	6	12 54 .54		22 .650
	12	12 20 .88		22 .607
	19	11 42 .98		22 .662

From these we have

PERIODIC TIME = $11^h\ 57^m\ 22^s.618 \pm 0^s.013$

The recent observations seem to confirm the suspected excentricity of the orbit.

The mean of the observed east elongations of 1892 from September to November was

$$48''.104 = 112650 \text{ miles}$$

while the mean of those for 1893-4 from Sept. to January is

$$47''.785 = 111910 \text{ miles}$$

These are reduced to distance 5.20.

This last fairly corresponds to the West elongations of 1892,

* Communicated by the author.

viz. 47″.712. M. Tisserand has found that the major axis of its orbit must make a complete revolution in five months.

No opportunity has yet occurred for a direct comparison of the light of the satellite with a star, so that its magnitude is still somewhat uncertain. It is scarcely possible that the brightness is greater than 13 magnitude—if it is as bright as that. So the assumed value of about 100 miles for its diameter should still be considered as not far from its actual size.

No eclipses of the satellite have been observed, and it is scarcely possible that they ever will be with our present instrumental equipment, as they will occur too close to the limb of Jupiter for the satellite to be seen.

Its shadow has not yet been seen on the surface of Jupiter and doubtless does not reach the planet unless as a point too small to be seen with our present telescopes. During the observations of this object in 1892 and 1893 a series of filar micrometer measures of the diameters of Jupiter were made for the homogeneous reduction of the observations.

From these the following values result They are also reduced to distance 5.20

Equatorial Diameter	38″.522 ± 0″.024 (34 nights)
Polar Diameter	36 .112 ± 0 .032 (24 nights)

which correspond respectively to

90194 ± 56 miles

and 84566 ± 75 miles

In comparing these with previous measures, it was found that filar micrometer measures of the diameters of Jupiter are uniformly about 1″ greater than those made with heliometers. The complete observations of the satellite, which were all made with the 36-in., will be published in the *Astronomical Journal.*

MT. HAMILTON, July 7, 1894.

PRELIMINARY NOTE ON THE OBSERVATIONS OF SATURN AND URANUS WITH THE 36-in. EQUATORIAL.*

E. E. BARNARD

SATURN.

Since the first of the year I have been engaged in the work of re-measuring the ball and ring system of Saturn with the 36-

* Communicated by the author

inch, a work especially suited for our great telescope. Among the other points in view in this work was a series of measures made with the intention of detecting any displacement of the ball from the center of the ring.

The measures were made from the ends of the ring to the nearest limb of the planet.

The results so far obtained from ten nights' measures, show a slight difference that would make the distance on the following side a little greater than on the preceding side This may be due to some peculiarity in the measures themselves as the quantity is less than 0".1.

The values reduced to the mean distance of Saturn are:

From *p* end of Ring to *p* limb of Saturn.	From *f* end of Ring to *f* limb of Saturn.
11".197	11".287

From ten nights' measures.

The difference, 0".090, would scarcely suggest any apparent deviation of the ball from the exact center of the rings

The dimensions of all the rings and the ball are also being carefully measured. A preliminary reduction of the work shows the results will agree very closely with those obtained by Hall with the 26-inch at Washington some 10 or 15 years ago. (See "Saturn and its Ring," Washington Observations, 1885, Appendix II.)

URANUS.

Besides a careful series of measures of the positions of the satellites of Uranus for a more accurate re-determination of the mass of that planet, I have also made a series of measures of its polar and equatorial diameters, and a series of measures of the position angle of its equator. The ellipticity of the disc is quite noticeable.

The following are the position angles so far obtained

		P. A. of Equator.
1894 April	29	11.3
"	30	16.8
May	6	19.5
"	7	12.3
"	21	7.0
"	28	7.1

From these it would appear that the equator of the planet essentially coincides with the planes of the orbits of the satellites, verifying the supposition that Uranus rotates on an axis deviating but little from the plane of its orbit.

The observations of the satellites extend from April 16.

The complete observations of both these planets will be published when the observations are finished.

MT. HAMILTON, June 9, 1894.

ORBIT OF THE BINARY STAR $O\Sigma$ 224.*

J. E. GORE.

Although the angular motion of this binary pair has been only about 60° between the years 1843 and 1892, I find that the motion has been round the apoastron end of the apparent ellipse, and that consequently the period of revolution will be shorter than the angular motion would at first sight seem to indicate, and shorter than that found by Professor Glasenapp in ASTRONOMY AND ASTRO-PHYSICS, No. 118. Using the measures given by Mr. Burnham in ASTRONOMY AND ASTRO-PHYSICS, No. 108, I have drawn the apparent ellipse from an interpolating curve, and from this ellipse, I obtained, by Professor Glasenapp's method, the following values of the coefficients in the general equation of the second degree.

$$\alpha x + \beta y + \gamma x^2 + \delta xy + \epsilon y^2 + 1 = 0.$$

$\alpha = +0.0136$	$\delta = -0.0001501$
$\beta = -0.02624$	$\epsilon = -0.000526$
$\gamma = 0.0002174$	

Substituting these values in Kowelsky's equations, I have obtained the following provisional elements for $O\Sigma$ 224.

$P = 96.13$ years.	$\lambda = 316^\circ\ 3'$
$T = 1833.25$	$i = 39^\circ\ 46'$
$e = 0.579$	$a = 0''.42$
$\omega = 179^\circ\ 14'$	$n = -3^\circ.7447$

Measured in the direction of the star's motion, which is retrograde, λ would be 43° 57'.

The following is a comparison between the measures used in calculating the orbit and the positions computed from the above elements. Some of the measures are rather discordant.

* Communicated by the author.

Epoch.	Observer.	θ_o	θ_c	$\theta_o - \theta_c$	ρ_o	ρ_c	$\rho_o - \rho_c$
		°	°	°	"	"	"
1843.22	Mädler	13.7	20.3	− 6.6	0.35	0.30	+ 0.05
1844.31	O. Struve	20 ±	14.3	+ 5.7	——	0.33	——
1845.30	Mädler	13.6	13.3	+ 0.3	0.20	0.35	− 0.15
1851.27	O. Struve	352.6	361.0	− 8.4	0.48	0.46	+ 0.02
1851.28	Mädler	17.5	1.0	+ 16.5	0.25	0.46	− 0.21
1857.34	Secchi	3.6	352.6	+ 11.0	——	0.53	——
1861.26	O. Struve	348.8	348.1	+ 0.7	0.59	0.56	+ 0.03
1868.03	Dembowski	339.2	341.0	− 1.8	0.5 ±	0.59	− 0.09
1871.31	O. Struve	328.4	337.6	− 9.2	0.59	0.59	0.00
1872.31	" "	336.8	336.7	+ 0.1	0.55	0.59	− 0.04
1873.23	Dembowski	329.8	335.8	− 6.0	——	0.59	——
1879.32	Schiaparelli	315.7	329.5	− 13.8	0.35	0.57	− 0.22
1880.16	Burnham	334.3	328.6	+ 5.7	0.62	0.57	+ 0.05
1881.25	Doberck	316.6	327.4	− 10.8	——	0.56	——
1882.27	"	309.9	326.3	− 16.4	——	0.56	——
1883.71	Engelmann	330.2	324.6	+ 5.6	0.53	0.55	− 0.02
1884.21	Perrotin	326.0	324.1	+ 1.9	0.55	0.55	0.00
1887.27	Schiaparelli	315.6	320.4	− 4.8	0.52	0.53	− 0.01
1892.37	Burnham	313.6	313.4	+ 0.2	0.48	0.49	− 0.01

The following are the formulæ for computing the position-angle and distance at any given time t:

(1). $u - 33.17 \sin u = - 3.7447(t - 1833.25)$

(2). $\tan \frac{1}{2}V = [0.2870500] \tan \frac{1}{2}u$

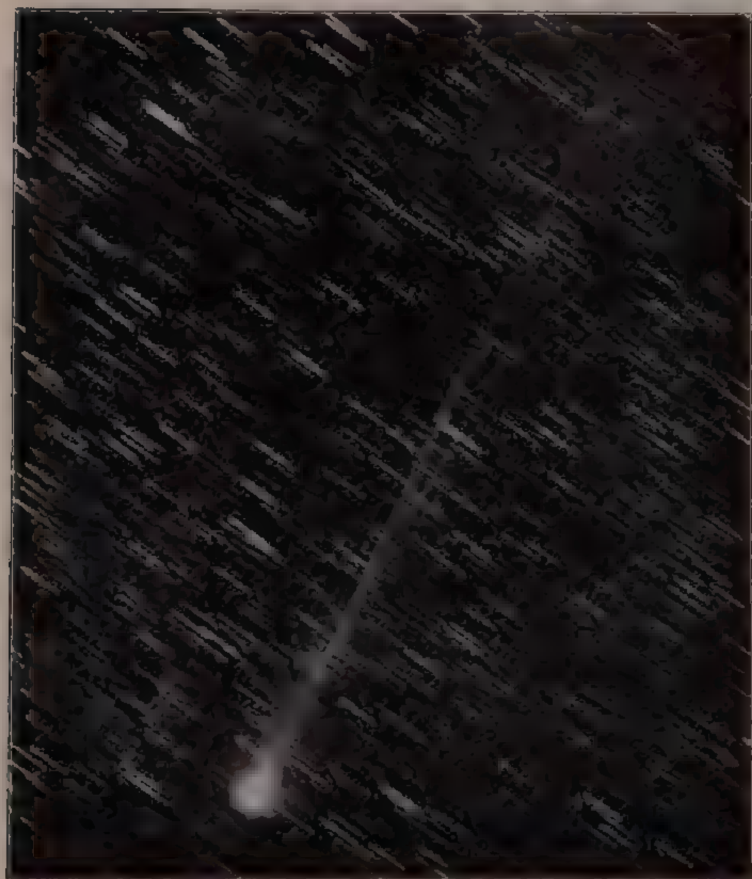
(3). $\tan(\theta_c - 179° 14') = [9.8101666] \tan(V + 316° 3')$

(4). $\rho = 0''.42(1 - 0.579 \cos u) \dfrac{\cos (V + 316° 3')}{\cos (\theta_c - 179° 14')}$

The figures in brackets are logarithms.

[redacted]

PLATE XVIII.



GALE'S COMET,

1894, MAY 5, $8^h 45^m$ — $11^h 15^m$ S. P. T.

BY E. E. BARNARD

(See article in June number.)

Astro-Physics.

ON THE SPECTRUM OF β LYRÆ *

H. C. VOGEL.

In conclusion I give a brief review of the more recent observations which have been made elsewhere.

From the short notice which Pickering published in *Astr. Nach.* 3051, it appears that the twenty-five plates of the spectrum of β Lyræ, which were taken in the course of more than four years, agree as to the most important points with the observations here given. He distinguishes, as we also were led to do, between the appearance of the lines in the first and that in the second half of the light period from one principal minimum to another. He says:

"Of the eleven plates in which the bright lines had a diminished wave-length, it was found that all had been taken during the second half of the period of variation, that is, after the second minimum and more than 6*d* 11ʰ after the principal minimum. The fourteen plates taken during the first half of the period all showed an increase in wave-length of the bright lines; that is, the dark lines appear bright on the side toward the red." Pickering further says: "The actual changes in the spectra when studied in detail are much more complicated than has been stated above, and show a variety of intermediate phases, and changes in the dark as in the bright lines. In some of the photographs several of the bright lines appear to be double."

Under the assumption that the object is a close double star, whose components possess different spectra and whose period of revolution equals the light period of 12.9 days, Pickering deduces from the observed displacement of the lines a velocity of 480 kilometres per second, and calculates the radius of the orbit, which is assumed circular, to be eighty million kilometres. The determination of the velocity was made, without doubt, from the measurements, taken at the time of a principal minimum or first maximum, of the bright and dark lines which lie apparently near each other. As a result of the use of the object-glass prism, and the consequently diminished sharpness of the spectra as compared with photographs taken with the slit spectroscope, the distance observed by Pickering is considerably greater than that

* Continued from page 367.

found here. If the calculation is carried further, it leads to an enormous mass (130 ☉) for the system. Also, if the more probable assumption is made, that the centre of mass of the system falls, not at the center of one star, but between the two, it follows that the mass of the system is 16 ☉, which is still extremely improbable. But just as I have shown above that it is not allowable, without further information, to regard the distance between the centres of the bright and dark lines as the amount of the displacement, so it is not consistent with the observed phenomena to assume that the orbit is circular.

Pickering's important notice concerning the double spectrum of β Lyræ, which up to that time had been unknown, appeared in June, 1891, and it closes with the following remarks as to further hypotheses, which, however, I cannot support, because they seem to offer no sufficient explanation of the complicated phenomena in question:

"The phenomena may also be due to a meteor stream or to an object like our Sun, revolving in 12*d* 22[h] and having a large protuberance extending over more than 180° in longitude. The occasional doubling of the lines would then be due to both ends of the protuberance being visible at the same time, one receding, the other approaching. The variation in light may be caused by the visibility of a larger or smaller portion of the protuberance."

Observations were made by J. E. Keeler* at the Lick Observatory during the year 1889. They were direct observations with the spectroscope, and refer to the visible spectrum between *C* and *F*. Keeler's results, in his own opinion, were not sufficient to give a comprehensive and at all satisfactory explanation of the observed phenomena; and on this account he intended to make further observations, which however was not done owing to his withdrawal from the Observatory. His preliminary results were published only because in his opinion it was improbable that they would ever be continued, as in the mean time it had become apparent that direct observations could not in general compare with those based upon the newer photographic methods even when the latter are made with telescopes of inferior power. The results are in my opinion not without importance, although they differ in many points from the deductions made from the photographic observations in other parts of the spectrum; and I quote them in Keeler's own words:

1. In the spectrum of β Lyræ the bright hydrogen lines C and F, the bright D_3 line, and the dark D lines are always visible with

* A. AND A. P., April, 1893, p. 114.

a telescope as large as the Lick refractor. Certain fainter bright lines are visible except at the time of a principal minimum.

2 The variations in the light of the star are principally due to changes in the brightness of the continuous spectrum.

3 The bright lines are brightest when the continuous spectrum is brightest. This is the case in most of the observations. Certain exceptions may possibly be real, in which case they would indicate either irregular variations of brightness, or a variation having a period different from that of the star, or they may be due to errors of estimation arising from the diminished brightness of the continuous spectrum at the time of a principal minimum.

4. The bright lines are broad and diffuse, particularly when the star is at a maximum. The D lines are very hazy, so that the components are hardly distinguishable.

5. During the greater part of the period of the star no remarkable changes occur in the appearance of the spectrum. The observations fail to show any connection between changes in the spectrum and the secondary minimum of the star

6. The most remarkable changes take place at the time of a principal minimum. The bright lines become dimmer, and perhaps sharper. The fainter bright lines disappear. The D lines become darker Strong absorption lines appear on the more refrangible side of certain bright lines in the green, the separation of the dark and bright lines being at least five tenth-metres Other bright lines are perhaps similarly affected. A narrow dark line appears above the D_3 line at the same time Shortly before the first maximum is reached the dark lines disappear."

The most complete observations on β Lyræ, which have been made up to the this time are those of Belopolsky.* They are of especial importance because the instrument used was the 30-in. Pulkowa refractor. Twenty-five spectrograms were obtained between Aug. 24 and Nov. 26, 1892. On account of the achromatising of the object glass for visual rays, Edward's orthochromatic plates were used, and the photograms are of the portion of the spectrum between D and $H\gamma$.

Belopolsky has made a special study of the F line ($H\beta$), the D_3 line, and the group at λ $447\mu\mu$ and $448\mu\mu$, and has given besides a table of all the lines which could be recognized in that portion of the spectrum lying between D and $H\gamma$. Accompanying the

* *Astr Nachr.* 3129, *Memorie della Societa degli Spettroscopisti Italiani* vol. XXII p 101. Another article in which the observations are published in detail has appeared while this paper was in press, in Mélange's *Math et Astr*, t VII, line 3

memoir are seven drawings of the spectrum as it appears at different phases of the light period. These are of interest especially in reference to the changes of the bright and dark F line, as they are in complete accord with the changes observed here in the $H\zeta$ line, which lies far in the violet. The description which Belopolsky gives of the changes in the F line agree very well with the observations [of the same line] made here at certain times. A translation of this description is as follows:

"At the time of the principal minimum, the bright line is single and lies on one side of a dark line. (At a considerable distance, quite by itself, there can be seen a second dark line.) At the time of the succeeding maximum, the bright line begins to become double, the component on the violet side being very narrow. During the second minimum, the bright line becomes double, and nearly symmetrical. During the second maximum, the appearance changes but slightly, the component on the red side becoming, however, narrower than the other. After this maximum* there appears on the outer edge of that component which lies towards the red a dark line."

This last statement is remarkable, because in the observations made here in case of the $H\beta$ line and $H\zeta$ line as well, there is noted the appearance of a dark line on the red side of the bright line at at the time of the second maximum.

From the drawing of Oct. 7 we get further confirmation of the observations which were made here, in that *after* the second maximum the component of the bright line towards the smaller wave-lengths is decidedly broader than *at* the time of the second maximum according to the drawing of Oct. 2; and that 1.3 days *before* the principal minimum (Oct. 8.6) the appearance of the line is entirely different from that *at* the time of the minimum.

On the Pulkowa photograms, a comparison spectrum, hydrogen, iron or sodium, is photographed simultaneously with the spectrum of the star; and in this way it is possible to determine from fifteen plates the motions of the bright *F* line during the light-period of 12.9 days. The results are in surprising agreement with the period; and I give here the measurements themselves, to which I have added for comparison the times of the chief divisions of the phases of the light-variation.

* Belopolsky says, p. 104: "*Après ce maximum.*" Yet on p. 107 this dark line is farther described, and we are told that it was seen on Sept. 8 and Nov. 25, 1892, that is one day before the second maximum, corresponding to the observations made here.

Time of Observation [illegible]	Phase	Velocity Relative to Sun Calculated from the Displacement of the bright F-line	Velocity Relative to Sun Calculated from the Displacement of the dark F-line	Difference $b - d$
Sept. 23.3	22.5 II Max.	− 83 kil.	− 36 kil.	− 47
24.4		− 86	− 73	− 13
25.4	25.7 I Min.	− 32	——	——
27.3	28.9 I Max.	+ 36	− 248	+ 178
30.3		+ 80	− 13	+ 118
Oct. 2.3	2.1 II Min.	+ 13	− 36	+ 49
3.3	5.4 II Max.	− 26	− 36	+ 10
7.3	8.6 I Min.	− 69	− 47	− 22
11.3	11.8 I Max.	+ 76	− 63	+ 139
19.3	18.3 II Max.	− 91	− 44	− 47
20.3		− 76	− 97	+ 21
22.3	21.5 I Min.	*	− 93	——
26.3	24.8 I Max.	+ 79	− 66	+ 145
Nov. 25.2		− 53	− 47	− 6
26.2	26.1 II Max.	− 79	− 77	− 2

A positive motion in this table means a displacement towards the red. Belopolsky next deduces from his observations that at the times of the principal (or first) minimum and the second minimum that component of the velocity of the bright line, which lies in the line of sight = 0, while at the time of the first maximum it is nearly + 82, and at the time of the second maximum it is − 82 kilometres per sec. The displacement of the dark lines with reference to the artificial hydrogen F line was also determined; and I have included these measurements in the above table, placing in the last column the exponent of the relative motion of the two lines in the sense of bright line − dark line.

Unfortunately I am not in a condition to directly verify these measurements, because, as I have already mentioned we have not yet succeeded in satisfactorily carrying out measurements of the displacement of the lines in the stellar spectrum with reference to the lines in the spectrum of an artificial source. It is however noteworthy that all the values for the displacement of the dark line are negative, although there must be assumed some connection between it and the bright line; while for the bright line, variations are found towards the positive and negative side, which so balance each other that there is no evidence any motion of the hypothetical system itself.

We must note that unfortunately the observations are incomplete for the times of the principal mininum. Once, on Sept. 25, the displacement of the dark line is not measured; a second time, on Oct. 22, according to Belopolsky's description, the displacement of the bright line could not be measured, because its edge was covered (?) by the dark line. It is apparent from the description of the Pulkowa spectrograms as well as from the draw-

* "*Un bord est couvert par la raie sombre, impossible de mesurer.*"

ings made from the observations taken there, that at the time of the principal minimum and also the first maximum the distance between the bright and dark lines is the greatest. Accordingly we would expect to find for the bright line on Oct. 22 a strong positive motion of perhaps 75 kilometres, or at least as great as at the time of the next day of observations which falls after the first maximum, and therefore the regularity would be greatly disturbed, with which the observed points lie on the curve for the displacement,—a curve which is based on the exceedingly improbable assumption of a nearly circular orbit.

The measurements were moreover difficult, even with the powerful resources of the Pulkowa Observatory; so that considerable variations need not cause surprise. Besides I must once more mention the fact that the measurements cannot be regarded as entirely free from objection, since, on account of the partial superposition of the bright line on the absorption line, we should expect to find that the positions of the lines are reciprocally influenced.

In reference to the drawings, which were evidently made with great care, it is surprising that in the first three the bright and dark F lines in the stellar spectrum should appear in relation to the artificial line differently from the way they ought to according to the measurements.

Belopolsky believes that he finds an explanation of the phenomena in the assumption that a body whose spectrum gives the bright lines produces a partial eclipse of another body by passing over it at the time of the minimum. One ground for this assumption is that the continuous spectrum becomes weak at the time of the principal minimum, while the bright *F* line shows no corresponding decrease in intensity. From the observed maximum velocity of the motion of the bright lines of 89 kilometres and the period of 12.9 days, he calculates the radius of the orbit (assumed to be circular) to be in round numbers fifteen million kilometres; the mass of the system is then equal to that of the Sun.

In my opinion the time has not yet come for an exhaustive explanation of these very complicated phenomena, because I do not regard even the great mass of observations here given as sufficient for this purpose. Yet I do not question the fact that the important features of the variations in the lines can be shown to depend upon the motion of neighboring celestial bodies. It can, however, be settled only by further observations, whether or not the assumption of two bodies will suffice for an explanation of the fluctuations in the light, and the alterations in the spectrum so far as they refer to the lines.

As the existence of very close double-stars has been conclusively proved by means of the spectroscope, the peculiar changes in the light of β Lyræ can be explained by the close passage of two celestial bodies, one of which emits less light than the other, on the assumption of a nearly circular orbit, or an elliptical one whose major axis coincides with the line of sight and whose periastron is towards the Sun. When the dimmer of the two passes in front of the brighter and partially covers it, we have the principal minimum; the two equal maxima occur when the line joining the centres of the two bodies is perpendicular to the line of sight; the second minimum follows when the bright body partially conceals the dimmer one. On the other hand, the relative displacements of the lines can be explained by the revolution of two bodies, one emitting a bright line spectrum the other having an absorption spectrum, provided that the orbit deviates widely from a circle, and the major axis of the orbit makes a considerable angle with the line of sight. Both phenomena cannot be brought successfully under one assumption.

Returning again to Belopolsky's conception, I wish to call attention to the fact that if one of the bodies gives a bright-line spectrum, it must be assumed that the spectrum with the dark lines belongs to that body which is eclipsed at the time of the minimum. In this case, at the times of the principal and subsidiary minima, the components in the line of sight of the motions of both stars must vanish; the centres of the bright and dark lines must coincide. At the times of the the maxima, the component of the motion in the line of sight must have its greatest value, dark and bright must be relatively displaced, and in opposite directions at the two maxima. All this, however, is directly contrary to observation.

Father Sidgreaves has recently published* his observations on the spectrum of β Lyrae. They were made with a spectroscope without a slit, in connection with an 8-inch refractor. Forty-five plates in all were taken, ten in the spring of 1892, the rest in May and August, 1893. The time of exposure is not definitely stated, yet it follows from the description that it must have been long; and that most of the poor plates were the result of under exposure. No essentially new points of view are gained from these observations. They are confined mainly to the changes in the $H\gamma$ line and the lines at $439\mu\mu$ and $447\mu\mu$, since other portions of the spectrum were to diffuse or too weak to permit a study of the details. This much, though, can be noted, that on the plate

* *Monthly Notices* Vol. LIV, p. 96.

which accompanies his paper and in which are given the changes in the lines from day to day throughout the light period, the Hβ line, lying at one limit, is always represented as bright, the Hδ line, lying at the other limit, is always represented as dark. I might observe further that our observations here show that it is hardly allowable to combine, as Sidgreaves does, observations made months apart, in order to show the changes in the lines during the light-period. However the observations of Sidgreaves may assume greater importance when they are published in a more detailed form.

NOTE ON THE SPECTRUM OF THE GREAT NEBULA IN ORION.*

WILLIAM HUGGINS

With reference to Professor Campbell's observations on the spectrum of the Orion nebula (p. 384) it may be well for me to state at once that the photographs taken 1888-1890 are still in good condition, and fully justify the interpretation we put upon them at the time. We regret very much that the small scale of the photographs, and the delicacy of some of the details make it impossible to reproduce them, in a sufficiently adequate manner, for publication.

It was our intention to work on this nebula last autumn, but the spectroscope designed for this work was not completed in time. We hope to do so next season, and will give special attention to the points on which Professor Campbell's recent observations differ from our earlier ones.

As Professor Campbell's remarks on the broadening of certain portions of the lines upon our plates (pp. 391-393) seem to show that he has not understood correctly the interpretation we put upon this appearence, I may say now that the view we took, and still hold is that this broadening is purely a photographic spreading on account of greater brightness of the line at that place. This greater brightness might be due to more energetic radiation, but is to be attributed more probably to radiation from a large number of molecules in consequence of a greater depth of the nebula in the line of sight, or of local condensation, at these places.

Further we suspected that the strong photographic line at 3727 may vary in brightness relatively to the hydrogen lines, at

* Communicated by the author.

different points of the nebula, in a manner similar to the known variation in the visible region of the principal line to the line of hydrogen at F.

In the construction of a spectroscope for taking photographs of the spectra of stars as early as 1876 (*Phil. Trans.* 1880, p. 670). I reduced the time of exposure by using a short camera with a lens of $6\frac{1}{2}$-inches focal length and a ratio of nearly $\frac{f}{4}$. In one of the telescopes recently constructed the camera lens has half the focal length only of the lens of the collimator, namely 12 inches and an aperture $2\frac{1}{4}$-inches. In the other instrument with a longer collimator, the camera lens has a focal length of $5\frac{3}{4}$-inches only, and a ratio of $\frac{f}{4}$ nearly.

THE TEMPERATURE OF THE SURFACE OF THE FIXED STARS AND OF THE SUN, COMPARED WITH THAT OF TERRESTRIAL SOURCES OF HEAT.*

J. SCHEINER

In the course of my investigations on the spectra of the brighter stars, with the aid of photographs taken at the Potsdam Observatory, I was struck by the peculiar behavior of a line (λ 4482) which belongs to the spectrum of magnesium. In nearly all spectra belonging to class I this line is prominent, either on account of its breadth or its intensity; in spectra of this class which contain four lines, it even equals the hydrogen lines in width. It is also very prominent in the spectra of Sirius, Vega, Procyon and other stars in whose spectra lines are more abundant, although not in the same degree as in those first mentioned; on the other hand it is weak in the solar spectrum and in the other spectra of class II—indeed in some representatives of this class it does not appear,—and it seems as if this line becomes weaker the closer the spectrum approaches to Class III.

In the spectrum of magnesium when produced artificially this line is also subject to great variations in breadth and intensity. It cannot be recognized in the spectrum of the magnesium flame, or in that of magnesium vapor in the electric arc, but it reaches a very great intensity and breadth in the spark spectrum. Living and Dewar† have called attention to this behavior of the line, and

* Translated from the *Sitzungsberichte der k. preuss. Akademie der Wissenschaften zu Berlin*, March, 1894.

† Proc. Roy. Soc., XXX, p. 93.

the investigations of Kayser and Runge, as well as my own observations, are in confirmation of their statements.

It is of course natural to ascribe the peculiarities of the line to differences of temperature of the magnesium vapor in the electric arc and in the spark, and therefrom to draw further conclusions as to the temperature of the fixed stars; it is however impossible to sharply separate the influence of temperature from that of pressure, and the only allowable conclusion with reference to the stars is, that magnesium vapor in stars of Class I is in the same condition as in electric sparks of high tension, and in stars belonging to Class II it is in the same condition as in the electric arc.

But it is remarkable that another line of the magnesium spectrum (λ 4352) behaves, according to my observations, in just the contrary manner to that described above. It does not appear in any of the spectra of Class I containing four lines, but begins to be visible in richer spectra of this class: is very prominent in the Sun and stars of Class II*a*, and in α Orionis (Class III*a*) is one of the strongest lines in the spectrum. In laboratory experiments also, this line has just the reverse appearance of that at λ 4482. In the spark spectrum it is hardly if at all recognizable, while in the arc spectrum it is very strong and broad. Living and Dewar have also noticed this peculiar character of the line.

The favorable circumstances that two lines belonging to the same substance have this opposite behavior, proves at once that the appearances presented by these lines in the stars depend only on the temperature and not on the pressure. With increased pressure all the lines of a gas become broader and more prominent; it cannot happen as a consequence of Kirchhoff's law that a line can narrow with increased pressure. On the other hand it is a well-known fact that single lines may become weaker and narrower at higher temperatures, although in general lines under these conditions became stronger and broader. I believe therefore that I am justified in making the following statement:

The temperature of the so-called reversing layer—the outermost layer of the photosphere—in stars of class III*a* is approximately equal to that of the electric arc (*about* 3000° *or* 4000° *C.*); in the Sun, and in stars of class II*a* the temprature is higher, but does not reach that of the spark of a Leyden jar; in stars of class I*a* it is approximately equal to the temperature of this spark (upper limit about 15000° *C*).

With this result is also given, for the first time, a direct proof of

the correctness of the physical interpretation of Vogel's spectral classes, according to which class II is developed by cooling from I, and III by a further process of cooling from II.

NOTE ON THE SPECTRA OF COMETS *b* and *c* 1893.*

W. W. CAMPBELL.

Professor Kayser, in a *Note on the Spectra of Comets* in the March number of this journal based upon my observations† of *Comet b 1893*, refers to my comparisons of the comet bright-line spectrum with the artificial spectra of carbon and cyanogen only by saying that I have "committed some mistakes." I have read Professor Kayser's note with great care (and considerable anticipation), and find the "mistakes" attributed to me are *two* in number. Now with perfect respect for Professor Kayser and his excellent work, I would point out that his use of the English expresssion "committed some mistakes" is unfortunate and misleading.

One of the "mistakes committed" is:

(*a*) "Campbell supposes that this part of the spectrum [of carbon and cyanogen, between λ 436 and λ 423] has not been covered by our work, but here he is mistaken." This is not a scientific point, and must not be discussed by me. I can only judge of what has been done by what has been published. My reasons for saying, "The region λ 436-λ 423 does not appear to have been covered by the work of Kayser and Runge," were taken from their publication,‡ and are as follows:

1. In describing the spectrum they say, on page 5, "* * * no part of the spectrum between $\lambda = 620\mu\mu$ and $\lambda = 340\mu\mu$ is free from carbon lines; there are certainly over 10,000 of them."—This led me to suppose that the region λ 436-λ 423 contains numerous lines. Further, Angström and others place the limits of the fifth carbon band at λ 438 and λ 423; and H. W. Vogel's photographs of cyanogen-flame and Bunsen-flame spectra show a number of very prominent lines in the region 436-λ 423.

2. In the next paragraph, page 6, they say, "We naturally did not plan to measure the wave-lengths of all these lines; we confined ourselves indeed to giving in detail the second, third and fourth cyanogen bands (at λ 422, λ 388, λ 359;) likewise, as an

* Communicated by the author.
† Described in A. & A.-P. for August, 1893.
‡ *In Abhandlungen d. Berlin. Akad., 1889.*

example of a carbon band, the green band which begins at λ 516; and finally, for reasons which will appear later, the beginning of the band at λ 474."—It will be seen that *the region λ 436-λ 428 is not included in their program of work.*

3. There are five bands in the spectrum of carbon. They publish three wave-lengths in the second band, and verbally describe the first and second bands on page 8. Their measures and photographs describe the third and fourth bands perfectly. Each of the first four bands contains a large number of lines. They publish three wave-lengths in the fifth band (4382, 4371, 4365), and I did not find further information concerning this band in any part of their paper. For the region λ 436-λ 423, constituting nearly the whole of the fifth band, no photographs, nor wave-lengths, nor verbal descriptions, are given. It seemed to be a part of the carbon spectrum not described by them in any manner whatever.

The other "mistake" is this:

(*b*). "Campbell erroneously compares the first three of these lines [meaning 4098, 4073, 4052] with lines measured by us in the second group of cyanogen, which are much too weak to have been seen in the comet."—Their published photographs do not cover all this region; and from their list of wave-lengths I selected three lines because they occupy the proper positions for coinciding with three observed comet lines and are marked "*stark*" (that is, strong or relatively bright). If Professor Kayser holds that they are much too faint to appear in the comet, I readily agree that the comparison at that point is of small weight; but it does not constitute a positive "mistake."

In all other points Professor Kayser arrives at identically the same conclusions reached by me last year,* except that he gives one additional coincidence at λ 509 (with Angström and Thalen's 5098 carbon line), and ascribes the comet lines λ 4366, λ 4313 without doubt to two bands of burning hydro-carbon, whereas I attributed them to possible lines in the flame spectrum of cyanogen. The subject is of sufficient importance to warrant the reproduction of my table of comparisons, and include in it the results just offered by Professor Kayser. His acknowledged authority on carbon and other spectra gives these results great weight, and his closing sentence contains an hypothesis which, if finally confirmed will be most important.

* In A. and A.-P. for August, 1893, and more complete comparisons in *Publ. Astr. Soc. Pac.* for Sept. 1893.

Comet lines—visual observations.		Identifications	
Wave Lengths	Description	Campbell	Kayser
600	Max. of red band broad faint	619-595 carbon	619-595* carbon
5633	Very faint line, edge yellow band. .	5635 "	5635 "
5588	Bright line in yellow band . . .	5585 "	5585 "
5163	Very bright line edge green band	5165 "	5165 "
5126	Very bright line in green band	5129 "	5129 "
5001	Very bright line in green band	—— "	5008* "
4734	Bright line edge blue band	4737 "	4737 "

Comet lines—photographic observations.		Identifications	
Wave Length	Description	Campbell	Kayser
4736	Very bright line, edge blue band.	4737 carbon	4737 carbon.
4716	Very bright line in blue band	4715 "	4715 "
4697	Very bright line in blue band....... . . .	4698 "	4698 "
4683	Very bright line in blue band	4685 "	4685 "
4675	Apparently very bright line not clearly separated from 4683.	4676‡ "	4677* "
4366	Very bright line.	Cyanogen flame†	Burning hydrocarbon.
4350	Very faint	—	—
4335	Faint line	—	—
4313	Very bright line	Cyanogen flame†	Burning hydrocarbon.
4298	Very bright line	—	—
4254	Very faint line	—	—
4234	Very faint line	—	—
4214	Very bright line.	4216 cyanogen	4216 cyanogen
4196	Bright line..	4197 "	4197 "
4178	Very faint line, uncertain.......	4181 "	4181 "
4126	Faint line	4128 "	—
4098	Bright line	4099 "	—
4073	Bright line	4074 "	—
4052	Bright line	4053 "	—
4043	Bright line	—	—
4019	Bright line.	—	—
4011	Faint line	—	—
3988	Very faint line..	—	—
3881	very bright line, probably brightest in spectrum	3883 cyanogen	3883 cyanogen
3870	Very bright line, resembles a band.	3871 "	3871 "

Professor Kayser calls attention to the fact that a cyanogen group of lines at λ 461-λ 450 was not observed in this comet, perhaps owing to faintness, or because it might be covered by the end of the fourth carbon group. A careful re-examination of the

* Angstrom and Thalen.
† Swan, Hasselberg, Watts.
‡ Watts.

negatives has revealed no signs of its presence. But I probably observed that group visually in *Comet c* 1893. (The spectrum is described in Publ., A. S. P., pp. 208-10). In this comet, in addition to the three carbon bands usually present, I observed two bands possibly not hitherto seen: one at λ 455 which I attributed to the cyanogen group of lines shown in H. W. Vogel's photographs, and a narrow band or line near λ 4863, which may possibly be the $H\beta$ hydrogen line. The green and blue carbon bands in this spectrum were much narrower, and more suddenly terminated on the more refrangible side than usual. The two extra bands, in addition to the usual three, were observed on four nights. It was not possible to get photographs of the spectrum.

LICK OBSERVATORY, 1894, March 20.

ON THE PHOTOGRAPHIC SPECTRUM OF THE GREAT NEBULA IN ORION.*

J. NORMAN LOCKYER, C. B., F. R. S.

The paper consists of a description and discussion of photographs of the spectrum of the Orion Nebula, taken with the 30-inch reflector at Westgate-on-Sea in February, 1890, of which a preliminary account was communicated to the Royal Society at the time. Fifty-four lines are tabulated as belonging to the spectrum of the nebula, nine of them being due to hydrogen. Tables are given showing:

1. The wave-lengths, intensities, and probable origins of the lines photographed in the spectrum of the nebula.
2. A comparison of the lines in the spectrum of the nebula with lines in the spectra of (*a*) P. Cygni, (*b*) bright line stars and planetary nebulæ, and (*c*) stars in Groups II, III, and IV, of the classification according to the meteoritic hypothesis.

The complete discussion has led to the following general conclusions:—

1. The spectrum of the nebula of Orion is a compound one consisting of hydrogen lines, low temperature metallic lines and flutings, and high temperature lines. The mean temperature, however, is relatively low.†

* Abstract of a paper read before the Royal Society. Communicated by the author.

† *Roy. Soc. Proc.* Vol. 43, p. 152, 1887.

2. The spectrum is different in different parts of the nebula.
3. The spectrum bears a striking resemblance to that of the planetary nebulæ and bright line stars.
4. The suggestion, therefore, that these are bodies which must be closely associated in any valid scheme of classification is confirmed.
5. Many of the lines which appear bright in the spectrum of the nebula appear dark in the spectra of stars of Groups II and III; and in the earlier stars of Group IV, and a gradual change from bright to dark lines has been found.
6. The view, therefore, that bright line stars occupy an intermediate position between nebulæ and stars of Groups II and III is greatly strengthened by these researches.

THE SPECTRUM CHANGES IN β LYRÆ. PRELIMINARY NOTE.*

J NORMAN LOCKYER, C B, F R S

The spectrum of this well known variable star was first investigated photographically by Professor Pickering, at Harvard College Observatory, and a preliminary account of the results was published in 1891.† Dark and bright lines were found to be associated in the spectrum, and further, the bright lines were found to change their positions with respect to the corresponding dark ones according to the interval of time which had elapsed since the preceding minimum.

It may be remarked that the period of the light-changes of the star is about twelve days twenty-two hours, and there are two approximately equal maxima of mag. 3.4, a principal minimum of mag. 4.5, and a secondary minimum of 3.9, the period of variation stated being that which elapses between two successive principal minima.

Professor Pickering found that during the first half of the period—that is, between principal and secondary minima—the bright lines were on the less refrangible sides of the corresponding dark ones, while during the second half they were displaced to the more refrangible sides. He further remarked that "the actual changes in the spectra, when studied in detail, are much more complicated than has been stated above, and show a variety of intermediate phases and changes in the dark as well as in the bright lines."

* Read before the Royal Society. Communicated by the author.
† *Ast. Nach.*, 2707; Observatory, 1891, p. 341.

At Professor Pickering's request, I took up the work at Kensington in July, 1891, the instrument employed being the 6-inch Henry object glass and prism of 7½°, which I have described in a previous communication.* Several photographs were taken with this instrument, but it was not until the new 6-inch prism of 45°† was employed in the research that any considerable advance was made. With the higher dispersion of this instrument the spectrum is depicted in greater detail, and more minute changes can therefore be determined.

Since my work was commenced, accounts of the photographic spectrum of β Lyræ have been published by Belopolsky,‡ Father Sidgreaves,§ and Vogel,‖ and various suggestions have been made by them and others as to the conditions which bring about the variability.

On this account, although the reductions of the sixty-four photographs which I have obtained are not yet completed, I have thought it desirable to give a brief *résumé* of the facts already acquired.

For the complete study of the problem more photographs will be required, and a considerable amount of time will be required for the discussion of them. The present communication, therefore, is limited to a preliminary consideration of the variation in the spectrum as photographed at Kensington, and I have consequently in it omitted reference to the results obtained by other workers. In a subsequent paper, however, a complete history of the subject will be given.

To facilitate references to the spectrum, thirteen photographs—roughly one for each day of the period—are reproduced in Plate 1. These have been enlarged about three times from the original negatives.¶

As it is a matter of great difficulty to mount a series of such photographs showing the exact coincidences of the lines, in comparing the different spectra in the plates, some allowance must be made for the slight differences in scale. Further, it is right to add that probably some of the fainter lines shown in the photographs are artificially produced by the process of enlargement, but the real lines will be readily identified by their appearance in more than one spectrum; the lines of particular interest are indicated in Plate 2.

* *Phil. Trans.*, 1893, vol. 184, p. 678.
† *Ibid.*, p. 679.
‡ *Mem. Soc. Spett. Ital.*, June, 1893.
§ *Monthly Notices*, R. A. S., 1894, p. 96.
‖ *Sitzungsberichte*, Berlin, February, 1894.
¶ The plates are omitted.

The light curve which forms part of Plate 1 is constructed after Argelander's drawing,* and the dotted lines drawn from the spectra to the period scale indicate the relation of each photograph to the light curve.

I proceed to state, step by step, the results of the preliminary examination of the photographs, and to indicate the spectral phenomena on which they are based.

1. *The spectrum is constant at the same interval from principal minimum.*

Apart from the slight differences, which seem to be accounted for by differences in the atmospheric conditions and consequently in the quality of the negatives, the spectrum appears to be the same at the same interval from minimum. The photographs reproduced in Plate I have been selected as being specially suitable for reproduction, but at most of the phases duplicates which are practically identical have been obtained.

2. *The kinds of variation shown on the photographs are as follows:*

(*a*) Periodical changes in the relative intensities of the lines.

(*b*) Periodical doublings of some of the dark lines.

(*c*) Periodical changes in the positions of the bright lines with respect to the dark ones.

3. *There are two bodies involved giving dark line spectra.*

On reference to Plate I it will be seen that at, and just before and after the second maximum, some of the dark lines are doubled. This indicates two sources of light giving dark line spectra and moving relatively to each other in the direction of the line of sight. When the relative movement in the line of sight is zero, none of the lines are doubled. The latter condition occurs about the time of the two minima.

4. *The maximum relative velocity of the two dark line components in the line of sight is about 156 miles per second.*

The greatest separation of the dark lines occurs about the time of second maximum, and the relative velocity, as determined by measurements of three of the doubles in the photograph of August 24, 1893, is that stated above. The individual measurements are as follows:

$$H\gamma = 155 \text{ miles per second}$$
$$H\delta = 154 \quad \text{"}$$
$$\lambda\, 4025 = 158 \quad \text{"}$$

5. *One of the dark line components bears a strong resemblance to Rigel and the other to Bellatrix.*

* De stella β Lyræ Disquisitio.

The spectra of the two components can readily be separated, for the reason that only lines common to both will be doubled. Among these are the lines of hydrogen. Lines special to either component are always single, and they retain the same relative positions with respect to one group of hydrogen lines throughout the period.

In Plate 2 photographs are given to facilitate an analysis of the compound dark-line spectrum. At the bottom of the diagram is a reproduction of a photograph taken near the time of second maximum (August 24, 1893), and the spectra of Rigel and Bellatrix are included in the same plate. The compound character of the dark line spectrum of β Lyræ at this time is shown by the fact that one group of lines corresponds very closely with those which appear in the spectrum of Rigel and when these are subtracted from the whole spectrum, a spectrum closely resembling that of Bellatrix remains, the latter spectrum being displaced in this photograph to the more refrangible side, as shown by the short lines drawn beneath the spectrum. The resemblance of the two components to Rigel and Bellatrix respectively, the spectra of which I have described in a previous paper,* is further shown by the following tabular comparison, the two dark line components of β Lyræ being called R and B respectively.

Component R	Rigel		Component B	Bellatrix	
Wave-Length	Wave-Length	Intensity.	Wave Length.	Wave Length.	Intensity
				3919	2
				3926	3
3933	3933	6	3933	3933	3
	3963	2	-	3963	3
3968	3968	6	3968	3968	6
3994	3994	1		3994	4
4008	4008	2	..	4008	5
4025	4025	3	4025	4025	6
				4040	2
				4069	2
				4071	2
				4075	2
4101	4101	6	4101	4101	6
				4104	2
				4119	2
4120.5	4120.5	2	4120.5	4120.5	4
4127	4127	5			
4130	4130	3			
4143	4143	2	4143	4143	5
				4168	3
	4172	1		4172	1
	4177	1	.	4177	1

* *Phil. Trans.* 1893, vol. 184, p. 693.

Component R	Rigel		Component B	Bellatrix	
Wave-Length	Wave-Length	Intensity.	Wave-Length.	Wave Length.	Intensity
4233	4233	2			
				4241.5	2
				4253	2
4267	4267	2		4267	[illegible]
4340	4340	6	4340	4340	6
				4345	2
4351	4351	[illegible]	4351	4351	2
4388	4388	3	4388	4388	6
				4394.3	[illegible]
				4414.5	2
				4417	2
				4457	[illegible]
4471	4471	4	4471	4471	6
4481	4481	5	4481	4481	[illegible]
				4553	[illegible]

It is not intended to suggest that the spectra of the two dark-line components are quite identical with those of Rigel and Bellatrix. These are simply the best known stars which they most closely resemble, and the similarity is pointed out as an indication that we have not to deal with bodies of an unfamiliar type. Throughout the paper I shall refer to the two components as R and B respectively.

The conditions at first maximum, as shown in Plate 1, are not so simple as those at second maximum, though there is evidence to show that at this point of the light curve the component B is receding with respect to R. As will be seen on reference to the photograph of March 13, 1894, the hydrogen lines are broadened, and the two lines near 4471 and 4481 have approached each other, as they should do if one belongs more especially to R and the other to B.

6. *When the two bodies lie along the line of sight, partial eclipses occur. This happens near the minima of the light curve.*

The differences in the intensities of the dark lines special to R and B, near the two minima, indicate that near the principal minimum R is partially eclipsed by B, while near the secondary minimum B is partially eclipsed by R. These changes will be seen on Plate 1, again in Plate 2. In the latter we have comparisons of β Lyræ at the two minima, with Bellatrix and Rigel. If we leave the bright lines out of consideration, it will be seen that near principal minimum, the spectrum of β Lyræ greatly resembles that of Bellatrix, the component B in this case lying between us and component R. As the eclipse is not total, however, the

The spectra of the two components can readily be separated, for the reason that only lines common to both will be doubled. Among these are the lines of hydrogen. Lines special to either component are always single, and they retain the same relative positions with respect to one group of hydrogen lines throughout the period.

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3933	3933	6	3933	3933	3
	3963	2		3963	3
3968	3968	6	3968	3968	6
3994	3994	1	..	3994	[illegible]
4008	4008	2		4008	5
4025	4025	3	4025	4025	6
				4040	2
				4069	2
				4071	2
				4075	2
4101	4101	6	4101	4101	6
				4104	2
				4119	2
4120.5	4120.5	2	4120.5	4120.5	4
4127	4127	3			
4130	4130	3			
4143	4143	2	4143	4143	5
				4168	3
	4172	1	..	4172	1
	4177	1		4177	1

* *Phil. Trans.*, 1893, vol. 184, p. 693.

8. *The bright lines are brightest soon after secondary minimum.*

If the brightness of the lines in reality remains constant, they will appear relatively brightest at the two minima, owing to the reduction of continuous spectrum which is associated with the increased brightness of the star at maximum, and for the same reason they should appear brighter at principal than at secondary minimum. Estimates of the brightness of the lines in relation to the continuous spectrum have been made independently by four of my assistants, and, although estimates of this kind are liable to error, the general agreement is sufficient to indicate that when all allowance is made for the varying continuous spectrum, there is a maximum of brightness of the bright lines about half a day after secondary minimum. The apparent increase of brightness near principal minimum seems to be due solely to the reduced intensity of the continuous spectrum.

I have to express my obligations to Messrs Fowler, Baxandall, Shackleton, Butler, Wardale, Crabtree, and North, who, at different times, have assisted in taking the photographs

ON BRESTER'S VIEWS AS TO THE TRANQUILITY OF THE SUN'S ATMOSPHERE.*

EGON VON OPPOLZER

Mr. A. Brester Jr., has recently published a totally new theory of the Sun. His fundamental statement is, that the atmosphere of the Sun is quite calm. Brester arrives at this conclusion by supposing that the different gases form layers, which are disposed according to their specific weight, and by assuming the absense of an atmospheric circulation resembling that produced by the different temperatures of pole and equator in the earth's atmosphere, which is the cause of its violent atmospheric motions We shall see that the conclusions which he draws from these suppositions cannot be regarded as valid, as they are contradictory to established facts.

It is but natural to suppose that the gases in the atmosphere of the Sun should strive to range themselves according to their specific weight, as every movement tends to the restoration of equilibrum, which can only subsist with this disposition of layers. If, moreover, we take into consideration that the principal

* Communicated by the author.

The spectra of the two components can readily be separa for the reason that only lines common to both will be dou Among these are the lines of hydrogen. Lines special to e component are always single, and they retain the same rel positions with respect to one group of hydrogen lines thro out the period.

In Plate 2 photographs are given to facilitate an analys the compound dark-line spectrum. At the bottom of the gram is a reproduction of a photograph taken near the tir second maximum (August 24, 1893), and the spectra of Rigel Bellatrix are included in the same plate. The compound cha ter of the dark line spectrum of β Lyræ at this time is show the fact that one group of lines corresponds very closely those which appear in the spectrum of Rigel and when these subtracted from the whole spectrum, a spectrum closely res ling that of Bellatrix remains, the latter spectrum being displ in this photograph to the more refrangible side, as shown by short lines drawn beneath the spectrum. The resemblance of two components to Rigel and Bellatrix respectively, the spe of which I have described in a previous paper,* is further sh by the following tabular comparison, the two dark line comp ents of β Lyræ being called R and B respectively.

Component R.	Rigel		Component B.	Bellatrix	
Wave Length	Wave-Length	Intensity	Wave-Length.	Wave Length	Inte
				3919	
				3926	
3933	3933	6	3933	3933	
	3963	2	..	3963	
3968	3968	6	3968	3968	
3994	3994	1	.	3994	
4008	4008	2	.	4008	
4025	4025	3	4025	4025	
				4040	
				4069	
				4071	
				4075	
4101	4101	6	4101	4101	
				4104	
				4119	
4120 5	4120.5	2	4120.5	4120.5	
4127	4127	3			
4130	4130	3			
4143	4143	2	4143	4143	
				4168	
	4172	1		4172	
	4177	1	..	4177	

* *Phil Trans*, 1893, vol. 184, p. 693.

body. According to Dunér the daily angle of rotation on the equator amounts to 14° 14′ in a latitude 45° to 11°99′. This demonstrates the existence of winds blowing over the interior of the Sun with a rapidity of at least 100 meters per second. To explain the peculiar rotation and apparently not to fall into contradiction with the assumption of a calm atmosphere, Brester propounds the hypothesis, that the flattened interior of the Sun is surrounded by a spherical atmosphere each having a different constant angle of rotation. This hypothesis is incompatible with mechanical principles, and moreover it would make a quite calm atmosphere impossible. If we consider the observation of sunspots, we find evidence of the most violent storms which last for days, and the average energy of which is more than three-fold as strong as that of the most violent storms of our atmosphere. The variation of spots in heliographical longitude and latitude demonstrates the existence of storms with a velocity of at least 100 metres. If we regard the first series of observations in Dunér's classical work, "*Recherches in la rotation du soleil*" (*Soc. Royal des 20 d' Upsal*), we find the following velocities per second at the equator:

1887 Date		velocity (km.)
June	3	+ 2.25
"	3	+ 1.84
"	4	+ 1.79
"	4	+ 2.25

This again corresponds to differences of velocity of 460 metres, consequently winds of at least 230 metres must exist. That real importance must be attributed to these differences, is to be seen by the probable error of Dunér's observation, which amounts to 0.02 km. Even if we quote Brester's own words, we see that his own theory leads to the conclusion that violent storms must exist on the Sun's surface:

"My theory also explains the Moon's motions as shown by the spots. They owe their different angular velocities in different latitudes to the cloudy zones in which they are borne. If they frequently move a little more rapidly than these clouds it is because the gas, which in the growing spot pushes back the matter of the photosphere, must move particularly toward the side where the resistance is least. Now as this side is that of the foremost edge of the spot, every time a spot undergoes sudden changes it ordinarily advances on the solar surface by making a sort of leap.* This leap will also take place when, as the gaseous contents of a spot are recondensing, the vacuum thus produced will draw in

* Young, The Sun, p. 110.

again the surrounding photospheric matter. For this matter rushing in, preferably on the side where it already moves in the direction of motion, will fill up the spot from behind, once more giving to its center a sudden acceleration."*

Brester's theory is accordingly not based on sound reasoning, it assumes conditions *a priori* improbable and impossible, as for instance the state of repose, or the flattened interior of the Sun with spherical atmosphere turning with a different angular velocity, and it leads to consequences which are not consistent with the fundamental idea. Brester's theory like that of Schmidt can be ranged among the interesting ones,—interesting in that they upset all hitherto existing opinions, and still try to explain everything in an *apparently* natural way. As long as nothing prevents the acceptance of a very high temperature of the Sun, or a hardly conceivable evaporation of the gases above the photosphere, one is not justified in grasping at revolutionary ideas. A clear insight into the properties of the atmosphere, such as modern meteorology is leading us to, will alone help to a final solution of the phenomena about the surface of the Sun. Let us continue in the track beaten by Galileo and Kepler, who at once divined the meteorological nature of the sunspots.

WEINDEN, 17 APRIL, 1894.

ASTRO-PHYSICAL NOTES.

All articles and correspondence relating to spectroscopy and other subjects properly included in ASTRO-PHYSICS, should be addressed to George E. Hale, Kenwood Observatory of the University of Chicago, Chicago, U. S. A. Authors of papers are requested to refer to last page for information in regard to illustrations, reprint copies, etc.

The Spectrum of β Lyræ.—Since Pickering's discovery of the composite character of the spectrum of β Lyræ, spectroscopic literature relating to this star has been rapidly accumulating. Important papers on the photographic spectrum have been published by Belopolsky, Vogel, Sidgreaves and Lockyer, and in general the results contained in them are considered to be only preliminary, so that still more complete papers may be expected. While observers differ as to some of the details, they are in fair agreement as to the main features of the spectral changes, nor are the visual observations of the spectrum made at the Lick Observatory in disagreement with the photographic results, when one considers that they represent an imperfect view of phenomena which are more accurately and completely recorded by photography.

So far no satisfactory hypothesis has been framed which accounts for the complex spectral changes of β Lyræ. The assumption of two bodies of different

* ASTRONOMY AND ASTRO-PHYSICS, March 1894, p. 228.

character moving in a circular orbit satisfies the observations of the star's light period, but leaves unexplained or flatly contradicts some of the most striking changes in the spectrum. Professor Vogel considers that even the large mass of material accumulated at the Potsdam Observatory is insufficient to serve as the basis of a satisfactory explanation.

It is worthy of remark that none of the recent writers on the photographic spectrum make any reference to the anomalous variations of brightness of the D_3 and other bright lines, reported by earlier observers. These apparent variations were quite probably due to differences in atmospheric conditions and to the small aperture of the instruments employed. It is at least a suspicious circumstance that the D_3 line in the spectrum of *γ* Cassiopeæ was observed at O'Gyalla in 1891 (Beobachtungen, Vol XIII and XIV, p 10) while during this year the star was frequently observed at Mt. Hamilton with the 36-inch equatorial and no line ever found at the place of D_3.

The Appearance of the D Line in the Spectrum of the Chromosphere.—In the May number of the *Memorie della Societa degli Spettroscopisti Italiani*, Herr Belopolsky gives some observations on the appearance of the helium line in the spectra of solar prominences. On a number of occasions a dark line was observed in the helium line, not in the middle, but somewhat toward one side. At first this was supposed to be a reversal of the line, and the unsymmetrical appearance was attributed to instrumental defects, but investigation showed that the appearance was real. A dark line was sometimes observed on the other side of the center.

As these absorption lines were not seen when the atmosphere was dry, but were quite distinct when it was moist, it was concluded that they are of telluric origin. The less refrangible line is double. The following measures of wave-lengths (Potsdam system) were made:

5876.5 double
5876.0 D_3
5875.8

Researches on the Spectra of the Metals by Professor Hasselberg.—In vol. 26 of the Proceedings of the Royal Academy of Sciences of Sweden, Professor Hasselberg begins a series of monographs on the spectra of the metals. He points out in the introduction that until very recently our knowledge of the exact wave-lengths of the lines of the metals was in a very unsatisfactory state, and quite inadequate to meet the demands of solar or even of stellar spectroscopy. The labors of Rowland and of Kayser and Runge have done much to remedy the difficulty, and it might even seem that such work as that of Hasselberg could now be regarded as superfluous. Aside from the value of independent confirmatory measurements, however, the different point of view from which Hasselberg regards the subject gives a special importance to his researches. The main object of Kayser and Runge was to discover numerical relations between lines in the spectra of the elements, and while it was important for their purpose that no line belonging to an element should be overlooked, it was of less consequence that lines belonging to some other element should be included. In Hasselberg's investigations special attention is paid to excluding all foreign lines.

The first of the series of monographs is on the spectrum of chromium. The wave-lengths are given on Rowland's scale, and are in close agreement with the results of Kayser and Runge. A carefully executed lithographed map accompanies the memoir.

The Photometric Catalogues of the Harvard College Observatory.—Professor Pickering replies, in *A. N.* 3229, to Dr. Chandler's criticisms of the Harvard photometric observations. The specific cases of error pointed out by Chandler are in general confirmed, but Professor Pickering objects to the inference that the magnitudes in the whole work are to be regarded with distrust. The errors referred to relate to variable stars, the observations of which are attended with unusual difficulties. "It is somewhat as though it should be argued from a physician's losing twenty per cent of his cholera patients that he had been equally unfortunate in his general practice." With the possible prejudice hinted at by Professor Pickering in this connection we have nothing to do; it is however natural to suppose that the attention of one specially interested in variable stars would be first attracted by errors relating to this class of objects.

With regard to the identification of stars observed with the photometer, Professor Pickering points out that when a star is observed out of the meridian the position of the reflecting mirror is recorded, together with the time, so that the approximate right ascension of the star can be determined from the record. The volume in which the observations of variable stars are to be discussed, and any errors in them considered has not yet been published.

For the great mass of stars in the Harvard Photometry which are not variable, comparison of the results with themselves and with other catalogues shows that the average error does not exceed one-tenth of a magnitude.

The Nature of Comet-Spectra.—In *A. N.* 3229, Professor Kayser replies to some comments by Professor Vogel on the paper translated in our May number, and gives some further consideration to Vogel's view that some comet spectra are made up of the superposed spectra of carbon and carbon monoxide. It seems quite certain that if the CO bands were bright enough to cause a displacement in the maximum brightness of the principal carbon bands, such of them as fall at other parts of the spectrum would have been visible. Others would easily be detected by photography, which has recently been applied with so much success to the investigation of comet-spectra.

Leaving out of consideration the influence of the slit-width, the maximum brightness of the cometary bands should, according to Professor Vogel's explanation, always fall at certain definite wave lengths. But the position of the maximum varies with the slit-width, and in fact very different positions have been found by different observers. As the slit-width used was not recorded, the observations are inconclusive, but the tables given by Professor Kayser will enable observers in the future to correct their measurements, and reduce them to the values that would have been obtained with a narrow slit.

It seems to us that, even with the advantage of the tables supplied by Professor Kayser, the results of visual observations would be subject to considerable doubt, except perhaps in the case of unusually bright comets. In this field, as in so many others, photography promises to be of the greatest usefulness.

New Observatories.—An editorial note in *A. N.* 3232 states that the founding of an Observatory at the University of Heidelberg is now an assured fact. The Observatory at Karlsruhe will be abandoned, and its instruments will be used in equipping the new Observatory, which will also have an astro-physical department. The site is on the Geisberg, about 270 metres above the level of the Rhine, and thirty-five minutes walk from the University.

The astronomical department will be under the direction of Professor Valentiner, the astro-physical under that of Professor Max Wolf.

Dr. Lewis Swift has transferred his instruments including the 16-inch refractor from the Warner Observatory at Rochester to Echo Mountain, Los Angeles County, California. The new institution is called the Lowe Observatory. It is about 3,500 feet above the sea level and about two miles from the station on Wilson's Peak formerly occupied by a party from Harvard College Observatory.

Radiation of Heated Gases.—In the *Philosophical Magazine* for March there is an article by Professor Smithells upon a subject which lies at the very foundation of astro-physics. Stated in terms of a concrete example, the question is why does a sodium bead turn the Bunsen flame yellow? Is the sodium chloride dissociated by heat, and does the free sodium atom then radiate light in virtue of its increased temperature?

Or, is there involved some process, perhaps like that of phosphorescence, which may be called, after E. Wiedemann, luminescence? Or is some purely chemical process, say reduction, an essential condition of luminosity? Or still again, is the phenomenon possibly an electrical one ultimately similar to that of the Geissler tube? Or is it true, as has been suggested, that in some, or all, cases we have two or more of these processes going on at the same time?

To state the question in *general* terms, are the conditions of Kirchhoff's law satisfied? If so is Kirchhoff's law itself satisfied? If not, what conditions are to be substituted?

The conditions of the above mentioned law are

(1) That the radiation emitted shall be at the expense of heat only, and

(2) That the radiation absorbed shall assume the form of heat only.

The law in question states equality between the following two ratios, *viz*:

(1) The ratio of the radiant energy of any one kind, emitted by a given substance at given temperature, to the radiation *of the same kind* given out by a perfectly black body *at the same temperature*, and

(2) The ratio of the absorbed radiation to the incident radiation when the absorbing body is of the substance in question.

As ordinarily placed in symbols this reads

$$\frac{E}{e} = A$$

Possibly it is simpler to say that $\frac{E}{A}$ is a constant for all bodies.

To this whole subject have recently been made a number of contributions which are of more than ordinary interest. Space forbids us to do much more than mention them.

Smithells' paper is preliminary. It contains a clear statement of the case, hints at some experimental difficulties, and concludes with an excellent critique of the work of Pringsheim.

To the latter is due the credit of having again started to rolling the ball which was first set in motion by Hittorf and W. Siemens. His (Pringsheim's) results may be found in *Wied. Ann.*, Bd. 45, p. 428, (1892), Bd. 49, p. 437 (1893), and Bd. 51, p. 441 (1894).

Briefly put, his views are that reduction processes are always at work in luminous flames, by which he means those flames which yield characteristic line spectra; that heat alone cannot bring out line spectra; and that, therefore, the conditions of Kirchhoff's laws are not satisfied, at least in luminous flames.

Still more recently, Paschen has taken up the subject and has pursued it with

great skill. His work is contained in three papers, the first of which was reviewed in this journal two months ago; the other two will be found in the current volume of *Wiedemann's Annalen* (Bd. 51, pp. 1-39 and pp. 40-46).

Paschen notes ambiguity in Pringsheim's use of the expression "characteristic spectrum," and proceeds to map and compare the absorption and emission spectra of carbon dioxide and water vapor, between the temperatures of 100° C. and 500° C. Kirchhoff's law he finds for these gases is certainly true in a qualitative, and probably true in a quantitative, way. His view is that this law is satisfied, both as to condition and content, in many radiating bodies, but that, in luminous flames, more is involved than mere heating; indeed, in all cases in which the radiation exceeds that of a black body at the same temperature, luminescence of some kind is at work.

The chief value of Pringsheim's work is perhaps that it confirms the view that a *uniform* temperature, equal to or less than that of the blast furnace, is incompetent to bring out the bright line spectra of metallic vapors. Concerning what may happen at higher temperatures no inference is to be drawn from his experiments.

Paschen, on the other hand, shows that since certain heated gases *do* obey Kirchhoff's law, they *probably* satisfy its conditions, *i. e.*, their radiation is probably due to heat alone. His papers are full of suggestion at every point, and the outline given above is extremely meagre.

Taken all together, these experiments show that the problem of the Bunsen burner is surprisingly complicated, and, so far from being solved, is as yet barely capable of clear statement.

We have here also fair warning against hasty interpretation of stellar and solar spectra. Why is one line sharp and another hazy? What is the meaning of an asymmetric reversal? Is not motion in the line of sight sometimes a great convenience? Is not luminescence a happy word to cover our ignorance? H. C.

Solar Observations at the Royal Roman College.—The following is an extract from a letter from Professor Tacchini:

I send you a resumé of the solar observations made during the first quarter of the year 1894.

SPOTS AND FACULÆ.

1894.	No. of days of Observations	Relative Frequency. of Spots	Relative Frequency. of days without spots.	Relative Size of Spots	Relative Size of Faculæ	No. of Groups per day
January	19	24.37	0.00	106.1	74.2	7.2
February	20	19.35	0.00	136.3	65.8	6.3
March	20	17.51	0.00	48.1	57.3	4.8

PROTUBERANCES.

1894.		Average No. per Day	Average Height per Day	Average Breadth per Day
			"	"
January	14	6.00	37.1	1.6
February	18	7.17	37.6	2.6
March	18	8.11	37.5	2.2

For the spots and faculæ a progressive diminution is shown. The great extent of the area of spots in February is due to the great spot in the southern hemisphere (−24° to −35°). The phenomena of the protuberances, on the other hand, show an increase when compared with the last quarter of 1893.

For the distribution of solar phenomena according to the latitude, we have obtained the following results.

First Quarter of Year 1894

Latitude	Protuberances		Faculæ		Spots	
90 + 80	0.000					
80 + 70	0.000					
70 + 60	0.003					
60 + 50	0.018					
50 + 40	0.008	0.324	0.000			
40 + 30	0.039		0.005		0.000	
30 + 20	0.080		0.072	0.428	0.062	0.391
20 + 10	0.088		0.154		0.185	
10 + 0	0.088		0.197		0.144	
0 — 10	0.057		0.197		0.155	
10 — 20	0.065		0.192		0.339	0.6[illegible]
20 — 30	0.111		0.120	0.567	0.103	
30 — 40	0.103		0.048		0.021	
40 — 50	0.013	0.676	0.010			
50 — 60	0.015					
60 — 70	0.222					
70 — 80	0.08[illegible]					
80 — 90	0.011					

All the solar phenomena were most frequent in the southern zones, and the same result is found for each month of the quarter. An unusual maximum of protuberances is to be noticed in the zone — 60° to — 70° which is also found for each month, while protuberances are very infrequent between + 40° and + 70°, and fail entirely between + 70° and + 90°. We have on only one occasion, on March 1, found evidences of eruption in latitude — 15°. P. TACCHINI.

CURRENT CELESTIAL PHENOMENA.

Planet Notes for September and October.

Mercury will be at superior conjunction Sept. 2 and will be in poor position for observation during the two months. He will be in conjunction with Saturn Sept. 30 and with Uranus Oct. 14. He will be at greatest eastern elongation, 24° 31' E. from the Sun, on the morning of Oct. 19. In the evening about this time Mercury, to northern observers, will set only a half hour after the Sun, so that it can be seen only in bright twilight. In the southern hemisphere the conditions for observation will be better.

Venus will remain 'morning star' during these months, steadily approaching the Sun and growing fainter. She will be in conjunction with the Moon Sept. 27 and Oct. 27. On Oct. 9 at 10^h 56^m A. M. Venus will be just 7' north of the star η Virginis, and on Oct. 29 at 10^h 07^m A. M. she will be 1° 06' south of Saturn. Both however will be too close to the Sun to be easily seen.

Mars during these months will be in excellent position for observation. He will be at opposition Oct. 20. His distance from the Earth will then be about 40,500,000 miles, or about 5,000,000 miles greater than it was at the opposition of 1892. His declination, however, is 33° further north, so that for northern observers the planet is in very much better position than in 1892. Professor Pickering has already reported interesting observations of the surface markings of the planet, made at the new Lowell Observatory at Flagstaff, Arizona, and it is not too much to expect that more and better observations will be obtained this

year than ever before. Mars is now in the constellation Pisces moving eastward. Sept. 15 he will turn the loop in his apparent course and begin retrograde (westward) motion, remaining in Aries and the corner of Pisces during the two months. The reader will easily recognize Mars by the ruddy color and great brilliancy, this being the brightest object in the southeastern sky. Mars will be 7° south of the Moon Sept. 18 at 10^h 49^m A. M. and 5° 31′ south of the same on Oct. 15 at 6^h 31^m A. M.

Jupiter is the brilliant star one sees rising a little to the north of east soon after midnight. In October Jupiter will be in position to be observed a little before midnight. He will be at quadrature, 90° east from the Sun Sept. 28; at conjunction with the Moon Sept. 22 at 3^h 09^m and Oct. 19 11^h 05^m P. M. Jupiter is in the feet of Gemini moving eastward, but will begin retrograde movement Oct. 24.

Saturn and *Uranus* will not be in position for observation, Saturn reaching conjunction with the Sun Oct. 21 and Uranus Nov. 7.

Neptune may be observed after midnight. He is in Taurus quite near the sixth magnitude star *l* Tauri.

Planet Tables for September and October.

[The times given are local time for Northfield. To obtain Standard Times for Places in approximately the same latitude add the difference between Standard and Local Time if west of the Standard Meridian or subtract if east].

Date.	R. A. h m	Decl. ° ′	Rises h m	Transits h m	Sets h m
			MERCURY.		
Sept. 5	11 08.8	+ 7 12	5 38 A. M.	12 09.4 P. M.	6 41 P. M.
15	12 13.1	− 0 39	6 33 "	12 34.3 "	6 35 "
25	13 10.7	− 8 01	7 21 "	12 52.4 "	6 24 "
Oct. 5	14 04.2	− 14 23	8 01 "	1 06.4 "	6 12 "
15	14 53.0	− 19 20	8 57	1 15.7 "	5 59 "
25	15 29.6	− 22 05	8 43 "	1 12.9 "	5 43 "
			VENUS.		
Sept. 5	9 35.6	+ 15 16	3 31 A. M.	10 36.5 A. M.	5 42 P. M.
15	10 23.5	+ 11 19	3 56 "	10 45.0 "	5 34 "
25	11 10.3	+ 6 50	4 22 "	10 52.2 "	5 23 "
Oct. 5	11 56.2	+ 2 01	4 48 "	10 58.9 "	5 10 "
15	12 42.0	2 57	5 13 "	11 05.1 "	4 57 "
25	13 28.3	− 7 51	5 40 "	11 12.0 "	4 44 "
			MARS.		
Sept. 5	2 13.6	+ 9 29	8 30 P. M.	3 11.8 A. M.	9 53 A. M.
15	2 16.0	+ 9 48	7 52 "	2 34.9 "	9 18
25	2 12.7	+ 9 15	7 10 "	1 52.3	8 35 "
Oct. 5	2 03.9	+ 9 23	6 23 "	1 04.3 "	7 45 "
15	1 51.6	+ 8 49	5 34 "	12 12.7 "	6 51 "
25	1 38.4	+ 8 15	4 44 "	11 20.1 P. M.	5 56 "
			JUPITER.		
Sept. 5	6 12.2	+ 23 03	11 27 P. M.	7 09.8 A. M.	2 53 P. M.
15	6 17.7	+ 23 02	10 53 "	6 35.8 "	2 19 "
25	6 22.0	+ 23 00	10 18 "	6 00.9 "	1 43 "
Oct. 5	6 25.2	+ 22 59	9 42 "	5 24.7 "	1 07 "
15	6 27.1	+ 22 58	9 05	4 47.2 "	12 30 "
25	6 27.5	+ 22 59	8 26 "	4 08.4 "	11 51 "
			SATURN.		
Sept. 5	13 27.8	6 43	8 51 A. M.	2 28.0 P. M.	8 05 P. M.
15	13 31.6	− 7 07	8 17 "	1 52.6	7 28 "
25	13 35.8	7 33	7 44 "	1 17.5 "	6 51 "
Oct. 5	13 40.2	7 59	7 11 "	12 42.6 "	6 14 "
15	13 44.8	− 8 26	6 38 "	12 07.7 "	5 38 "
25	13 49.3	8 52	6 05 "	11 32.9 "	5 01 "

URANUS.

Date. 1894.		R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m.
Sept.	5	14 39.5	− 15 10	10 38 A. M.	3 39.7 P. M.	8 42 P. M.
	15	14 41.2	− 15 18	10 01 "	3 02.0 "	8 03 "
	25	14 43.1	− 15 27	9 24 "	2 24.7 "	7 25 "
Oct.	5	14 45.2	− 15 37	8 47 "	1 47.3 "	6 47 "
	15	14 47.4	− 15 47	8 11 "	1 10.3 "	6 09 "
	25	14 49.8	− 15 58	7 35 "	12 33.3 "	5 32 "

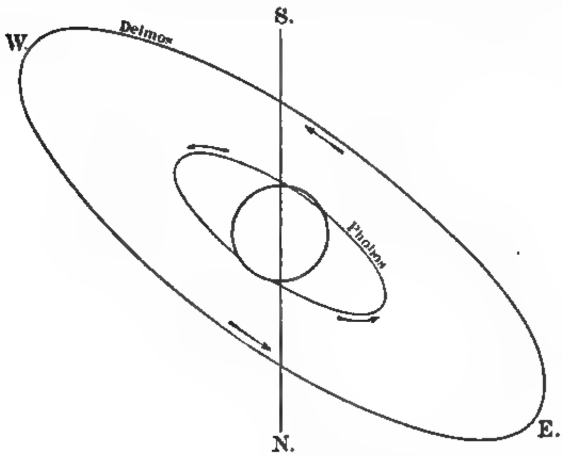
NEPTUNE.

Sept.	5	4 58.9	+ 21 13	10 23 P. M.	5 56.6 A. M.	1 30 P. M.
	15	4 59.0	+ 21 13	9 44 "	5 17.4 "	12 51 "
	25	4 59.0	+ 21 12	9 05 "	4 38.0 "	12 11 "
Oct.	5	4 58.7	+ 21 11	8 25 "	3 58.4 "	11 32 "
	15	4 58.2	+ 21 10	7 45 "	3 18.6 "	10 52 "
	25	4 57.4	+ 21 09	7 05 "	2 38.6 "	10 12 "

THE SUN.

Sept.	5	10 57.9	+ 6 38	5 28 A. M.	11 58.5 A. M.	6 29 P. M.
	15	11 33.8	+ 2 50	5 39 "	11 55.0 "	6 11 "
	25	12 09.7	− 1 03	5 51 "	11 51.5 "	5 52 "
Oct.	5	12 46.0	− 4 50	6 03 "	11 48.3 "	5 33 "
	15	13 22.8	− 8 43	6 16 "	11 45.8 "	5 16 "
	25	14 00.6	− 12 17	6 29 "	11 44.1 "	4 59 "

The Satellites of Mars.



DEIMOS.

Sept.	24	8.3	A. M.	W.
	26	5.7	"	E.
	28	3.1	"	W.
	30	12.5	A. M.	E.
Oct.	1	9.9	P. M.	W.
	3	7.3	"	E.
	5	4.7	"	W.
	7	2.1	"	E.
	9	11.5	A. M.	W.
	11	9.0	"	E.
	13	6.5	"	W.
	15	3.8	"	E.
	17	1.2	"	W.
	18	10.6	P. M.	E.
	20	8.0	"	W.
	22	5.5	"	E.
	24	2.9	"	W.
	26	12.3	"	E.
	28	9.7	A. M.	W.
	30	7.1	"	E.

PHOBOS.

		h					h					h		
Sept.	24	4.6	A. M.	W.	Oct.	6	11.2	"	E.	Oct.	18	5.8	"	W.
	25	7.4	"	E.		7	2.0	P. M.	W.		19	8.6	"	E.
	26	10.2	"	W.		8	4.8	"	E.		20	11.4	"	W.
	27	1.0	P. M.	E.		9	7.6	"	W.		22	2.2	A. M.	E.
	28	3.7	"	W.		10	10.4	"	E.		23	5.0	"	W.
	29	6.5	"	E.		12	1.1	A. M.	W.		24	7.7	"	E.
	30	9.3	"	W.		13	3.9	"	E.		25	10.5	"	W.
Oct.	2	12.1	A. M.	E.		14	6.7	"	W.		26	1.3	P. M.	E.
	3	2.9	"	W.		15	9.5	A. M.	E.		27	4.1	"	W.
	4	5.7	"	E.		16	12.3	P. M.	W.		28	6.9	"	E.
	5	8.4	"	W.		17	3.0	"	E.		29	9.7	"	W.
											31	12.5	A. M.	E.

For Phobos the central time of every seventh eastern and western elongation is given, and for Deimos every third; the intermediate ones may be found by adding the periodic time of each satellite. Periodic time of Phobos $7^h\ 39^m.2$. Periodic time of Deimos $1d\ 6^h\ 17^m.9$

Phases and Aspects of the Moon.

		Central Time. d	h	m
First Quarter	Sept.	6	7	03 P. M.
Apogee		10	2	12 A. M.
Full Moon		14	10	21 P. M.
Last Quarter		22	6	32 A. M.
Perigee		25	11	30 P. M.
New Moon		28	11	44 "
First Quarter	Oct.	6	1	01 "
Apogee		7	6	48 "
Full Moon		14	12	41 "
Last Quarter		21	12	56 "
Perigee		22	7	42 A. M.
New Moon		28	11	57 "

Minima of Variable Stars of the Algol Type.

[Given to the nearest hour in Central Standard Time.]

U CEPHEI.

Alternate Minima

	d	h
Sept.	3	6 P. M.
	8	5 "
	13	5 "
	18	5 "
	23	4 "
	28	4 "
Oct.	3	4 "
	8	3 "
	13	3 "
	18	3 "
	23	2 "
	28	2 "

ALGOL.

Alternate Minima

Sept.	5	4 P. M.

λ TAURI CONT.

	d	h
Oct.	5	1 A. M.
	12	11 P. M.
	20	8 "
	28	6 "

R. CANIS MAJORIS.

Every third minimum

Sept.	3	2 A. M.
	6	noon.
	9	9 P. M.
	13	7 A. M.
	16	5 P. M.
	20	3 A. M.
	23	1 P. M.
	26	10 "
	30	8 A. M.
Oct.	3	6 P. M.
	7	4 A. M.

U OPHIUCHI.

Every fourth Minimum

Sept.	3	10 A. M.
	6	7 P. M.
	10	3 A. M.
	13	noon
	16	8 P. M.
	20	5 A. M.
	23	1 P. M.
	26	10 P. M.
	30	6 A. M.
Oct.	3	3 P. M.
	6	11 "
	10	8 A. M.
	13	4 P. M.
	17	1 A. M.
	20	9 "
	23	6 P. M.
	27	2 A. M.

Occultations Visible at Washington.

Date 1894		Star's Name	Magnitude	IMMERSION Washington M. T.		IMMERSION Angle f'm N p't.	EMERSION Washing-ton M. T.		EMERSION Angle f't N p't.	Duration	
				h	m	°	h	m	°	h	m
Sept.	11	ψ Capricorni	5½	5	48	12	6	27	311	0	39
	13	70 Aquarii	6	6	21	19	7	09	[illegible]	0	48
	13	Lalande 44734	7	9	04	46	10	25	249	1	21
	18	47 Arietis	6	16	36	65	17	56	247	1	20
	19	9 Tauri	7	8	02	10	8	27	307	0	25
	19	23 Tauri	5	11	44	58	12	51	247	1	07
	19	24 Tauri	8	12	19	18	13	27	256	1	08
	19	η Tauri	3	12	21	53	13	31	[illegible]	[illegible]	10
	19	B. A. C. 1171	8	12	59	28	14	10	276	1	11
	19	27 Tauri	4	13	10	105	14	08	200	0	58
	19	28 Tauri	6	13	10	86	14	20	219	1	10
	23	ω¹ Cancri	6	14	05	13	14	13	358	0	08
	23	ω² Cancri	6	14	02	98	15	02	273	1	00
	25	26 Leonis	8	14	03	148	14	42	247	0	39
Oct.	6	B. A. C. 6628	6	10	23	121	11	04	197	0	41
	8	χ Capricorni	5½	12	21	117	12	55	184	0	34
	11	B. A. C. 8184	6	15	20	42	16	23	257	1	03
	13	B. A. C. 221	6	6	24	64	7	29	227	1	05
	13	70 Piscium	8	16	35	54	17	34	254	0	59
	13	ε Piscium	4	16	53	83	17	48	227	0	55
	15	27 Arietis	6	9	03	47	10	09	245	1	06
	16	B. A. C. 1055	7	8	20	101	9	06	204	0	46
	16	66 Arietis	6	10	28	42	11	31	258	1	06
	17	γ Tauri	6	8	06	78	8	56	232	0	50
	19	49 Aurigæ	6	10	37	73	11	34	275	0	57
	19	54 Aurigæ	6	12	26	35	13	20	315	0	44
	19	25 Geminorum	6	13	16	61	14	22	293	1	06
	20	c Geminorum	6	13	10	146	13	51	223	0	41

Maxima and Minima of Variable Stars

[These ephemerides by Dr. Loewy in the "Companion to the Observatory," and by Dr. Hartwig in the "Vierteljahrsschrift der Astronomische Gesellschaft."]

MAXIMA.

Sept.	1	X Libræ.
	2	T Arietis.
	5	U Piscium.
	6	T Geminorum.
	7*	T Delphini.
	8	R Centauri.
	13	V Aurigæ.
	14	Y Capricorni.
	16	U Monocerotis.
	16	T Aquarii.
	18	U Canis Min.
	18†	V Capricorni.
	19	R Lyræ.
	20	S Leonis.
	29	S Carinæ.
	29	R Draconis.
	30	R Canum Ven.
	30	R Camelopardi.
Oct.	1	S Vulpeculæ.
	4	W Libræ.
	4	R Orionis.
	4	S Orionis.
	5	R Reticuli.

MAXIMA CONT.

Oct.	8	S Camelopardi.
	12	T Virginis.
	13	S Aquilæ.
	13	R Arietis.
	14	V Leonis.
	14	W Leonis.
	16	W Herculis.
	17	W Tauri.
	20	R Serpentis.
	21	W Capricorni.
	23	X Bootis.
	23	RR Virginis.
	25	R Scuti.
	26	R Sagittæ.
	26	S Geminorum.
	26	U Virginis.
	28	X Scorpii.
	31	U Monocerotis.

MINIMA.

Sept.	4	R Lyræ.
	4	S Vulpeculæ.
	5	U Orionis.

MINIMA CONT.

Sept.	20	S Libræ.
	20	R Scuti.
	20	R Canis Min.
	22	R Trianguli.
	23	o Ceti.
	29	R Sculptoris.
	29	W Cygni.
Oct.	1	RT Cygni.
	2	R Piscium.
	6	U Bootis.
	10	X Capricorni.
	10	R Sagittæ.
	13	U Monocerotis.
	20	R Lyræ.
	22	R Vulpeculæ.
	22	R Persei.
	24	S Bootis.
	24	R Andromedæ.
	26	U Bootis.
	27	R Virginis.
	30	Z Cygni.

* The "Companion to the Observatory" gives this as Sept. 9.

† The "Companion to the Observatory" gives this as Sept. 22.

A Partial Eclipse of the Moon will occur on the night of Sept. 14, 1894. It will be visible throughout North and South America. The beginning will be visible in the western part of Europe and Africa. The accompanying diagram will give the reader some idea of the Moon's course as it passes by the Earth's shadow. The large shaded circle represents a cross-section of the Earth's shadow and the small circles represent the Moon at first and last contacts and middle of eclipse. The Moon will pass by the lower edge of the shadow, touching it first at the southernmost point. The observer will therefore see the shadow first at the north point of the Moon's disk. As the Moon moves up toward the left the

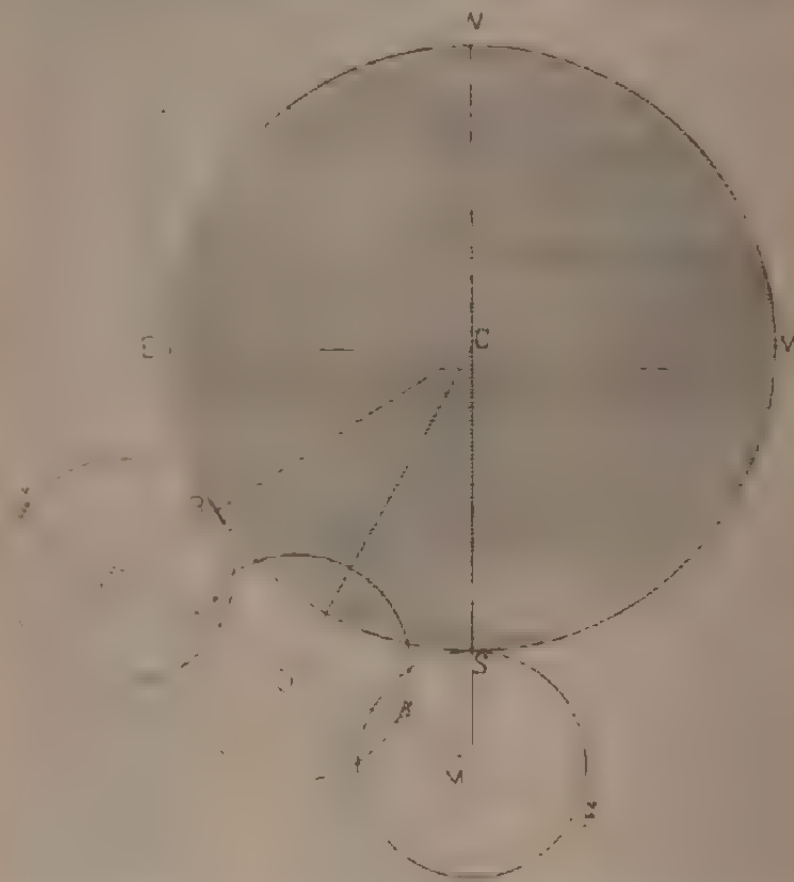


DIAGRAM SHOWING THE COURSE OF THE MOON BY THE EARTH'S SHADOW DURING THE PARTIAL ECLIPSE, SEPT. 14, 1894

shadow will appear to move down toward the right, covering at the middle of the eclipse a little less than a quarter of the diameter of the Moon's disk, and leaving it at a point 58° to the west from the north point. The first contact will occur at $9^h\ 36^m$ central standard time, the Moon's center being then at the point M. Before this a faint shading, due to the penumbra of the Earth's shadow,

will have been noticed on the upper part of the disk. At $10^h\ 32^m$ the Moon will be at O, and the eclipse at its maximum. At $11^h\ 28^m$ the Moon will be at P leaving the shadow at R. After that there will be only the faint penumbral shading on the west side of the disk.

Elements of the Eclipse.

Greenwich mean time of conjunction in right ascension Sept. 14, $15^h\ 35^m\ 42^s.8$.

Sun's right ascension	$11^h\ 31^m\ 36^s.20$	Hourly motion	$8^s.97$
Moon's right ascension	23 31 36 .20	Hourly motion	109 .98
Sun's declination	$3°\ 04'\ 10''.0$ N	Hourly motion	$0'\ 57''.8$ S
Moon's declination	3 59 33 .5 S	Hourly motion	14 52 .6 N
Sun's equa. hor. parallax	8 .5	Sun's true semi diameter	15 54 .9
Moon's equa. hor. "	55 24 .1	Moon's " "	15 05 .6

Times of the Phases.

		h m	
Moon enters penumbra	Sept. 14	7 58.6 P. M.	Central Standard Time.
Moon enters shadow		9 35.6 "	
Middle of eclipse		10 31.6 "	
Moon leaves shadow		11 27.7 "	
Moon leaves penumbra	Sept. 15	1 04.4 A. M.	

A Total Eclipse of the Sun will occur Sept. 28, 1894. It will be invisible in America. The path of totality passes across the Indian Ocean as shown in the accompanying cut. The eclipse will be partial in Africa, Persia, Hindostan and

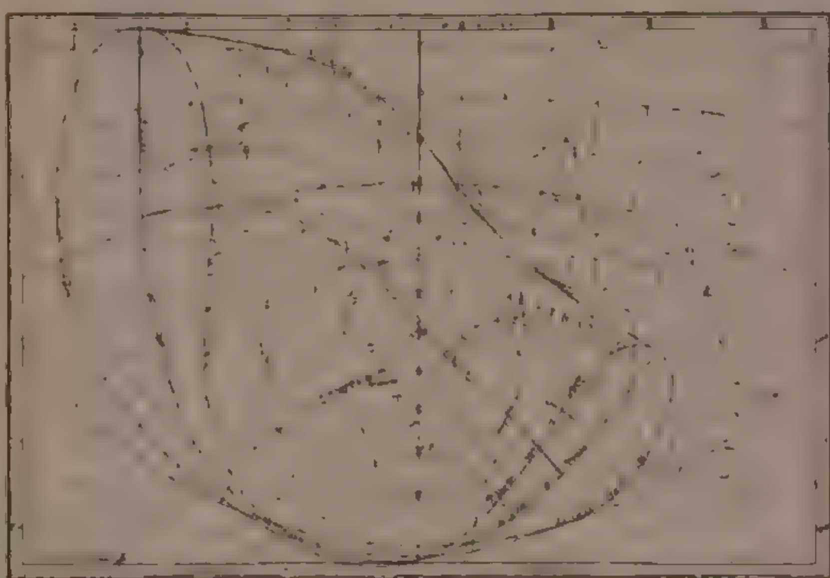


Chart of the Total Eclipse of the Sun Sept. 28, 1894.

southern Australia. The times marked on the chart are expressed in Greenwich mean time.

CIRCUMSTANCES OF THE ECLIPSE.

		Greenwich M. T.		Longitude from Greenwich		Latitude	
		h	m	°	′	°	′
Eclipse begins............	Sept. 28	15	01.0	42	50.7 E.	11	49.2 N.
Central eclipse begins.		16	03.7	26	41.3 E.	1	47.1 N.
Eclipse at noon		18	06.2	86	01.3 E.	[illegible]	11.6 S.
Central eclipse ends		19	14.1	162	43.3 E.	56	24.9 S.
Eclipse ends................		20	17.0	145	34.0 E.	16	24.1 S.

NEWS AND NOTES.

It is our custom to send not more than one number of this magazine after subscriptions have expired unless notified by subscribers that the publication should be continued. Usually we have informed our patrons by letter of the time when renewals should be made.

Hereafter it is the intention of the publisher to mail this periodical on or about the 25th of the month preceding the one for which it is dated instead of the last day of the month as heretofore. This change is made to accommodate foreign subscribers whose patronage is increasingly large, and whose wishes are therefore correspondingly important. Contributors are respectfully asked to bear this in mind, and to send in articles not later than the 12th and minor notices on or before the 18th of the month of issue.

Professor George E. Hale writes from Palermo under date of June 20 as follows:

"I am delayed here on account of snow on Mt. Etna which has hitherto prevented the pack animals from reaching the Observatory. Within a week, however, we hope to make the ascent, though we may be still further delayed by some apparatus in transit from Germany. We have been in southern Italy and Sicily about three weeks, but during the whole time we have not seen the sky as blue as it frequently is in Chicago. The season is said to be a very exceptional one, and I much fear that even the altitude of Etna will not be sufficient to take us out of the mist which seems to enshroud everything."

It will be remembered from previous notices, that one object of Professor Hale's visit to Etna at this time is to study the Sun's corona without an eclipse by the aid of photography if possible.

C. M. Charroppin, S. J., writes us from Corozal, British Honduras, under date of June 20, that "Mars is now in splendid position, being high in the heavens; but, my telescope is entirely too small for useful observations. I am at present negotiating with the states for a six-inch. I hope to get it before the time of the transit of Mercury. I was very much amused to receive a ——————— paper (from the states) with the following notice: 'Fr. Charroppin has gone to British Honduras to observe the transit of Venus.' He will be the only astronomer in that island to observe this rare phenomenon.' It is pardonable for an editor to call the transit of Mercury the transit of Venus; but to call British Honduras an island is a blunder that ought to make the paper a back number."

Scheiner's Spectral Analyse der Gestirne.—Early in June the publishers (Messrs.

Ginn & Co.) of Frost's translation of Scheiner's *Spectral Analyse der Gestirne* announced that more time was needed for the publication of the book than was anticipated, but it was then thought that it would be issued before the end of June. The work has not yet appeared so far as we know, but doubtless it will soon be ready for the market. We have not the least doubt but that the book will be benefited by the delay, for such publications cannot be hurried very safely.

Auroræ and Solar Prominences.—Upon the night of June 6th, when the eruptive prominences which are described as having been seen on June 7th at Goodsell Observatory, were exactly on the Sun's limb there was an aurora with streamers. Upon June 9th there was another prominent aurora—another section of this same area of disturbance being at the limb at that time also. These displays were best seen apparently in Canada. Further details will be had when reports for the month are at hand.

It is of interest to note that June 7th was the 17th day of the synodic period as I have them arranged, this being the date of a long and very prominent series of auroras and their clusters, which has been the most persistent of any now in progress at the 27¼ day interval.

M. A. VEEDER.

The Pulkova Catalogue for 1885.—They who have read the reports and other publications of the Pulkova Observatory are aware that, besides the standard catalogue for 1845 mentioned in Professor Eastman's address, there is also one for 1865 which is the basis of the present Jahrbuch ephemerides. This is far more accurate than the former, in right ascension because the chronographic method was added to the eye-and-ear method, in declination because of more complete observations according to the programme. A later one for 1885 is now in progress and the declinations are published.

They are still more completely observed and discussed. The instrument is the same as before, a vertical circle. But it has been re-divided by the Repsolds. The same method is used; that is every observation is double, the instrument was rotated on its vertical axis after taking one altitude, and another taken in reversed position. The level is of course used to indicate the zero point.

The variation of the latitude is observed and allowed; the refractions are re-discussed allowing for the difference between exterior and interior temperatures.

The final result is to indicate that the declinations for 1865 brought up with the proper motions from Auwers and Bradley are very nearly in accordance with those for 1885, the average difference being less than 0.05. The new Pulkova declinations then are a most important and welcome confirmation of those given in the Berlin Jahrbuch.

T. H. SAFFORD.

Williamston, July 5, 1894.

A Device for Securing a Mercury Surface Undisturbed by Earth Tremors.—In making observations by reflection from a mercury horizon, the observer is frequently troubled on account of agitation of the mercury. This is especially troublesome in the neighborhood of a city or railroad. On the pier of the transit circle of the University of Minnesota the disturbance is so great and so constant, that it is impossible to see the reflected images of the wires until late at night. Even then they are seen with great indistinctness. To overcome this difficulty Mr. Saegmuller sent with the transit circle a bunch of felt cloth and a copper disk. His instructions were to put the felt cloth in the mercury cup, place the

disk upon it and fill the slightly concave surface of the disk with mercury. A trial of this method showed the images to be somewhat improved, but they were still seen with great difficulty. However, when the felt cloth was removed and the disk floated on the mercury, the reflecting surface upon the disk was found to be absolutely quiet. The images of the wires came out with perfect distinctness at any time and under any ordinary condition of disturbance. During the passing of a railroad train several hundred yards distant, a slight oscillatory motion of the images was noticed.

The success of this method is due to two causes. First, since the depth of mercury on the disk is slight, the attraction of the copper tends to prevent surface waves. Second, because the disk floats in mercury, the outside surface waves merely break upon the sides of the disk without imparting their motion to it.

F. P. LEAVENWORTH.

University of Minnesota, July 9, 1894.

Nova Aurigæ.—On the mornings of July 12 and 14 I examined this object with the 12-inch.

It has not changed in brightness from the last observations in the spring.

It is apparently the slightest bit brighter than the star which Mr. Burnham has called F in his early measures of the Nova, and which is in R. A. 32ˢ and distance 85″.

E. E. BARNARD.

Mt. Hamilton, July 14, 1894.

Temple's Periodic Comet.—This faint comet is now being observed with the 12-inch equatorial. It is extremely faint with that instrument. About $\frac{1}{2}'$ in diameter with scarcely any sensible brightening in the middle.

It promises to be observable for some time yet.

E. E. BARNARD.

Mt. Hamilton, July 14, 1894.

In my paper on the Proper Motion of the Stars in the Dumb-Bell Nebula, ASTRONOMY AND ASTRO-PHYSICS for June, I desire to make the following correction on p. 447.

For the star *d* in the $\Delta\delta$ for

$$\begin{array}{r} +\ 15''.79 \\ +\ 16\ \ .27 \\ \hline +\ 16''.37 \end{array}$$

read

$$\begin{array}{r} +\ 15''.81 \\ +\ 16\ \ .37 \\ \hline +\ 16''.10 \end{array}$$

This gives for the comparison with Struve in $\Delta\delta$ for *d*

		Diff. from Struve.
Struve	+ 12″.8	
Wilson	+ 16 .6	− 3″.8
Barnard	+ 16 .1	− 3. 3

Differing from the value in June A. AND A.-P. by 0″.3

E. E. BARNARD.

Photometric Catalogues of the Harvard College Observatory.—In *Astronomische Nachrichten*, Vol. 134, p. 355, S. C. Chandler severely criticises the photometric observations which have been published in some of the catalogues of Harvard

College Observatory. E. C. Pickering, the director of the above named Observatory answers these criticisms in *A. N.*, Vol. 135, p. 220. In the article referred to Mr. Chandler claims that upon examination "It soon became manifest that there were numerous incongruities in the observations given" in Vol. XXIV of the Harvard College Observatory Annals, so much so, as to leave upon him, "an impression of distrust whether any of these observations are suitable for any precise or critical purpose." Then follows fifteen citations of error in support of this lively distrust, all of which are variable stars, ranging at their minima, from the 9th to the 14th magnitude in brightness. The hypothesis which Mr. Chandler assumes as reasonable for the cause of these errors, is generally the observation of wrong stars.

He next says "still stronger presumption exists of a similar defect in the observations for the Photometric Catalogue of bright stars in Vol. XIV, where the discordances are yet more startling and numerous and have puzzled astronomers who have had occasion to examine those results critically." In support of this sweeping "presumption" three observations of one star are given, and he adds in the same paragraph that "this object is a faint telescopic star of 9.5 magnitude which must have crept into the the working-list from failure to notice the correction BB. VI, p. 378. But to go into the errors in the Photometric Catalogue lies outside of the intended scope of this note."

Director Pickering's answer to this attack is explicit, courteous and dignified. He says the only error pointed out in Vol. XIV had been detected at Harvard Observatory, and an explanation forwarded months ago to the superintendent of the British Nautical Almanac. These observations were among the first undertaken, and the errors were due to errors in other catalogues.

Fifteen instances of error are also pointed out in a total list of eighty-six variable stars whose observations are printed in Vol. XXIV, and Mr. Chandler's assumption is that similar errors exist throughout the entire Catalogue of over 20,000 stars contained in the same volume. Of these fifteen stars Professor Pickering says the original records show that eight places were wrong because of wrong reduction originally, and that the plan of observation afforded a means of satisfactory identification of the right stars in each case. He points out three errors that Mr. Chandler made in trying to locate the stars he observed and to give the right magnitude. Two other errors Professor Pickering says were made with the large meridian photometer when it was first employed. The original record of the observation of another star observed in 1888 is missing. In the case of two others the mis-identified stars are so close to the positions of the stars named that the magnifying power used on the photometer would not make the difference in position perceptible.

Director Pickering also claims that it can not be assumed that the observations of variable stars of long period are wrong because their measured magnitudes do not agree with those found by prediction. For o Ceti has been observed more than four centuries, yet its time for maximum, this year, as predicted by Mr. Chandler was Feb. 17, but the maximum really came more than one month later than the prediction. It is also a well known fact that the different maxima of this star vary by as much as three whole magnitudes. Criticisms on the errors of magnitudes in long period variable stars is not very safe business in the present state of progress of variable star astronomy. In the light of Professor Pickering's answer it seems to us that Mr. Chandler has greatly exaggerated his case whether he is conscious of it or not (think he is not), he has unjustly and wrongfully berated reasonably good work. Such an arraignment is not a high-minded benefaction to science. It can not be esteemed an enviable honor for any

of its truth-loving votaries to wear. We do not wonder that Dr. Gould should feelingly disclaim any share in the onus of such a thing in the minds of people generally interested. On the other hand how different are the newspaper mouthings of a few science fledglings at the "Hub" who pose as judges supreme on the merits or demerits of "Old Harvard." Judged by the true scientific spirit the two views are as far apart as the east is from the west.

The Radiant of the August Perseids.—Mr. Monck's repeated attacks on the motion of the Perseid radiant point remind me forcibly of the late John Hampden's assaults on the rotundity of the Earth. The latter gentleman, it is true, never succeeded in making the globe flat, for it apparently still retains its spherical shape, nor will all Mr. Monck's endeavors alter the behavior of the Perseid radiant by bringing it to a standstill, for in the ordinary routine of Nature it will go on moving much the same as at present until the end of time!

This motion is amply proved both by observation and theory. Mr. Monck avers there is no motion though he has never investigated the feature either in its observational or mathematical bearings. Being unacquainted with either the one or the other he is obviously not qualified to speak upon the matter at all, for, to be perfectly frank with him, he cannot know what he is talking about. He is trying to raise a superstructure without putting in any foundation, and has assumed the position of an authority without the research and experimental knowledge necessary to merit that title.

In spite of the good advice that has been previously tendered to him Mr. Monck is likely to go on hammering away at the Perseid radiant for a long time to come and with precisely the same negative result as hitherto. His frequent expressions, such as "I think," "It appears to me," "I believe," etc., etc., show that he rests his argument on mere supposition and unsupported ideas and that there is no reliable groundwork whatever for his puerile criticism. Let him attempt by patient labor to accumulate facts and all the evidence possible bearing upon the subject and then he will be entitled to attention, but his present posture is certainly as ineffective as it is ridiculous.

The motion of the Perseid radiant can be observed as a conspicuous feature in July and August of every year by any one who will take the trouble to watch the paths of the meteors and record them accurately. Mr. Monck could readily witness it for himself were his observational powers up to mediocrity, but most unfortunately they are not and he has fallen into the error of supposing that mere cavilling can supply the place of practical investigation.

While, during the last quarter of a century or so, Mr. Monck has been snugly located in his feather bed and utterly oblivious as to what has been going on in the heavens, some of us have been out of doors all night watching the fall of meteors and often enough shivering with the cold. It can be fairly said of the observations which have accumulated that they were conducted with all the care and pleasurable interest such as only an intense love for the subject could inspire. The results have been published and in respect to the mobile radiation of the Perseids theory has demonstrated its perfect consistency and cut away the ground from under the feet of skeptics. But it is really quite unnecessary to recapitulate details or to enter into lengthy argument in any vain attempt to force the truth upon one to whom it appears to be so singularly indigestible.

Bristol, 1894, May 17. W. F. DENNING.

Mathematics and Astronomy in the University of Chicago.—But for lack of space we would gladly publish the entire programme of the departments of Mathematics and Astronomy now in use at the University of Chicago. It is an excellent one and it will certainly be liberally patronized. For the immediate information of those interested we give the

CONSPECTUS OF COURSES—1894-5

SUMMER	*AUTUMN*	*WINTER*	*SPRING*
25 Gauss's Method (See) DM	1 Astronomical Photography (Hale) DM	2 Solar Physics (Hale) 2DM	3 Solar Physics continued (Hale) 2DM
26 Theory of Attractions (See) DM	4 Stellar Spectroscopy (Hale) DM	35 Theory of [illegible] (See) DM	4 Astro-Physical Research (Hale)
27 General Astronomy (Laves) DM	30 Theory of Tides (See) DM	36 General Astronomy (See) DM	40 Lunar Theory and Cosmogony (See) DM
28 Latitude and Longitude (Laves) DM	31 Gravitation (See) DM	37 Dynamics of a System (Laves) DM	41 General Astronomy (continued) (See) DM
29 Seminar (See and Laves)	32 Partial Differential Equations (Laves) DM	38 Spherical Astronomy Part II (Laves) DM	42 [illegible] (Laves) DM
	34 Spherical and Practical Astronomy Part I (Laves) DM	44 Seminar (See and Laves)	43 Special Perturbations (Laves) DM
	44 Seminar (See and Laves)		44 Seminar (See and Laves)

United States Naval Observatory.—On the 9th of June, Senator Morrill of Vermont offered an amendment to the bill before the Senate of the United States making appropriations for the naval service and for other purposes. That amendment was as follows:

That from and after the passage of this act the Superintendent of the United States Naval Observatory shall be a person selected from civil life learned in the science of astronomy, to be appointed by the President, by and with the advice and consent of the Senate, and shall receive an annual salary of five thousand dollars; he may also occupy the dwelling house near the Observatory free of rent. The Superintendent aforesaid is hereby authorized and directed, with the approval of the Secretary of the Navy, to reorganize said Observatory establishment and to make such regulations as may be expedient in relation to the Observatory and its subordinate officers, professors and other employés: *Provided*, That after January first, eighteen hundred and ninety-five, the total salaries and annual expenditures shall be adjusted to a basis of not exceeding fifty thousand dollars per annum.

On June 11 Mr. Morrill supported this amendment by a speech about ten minutes long setting forth in a clear, able, and most convincing manner the desirability of the changes proposed. He also asked that certain important papers relating to the amendment be printed to accompany it. These papers were:

1. Budget of the Royal Observatory, Greenwich, with corresponding appropriations for the support of the Naval Observatory, Washington.
2. Statement of ordinary work of the Observatory.
3. Letter from Professor Asaph Hall, in 1892, to Senator Hale of Maine.
4. Letters from S. C. Chandler, C. A. Young, B. A. Gould, Lewis Boss.
5. Extract from the Report of the Secretary of the Navy for 1892, p. 57, Naval Observatory.
6. Royal Astronomical Society of London.

Mr. Morrill's measure was favorably and unanimously reported upon by the Senate Committee on Naval affairs, and the amendment was inserted in the Leg-

islative bill by the Senate committee on appropriations. While the bill was pending in the Senate the committee consulted the Secretary of the Navy who suggested some changes. When the measure was put into shape, it proved to be one leaving the Naval Superintendent in charge, and adding to the organization a "Director of Astronomy" to have charge of the Astronomical work and to control the astronomers, assistant astronomers and computers, but nothing further. It was however, put in by the Senate, and went back to the House. Here Mr. Reed of Maine, made a strong speech against the measure because it left the naval officers in control (See Cong. Rec., July 20). July 24, a conference committee refused to adopt the Senate measure, and so the whole matter is postponed until next session. The friends of the Morrill measure will doubtless give this matter early attention at the next session of Congress.

The Chicago Academy of Sciences.—*Section of Astronomy and Mathematics. June 11.*—The regular monthly meeting was held in the apartments of the Commerce Club, Auditorium Building, Professor G. W. Hough, President, in the chair.

Professor S. W. Burnham read the first paper of the evening on "*Astronomical Photography.*" The speaker exhibited on the screen a series of the finest recent astronomical photographs, and discussed at some length the difficulties and limitations of modern photography as applied to the heavenly bodies. The photographs projected with the lantern included the celebrated photographs of the Milky Way by Professor Barnard and Dr. Max Wolf, pictures of the Orion, Andromeda and other nebulæ, also photographs of several clusters, including the Pleiades, and especially Omega Centauri, as taken by Gill and Pickering. Professor Burnham then discussed the application of photography to comets, and exhibited Professor's Barnard's fine photographs of comet Swift, and his very recent photographs of comet Gale. It was remarked that photography gave the only means of obtaining an unprejudiced record of the appearance of comets, and the sudden changes which their tails undergo from day to day. At the conclusion of Professor Burnham's paper, a general discussion followed, in which all the resident astronomers took part. Dr. T. J. J. See read the second paper of the evening on "*The determination of the relative motion of the companion of a Binary star in the line of sight, and on an approximate method of finding the eccentricities of the orbits of Spectroscopic Binaries.*" The speaker referred to his paper in the ASTRONOMY AND ASTRO-PHYSICS for November, 1893, in which he gave approximate formulæ for the motion in the line of sight when the orbit is not very eccentric; he then gave the rigorous formulæ for the motion in the line of sight whatever be the eccentricity. Here he supposed the elements to be known from micrometrical measures. The converse problem of finding the elements from the observed motion in the line of sight was then taken up, and the speaker showed how this problem could be solved by successive approximations. The study of the velocity curves would give the principal elements and especially the eccentricity. Dr. See referred to the important work of Pickering and Vogel on Spectroscopic Binaries, and expressed the hope that they would deduce the elements of the systems already discovered. He said that the study of these rapid systems was of great importance, as it might in a few years reveal sensible changes in the period due to the working of tidal friction. The speaker also pointed out the high importance of applying the spectroscope to the variable stars, for the purpose of deciding whether the changes are due to revolving bodies which are highly inclined upon our visual ray, as is certainly the case with variables of the Algol type.

In the discussion, Professor Hough remarked that while the theory advanced in the paper was sound, it would be difficult to actually apply it in the present state of astronomy, owing to the great delicacy required in the observations. After some further remarks the section adjourned. *G.*

Disk of Jupiter's First Satellite.—The table on page 124 of our June issue is in error. In the fourth column, near the bottom, D should be inserted between the two P's, causing all the letters above it to appear one line higher. That will credit Professor Pickering (P) with observations 1, 3, 5, 7, 9, and 11 as appears in the proof he read. The mistake was made by the carelessness of the printer.

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These books can be bought of Messrs. Wm. Wesley & Son, Essex St., Strand London, England

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CONTENTS FOR JUNE 1894.

PLATE XIX.

SCHIAPARELLI'S MAP OF MARS, 1888.

Astronomy and Astro-Physics.

VOL. XIII, No. 8. OCTOBER, 1894 WHOLE No 128.

General Astronomy.

ON THE MAGNITUDE OF THE SOLAR SYSTEM.*

WM. HARKNESS

Nature may be studied in two widely different ways. On the one hand we may employ a powerful microscope which will render visible the minutest forms and limit our field of view to an infinitesimal fraction of an inch situated within a foot of our own noses, or on the other hand, we may occupy some commanding position, and from thence, aided perhaps by a telescope, we may obtain a comprehensive view of an extensive region. The first method is that of the specialist, the second is that of the philosopher, but both are necessary for an adequate understanding of nature. The one has brought us knowledge wherewith to defend ourselves against bacteria and microbes which are among the most deadly enemies of mankind, and the other has made us acquainted with the great laws of matter and force upon which rests the whole fabric of science. All nature is one, but for convenience of classification we have divided our knowledge into a number of sciences which we usually regard as quite distinct from each other. Along certain lines, or more properly, in certain regions, these sciences necessarily abut on each other, and just there lies the weakness of the specialist. He is like a wayfarer who always finds obstacles in crossing the boundaries between two countries, while to the traveler who gazes over them from a commanding eminence the case is quite different. If the boundary is an ocean shore there is no mistaking it; if a broad river or a chain of mountains it is still distinct, but if only a line of posts traced over hill and dale, then it becomes lost in the natural features of the landscape, and the essential unity of the whole region is apparent. In that case the border land is wholly a human conception of which nature takes no cognizance, and so it is with the scientific border land to which I propose to invite your attention this evening.

*Communicated by the author; being the presidential address delivered before the American Association for the advancement of Science, at its Brooklyn meeting, August 16, 1894.

To the popular mind there are no two sciences further apart than astronomy and geology. The one treats of the structure and mineral constitution of our earth, the causes of its physical features and its history, while the other treats of the celestial bodies, their magnitudes, motions, distances, periods of revolution, eclipses, order, and of the causes of their various phenomena. And yet many, perhaps I may even say most, of the apparent motions of the heavenly bodies are merely reflections of the motions of the Earth, and in studying them we are really studying it. Furthermore, precession, nutation and the phenomena of the tides depend largely upon the internal structure of the Earth, and there astronomy and geology merge into each other. Nevertheless, the methods of the two sciences are widely different, most astronomical problems being discussed quantitatively by means of rigid mathematical formulæ, while in the vast majority of cases the geological ones are discussed only qualitatively, each author contenting himself with a mere statement of what he thinks. With precise data the methods of astronomy lead to very exact results, for mathematics is a mill which grinds exceeding fine; but after all, what comes out of a mill depends wholly upon what is put into it, and if the data are uncertain, as is the case in most cosmological problems, there is little to choose between the mathematics of the astronomer and the guesses of the geologist.

If we examine the addresses delivered by former presidents of this Association, and of the sister—perhaps it would be nearer the truth to say the parent Association, on the other side of the Atlantic, we shall find that they have generally dealt either with the recent advances in some broad field of science, or else with the development of some special subject. This evening I propose to adopt the latter course, and I shall invite your attention to the present condition of our knowledge respecting the magnitude of the solar system, but in so doing it will be necessary to introduce some considerations derived from laboratory experiments upon the luminiferous ether, others derived from experiments upon ponderable matter, and still others relating both to the surface phenomena and to the internal structure of the earth, and thus we shall deal largely with the border land where astronomy, physics and geology merge into each other.

The relative distances of the various bodies which compose the solar system can be determined to a considerable degree of approximation with very crude instruments as soon as the true plan of the system becomes known, and that plan was taught by

Pythagoras more than five hundred years before Christ. It must have been known to the Egyptians and Chaldeans still earlier, if Pythagoras really acquired his knowledge of astronomy from them, as is affirmed by some of the ancient writers, but on that point there is no certainty. In public Pythagoras seemingly accepted the current belief of his time, which made the Earth the center of the universe, but to his own chosen disciples he communicated the true doctrine that the Sun occupies the center of the solar system and that the Earth is only one of the planets revolving around it. Like all the world's greatest sages he seems to have taught only orally. A century elapsed before his doctrines were reduced to writing by Philolaus of Crotona, and it was still later before they were taught in public for the first time by Hicetas, or as he is sometimes called Nicetas, of Syracuse. Then the familiar cry of impiety was raised and the Pythagorean system was eventually suppressed by that now called the Ptolemaic, which held the field until it was overthrown by Copernicus almost two thousand years later. Pliny tells us that Pythagoras believed the distances to the Sun and Moon to be respectively 252,000 and 12,600 stadia, or, taking the stadium at 625 feet, 29,837 and 1,492 English miles; but there is no record of the method by which these numbers were ascertained.

After the relative distances of the various planets are known, it only remains to determine the scale of the system, for which purpose the distance between any two planets suffices. We know little about the early history of the subject, but it is clear that the primitive astronomers must have found the quantities to be measured too small for detection with their instruments, and even in modern times the problem has proved to be an extremely difficult one. Aristarchus of Samos, who flourished about 270 B. C., seems to have been the first to attack it in a scientific manner. Stated in modern language his reasoning was that when the Moon is exactly half full the Earth and Sun, as seen from its centre, must make a right angle with each other, and by measuring the angle between the Sun and Moon, as seen from the Earth at that instant, all the angles of the triangle joining the Earth, Sun and Moon would become known, and thus the ratio of the distance of the Sun to the distance of the Moon would be determined. Although perfectly correct in theory, the difficulty of deciding visually upon the exact instant when the Moon is half full is so great that it cannot be accurately done, even with the most powerful telescopes. Of course Aristarchus had no telescope, and he does not explain how he effected the observation, but his con-

clusion was that at the instant in question the distance between the centers of the Sun and Moon as seen from the Earth, is less than a right angle by 1-30th part of the same. We should now express this by saying that the angle is 87 degrees, but Aristarchus knew nothing of trigonometry, and in order to solve his triangle he had recourse to an ingenious, but long and cumbersome geometrical process which has come down to us and affords conclusive proof of the condition of Greek mathematics at that time. His conclusion was that the Sun is nineteen times further from the Earth than the Moon, and if we combine that result with the modern value of the Moon's parallax, viz., 3,422.38 seconds, we obtain for the solar parallax 180 seconds, which is more than twenty times too great.

The only other method of determining the solar parallax known to the ancients was that devised by Hipparchus about 150 B. C. It was based on measuring the rate of decrease of the diameter of the Earth's shadow cone by noting the duration of lunar eclipses, and as the result deduced from it happened to be nearly the same as that found by Aristarchus, substantially his value of the parallax remained in vogue for nearly two thousand years, and the discovery of the telescope was required to reveal its erroneous character. Doubtless this persistency was due to the extreme minuteness of the true parallax, which we now know is far too small to have been visible upon the ancient instruments, and thus the supposed measures of it were really nothing but measures of their inaccuracy.

The telescope was first pointed to the heavens by Galileo in 1609, but it needed a micrometer to convert it into an accurate measuring instrument, and that did not come into being until 1639, when it was invented by William Gascoigne. After his death, in 1644, his original instrument passed to Richard Townley, who attached it to a fourteen-foot telescope at his residence in Townley, Lancashire, England, where it was used by Flamsteed in observing the diurnal parallax of Mars during its opposition in 1672. A description of Gascoigne's micrometer was published in the Philosophical Transactions in 1667, and a little before that a similar instrument had been invented by Auzout, in France, but observatories were fewer then than now, and so far as I know J. D. Cassini was the only person beside Flamsteed who attempted to determine the solar parallax from that opposition of Mars. Foreseeing the importance of the opportunity, he had Richer dispatched to Cayenne some months previously, and when the opposition came he effected two determinations of the

parallax; one being by the diurnal method, from his own observations in Paris, and the other by the meridian method, from observations in France by himself, Römer and Picard, combined with those of Richer at Cayenne. This was the transition from the ancient instruments with open sights to telescopes armed with micrometers, and the result must have been little short of stunning to the seventeenth century astronomers, for it caused the hoary and gigantic parallax of about 180 seconds to shrink incontinently to 10 seconds, and thus expanded their conception of the solar system to something like its true dimensions. More than fifty years previously Kepler had argued from his ideas of the celestial harmonies that the solar parallax could not exceed 60 seconds, and a little later Horrocks had shown on more scientific grounds that it was probably as small as 14 seconds, but the final death blow to the ancient values ranging as high as two or three minutes came from these observations of Mars by Flamsteed, Cassini and Richer.

Of course the results obtained in 1672 produced a keen desire on the part of astronomers for further evidence respecting the true value of the parallax, and as Mars comes into a favorable position for such investigations only at intervals of about sixteen years, they had recourse to observations of Mercury and Venus. In 1677 Halley observed the diurnal parallax of Mercury, and also a transit of that planet across the Sun's disk, at St. Helena, and in 1681 J. D. Cassini and Picard observed Venus when she was on the same parallel with the Sun, but although the observations of Venus gave better results than those of Mercury, neither of them was conclusive, and we now know that such methods are inaccurate even with the powerful instruments of the present day. Nevertheless Halley's attempt by means of the transit of Mercury ultimately bore fruit in the shape of his celebrated paper of 1716, wherein he showed the peculiar advantages of transits of Venus for determining the solar parallax. The idea of utilizing such transits for this purpose seems to have been vaguely conceived by James Gregory, or perhaps even by Horrocks, but Halley was the first to work it out completely, and long after his death his paper was mainly instrumental in inducing the governments of Europe to undertake the observations of the transits of Venus in 1761 and 1769, from which our first accurate knowledge of the Sun's distance was obtained

Those who are not familiar with practical astronomy may wonder why the solar parallax can be got from Mars and Venus, but not from Mercury or the Sun itself. The explanation de-

pends upon two facts. Firstly, the nearest approach of these bodies to the Earth is for Mars 33,874,000 miles, for Venus 23,-654,000 miles, for Mercury 47,935,000 miles, and for the Sun 91,239,000 miles. Consequently, for us Mars and Venus have very much larger parallaxes than Mercury or the Sun, and of course the larger the parallax the easier it is to measure. Secondly even the largest of these parallaxes must be determined within far less than one-tenth of a second of the truth, and while that degree of accuracy is possible in measuring short arcs, it is quite unattainable in long ones. Hence one of the most essential conditions for the successful measurement of parallaxes is that we shall be able to compare the place of the near body with that of a more distant one situated in the same region of the sky. In the case of Mars that can always be done by making use of a neighboring star, but when Venus is near the Earth she is also so close to the Sun that stars are not available, and consequently her parallax can be satisfactorily measured only when her position can be accurately referred to that of the Sun, or in other words, only during her transits across the Sun's disc. But even when the two bodies to be compared are sufficiently near each other, we are still embarrassed by the fact that it is more difficult to measure the distance between the limb of a planet and a star or the limb of the Sun than it is to measure the distance between two stars, and since the discovery of so many asteroids that circumstance has led to their use for the determination of the solar parallax. Some of these bodies approach within 75,230,000 miles of the Earth's orbit, and as they look precisely like stars, the increased accuracy of pointing on them fully makes up for their greater distance as compared with Mars or Venus.

After the Copernican system of the world and the Newtonian theory of gravitation were accepted it soon became evident that trigonometrical measurements of the solar parallax might be supplemented by determinations based on the theory of gravitation, and the first attempts in that direction were made by Machin in 1729 and T. Mayer in 1753. The measurement of the velocity of light between points on the Earth's surface, first effected by Fizeau in 1849, opened up still other possibilites, and thus for determining the solar parallax we have at our command no less than three entirely distinct classes of methods, which are known respectively as the trigonometrical, the gravitational and the photo-tachymetrical. We have already given a summary sketch of the trigonometrical methods as applied by the ancient astronomers to the dichotomy and shadow cone of the Moon,

and by the moderns to Venus, Mars and the asteroids, and we shall next glance briefly at the gravitational and photo-tachymetrical methods.

The gravitational results which enter directly or indirectly into the solar parallax are six in number, to wit: first, the relation of the Moon's mass to the tides; second, the relation of the Moon's mass and parallax to the force of gravity at the Earth's surface; third, the relation of the solar parallax to the masses of the Earth and Moon; fourth, the relation of the solar and lunar parallaxes to the Moon's mass and parallactic inequality; fifth, the relation of the solar and lunar parallaxes to the Moon's mass and the Earth's lunar inequality; sixth, the relation of the constants of nutation and precession to the Moon's parallax.

Respecting the first of these relations it is to be remarked that the tide producing forces are the attractions of the Sun and Moon upon the waters of the ocean, and from the ratio of these attractions the Moon's mass can readily be determined. But unfortunately the ratio of the solar tides to the lunar tides is affected both by the depth of the sea and by the character of the channels through which the water flows, and for that reason the observed ratio of these tides requires multiplication by a correcting factor in order to convert it into the ratio of the forces. The matter is further complicated by this correcting factor varying from port to port, and in order to get satisfactory results long series of observations are necessary. The labor of deriving the Moon's mass in this way was formerly so great that for more than half a century La Place's determination from the tides at Brest remained unique, but the recent application of harmonic analysis to the data supplied by self registering tide gauges is likely to yield abundant results in the near future.

Our second gravitational relation, viz., that connecting the Moon's mass and parallax with the force of gravity at the Earth's surface, affords an indirect method of determining the Moon's parallax with very great accuracy if the computation is carefully made, and with a fair approximation to the truth even when the data are exceedingly crude. To illustrate this, let us see what could be done with a railroad transit such as is commonly used by surveyors, a steel tape, and a fairly good watch. Neglecting small corrections due to the flattening of the Earth, the centrifugal force at its surface, the eccentricity of its orbit and the mass of the Moon; the law of gravitation shows that if we multiply together the length of the seconds pendulum, the square of the radius of the Earth and the square of the length of

the sidereal month, divide the product by four, and take the cube root of the quotient, the result will be the distance from the Earth to the Moon. To find the length of the seconds pendulum we would rate the watch by means of the railroad-transit, and then making a pendulum out of a spherical leaden bullet suspended by a fine thread, we would adjust the length of the thread until the pendulum made exactly 300 vibrations in five minutes by the watch. Then, supposing the experiment to be made here or in New York City, we would find that the distance from the point of suspension of the thread to the centre of the bullet was about $39\frac{1}{8}$ inches, and dividing that by the number of inches in a mile, viz., 63,360, we would have for the length of the seconds pendulum one sixteen hundred and twentieth of a mile. The next step would be to ascertain the radius of the Earth, and the quickest way of doing so would probably be, first, to determine the latitude of some point in New York City by means of the railroad transit; next to run a traverse survey along the old post road from New York to Albany, and finally to determine the latitude of some point in Albany. The traverse survey should surely be correct to one part in three hundred, and as the distance between the two cities is about two degrees, the difference of latitude might be determined to about the same percentage of accuracy. In that way we would find the length of two degrees of latitude to be about 138 miles, whence the Earth's radius would be 3,953 miles. It would then only remain to observe the time occupied by the Moon in making a sidereal revolution around the Earth, or, in other words, the time which she occupies in moving from any given star back to the same star again. By noting that to within one quarter of her own diameter we would soon find that the time of revolution is about 27.32 days, and multiplying that by the number of seconds in a day, viz., 86,400, we would have for the length of the sidereal month 2,360,000 seconds. With these data the computation would stand as follows: The radius of the Earth, 3,953 miles, multiplied by the length of a sidereal month, 2,360,000 seconds, and the product squared gives 87,060,000,000,000,000,000. Multiplying that by one-fourth of the length of the seconds pendulum, viz., $\frac{1}{6480}$ of a mile, and extracting the cube root of the product, we would get 237,700 miles for the distance from the Earth to the Moon, which is only about 850 miles less than the truth, and certainly a remarkable result considering the crudeness of the instruments by which it might be obtained. Nevertheless, when all the conditions are rigorously taken into account these

data are to be regarded as determining the relation between the Moon's mass and parallax, rather than the parallax itself.

Our third gravitational relation, to wit: that existing between the solar parallax, the solar attractive force and the masses of the Earth and Moon, is analagous to the relation existing between the Moon's mass and parallax and the force of gravity at the Earth's surface, but it cannot be applied in exactly the same way on account of our inability to swing a pendulum on the Sun. We are therefore compelled to adopt some other method of determining the Sun's attractive force, and the most available is that which consists in observing the perturbative action of the Earth and Moon upon our nearest planetary neighbors, Venus and Mars. From this action the law of gravitation enables us to determine the ratio of the Sun's mass to the combined masses of the Earth and Moon, and then the relation in question furnishes a means of comparing the masses so found with trigonometrical determinations of the solar parallax. Thus it appears that notwithstanding necessary differences in the methods of procedure, the analogy between the second and third gravitational relations holds not only with respect to their theoretical basis, but also in their practical application, the one being used to determine the relation between the mass of the Moon and its distance from the Earth, and the other to determine the relation between the combined masses of the Earth and Moon and their distance from the Sun.

Our fourth gravitational relation deals with the connection between the solar parallax, the lunar parallax, the Moon's mass and the Moon's parallactic inequality. The important quantities are here the solar parallax and the Moon's parallactic inequality, and although the derivation of the complete expression for the connection between them is a little complicated, there is no difficulty in getting a general notion of the forces involved. As the Moon moves around the Earth she is alternately without and within the Earth's orbit. When she is without, the Sun's attraction on her acts with that of the Earth; when she is within, the two attractions act in opposite directions. Thus in effect the centripetal force holding the Moon to the Earth is alternately increased and diminished, with the result of elongating the Moon's orbit toward the Sun and compressing it on the opposite side. As the variation of the centripetal force is not great, the change of form of the orbit is small; nevertheless, the summation of the minute alterations thereby produced in the Moon's orbital velocity suffices to put her sometimes ahead and sometimes behind

her mean place to an extent which oscillates from a maximum to a minimum, as the Earth passes from perihelion to aphelion, and averages about 125 seconds of arc. This perturbation of the Moon is known as the parallactic inequality, because it depends on the Earth's distance from the Sun, and can therefore be expressed in terms of the solar parallax. Conversely, the solar parallax can be deduced from the observed value of the parallatic inequality, but unfortunately there are great practical difficulties in making the requisite observations with a sufficient degree of accuracy. Notwithstanding the ever recurring talk about the advantages to be obtained by observing a small, well-defined crater instead of the Moon's limb, astronomers have hitherto found it impracticable to use anything but the limb, and the disadvantage of doing so, as compared with observing a star, is still further increased by the circumstance that in general only one limb can be seen at a time, the other being shrouded in darkness. If both limbs could always be observed we should then have a uniform system of data for determining the place of the centre, but under existing circumstances we are compelled to make our observations half upon one limb and half upon the other, and thus they involve all the systematic errors which may arise from the conditions under which these limbs are observed, and all the uncertainty which attaches to irradiation, personal equation and our defective knowledge of the Moon's semi-diameter.

Our fifth gravitational relation is that which exists between the solar parallax, the lunar parallax, the Moon's mass and the Earth's lunar inequality. Strictly speaking the Moon does not revolve around the Earth's center, but both bodies revolve around the common center of gravity of the two. In consequence of that an irregularity arises in the Earth's orbital velocity around the Sun, the common center of gravity moving in accordance with the laws of elliptic motion, while the Earth, on account of its revolution around that center, undergoes an alternate acceleration and retardation which has for its period a lunar month, and is called the lunar inequality of the Earth's motion. We perceive this inequality as an oscillation superposed on the elliptic motion of the Sun, and its semi-amplitude is the measure of the angle subtended at the Sun by the interval between the center of the Earth and the common center of gravity of the Earth and Moon. Just as an astronomer on the Moon might use the radius of her orbit around the Earth as a base for measuring her distance from the Sun, so we may use this interval for the same purpose. We find

its length in miles from the equatorial semi-diameter of the Earth, the Moon's parallax and the Moon's mass, and thus we have all the data for determining the solar parallax from the inequality in question. In view of the great difficulty which has been experienced in measuring the solar parallax itself, it may be asked why we should attempt to deal with the parallactic inequality, which is about 26 per cent smaller? The answer is, because the latter is derived from differences of the Sun's right ascension, which are furnished by the principal observatories in vast numbers, and should give very accurate results on account of their being made by methods which insure freedom from constant errors. Nevertheless, the Sun is not so well adapted for precise observations as the stars, and Dr. Gill has recently found that heliometer measurements upon asteroids which approach very near to the Earth yield values of the parallactic inequality superior to those obtained from right ascensions of the Sun.

Our sixth gravitational relation is that which exists between the Moon's parallax and the constants of precession and nutation. Every particle of the Earth is attracted both by the Sun and by the Moon, but in consequence of the polar flattening the resultant of these attractions passes a little to one side of the Earth's center of gravity. Thus a couple is set up, which, by its action upon the rotating Earth, causes the axis thereof to describe a surface which may be called a fluted cone, with its apex at the Earth's center. A top spinning with its axis inclined describes a similar cone, except that the flutings are absent and the apex is at the point upon which the spinning occurs. For convenience of computation we resolve this action into two components, and we name that which produces the cone the luni-solar precession, and that which produces the flutings the nutation. In this phenomenon the part played by the Sun is comparatively small, and by eliminating it we obtain a relation between the luni-solar precession, the nutation and the Moon's parallax which can be used to verify and correct the observed values of these quantities.

In the preceding paragraph we have seen that the relation between the quantities there considered depends largely upon the flattening of the Earth, and thus we are lead to inquire how and with what degree of accuracy that is determined. There are five methods, viz: one geodetic, one gravitational, and three astronomical. The geodetic method depends upon measurements of the length of a degree on various parts of the Earth's surface; and with the data hitherto accumulated it has proved quite un-

satisfactory. The gravitational method consists in determining the length of the seconds pendulum over as great a range of latitude as possible, and deducing therefrom the ratio of the Earth's polar and equatorial semi-diameters by means of Clairaut's theorem. The pendulum experiments show that the Earth's crust is less dense on mountain plateaux than at the sea coast, and thus for the first time we are brought into contact with geological considerations. The first astronomical method consists in observing the Moon's parallax from various points on the Earth's surface, and as these parallaxes are nothing else than the angular semi-diameter of the Earth at the respective points, as seen from the Moon, they afford a direct measure of the flattening. The second and third astronomical methods are based upon certain perturbations of the Moon which depend upon the figure of the Earth, and should give extremely accurate results, but unfortunately very great difficulties oppose themselves to the exact measurement of the perturbations. There is also an astronomico-geological method which cannot yet be regarded as conclusive on account of our lack of knowledge respecting the law of density which prevails in the interior of the Earth. It is based upon the fact that a certain function of the Earth's moments of inertia can be determined from the observed values of the coefficients of precession and nutation, and could also be determined from the figure and dimensions of the Earth if we knew the exact distribution of matter in its interior. Our present knowledge on that subject is limited to a superficial layer not more than ten miles thick, but it is usual to assume that the deeper matter is distributed, according to La Grange's law, and then by writing the function in question in a form which leaves the flattening indeterminate, and equating the expression so found to the value given by the precession and nutation, we readily obtain the flattening. As yet these methods do not give consistent results, and so long as serious discrepancies remain between them there can be no security that we have arrived at the truth.

It should be remarked that in order to compute the function of the Earth's moments of inertia which we have just been considering, we require not only the figure and dimensions of the Earth and the law of distribution of density in its interior, but also its mean and surface densities. The experiments for determining the mean density have consisted in comparing the Earth's attraction with the attraction either of a mountain, or of a known thickness of the Earth's crust, or of a known mass of metal. In the case of mountains the comparisons have been made with

plumb lines and pendulums; in the case of known layers of the Earth's crust they have been made by swinging pendulums at the surface and down in mines; and in the case of known masses of metal they have been made with torsion balances, fine chemical balances and pendulums. The surface density results from a study of the materials composing the Earth's crust, but notwithstanding the apparent simplicity of that process, it is doubtful if we have yet attained as accurate a result as in the case of the mean density.

Before quitting this part of our subject, it is important to point out that the luni-solar precession cannot be directly observed, but must be derived from the general precession. The former of these qualities depends only upon the action of the Sun and Moon, while the latter is affected in addition by the action of all the planets, and to ascertain what that is we must determine their masses. The methods of doing so fall into two great classes, according as the planets dealt with have or have not satellites. The most favorable case is that in which one or more satellites are present, because the mass of the primary follows immediately from their distances and revolution times, but even then there is a difficulty in the way of obtaining very exact results. By extending the observations over sufficiently long periods the revolution times may be ascertained with any desired degree of accuracy, but all measurements of the distance of a satellite from its primary are affected by personal equation, which we cannot be sure of completely eliminating, and thus a considerable margin of uncertainty is brought into the masses. In the cases of Mercury and Venus, which have no satellites, and to a certain extent in the case of the Earth also, the only available way of ascertaining the masses is from the perturbations produced by the action of the various planets on each other. These perturbations are of two kinds, periodic and secular. When sufficient data have been accumulated for the exact determination of the secular perturbations, they will give the best results, but as yet it remains advantageous to employ the periodic perturbations also.

Passing now to the photo-tachymetrical methods, we have first to glance briefly at the mechanical appliances by which the tremendous velocity of light has been successfully measured. They are of the simplest possible character, and are based either upon a toothed wheel, or upon a revolving mirror.

The toothed-wheel method was first used by Fizeau in 1849. To understand its operation, imagine a gun barrel with a toothed wheel revolving at right angles to its muzzle in such a way that

the barrel is alternately closed and opened as the teeth and the spaces between them pass before it. Then, with the wheel in rapid motion, at the instant when a space is opposite the muzzle let a ball be fired. It will pass out freely, and after traversing a certain distance, let it strike an elastic cushion and be reflected back upon its own path. When it reaches the wheel, if it hits a space it will return into the gun barrel, but if it hits a tooth it will be stopped. Examining the matter a little more closely we see that as the ball requires a certain time to go and return, if, during that time the wheel moves through an odd multiple of the angle between a space and a tooth the ball will be stopped, while if it moves through an even multiple of that angle the ball will return into the barrel. Now imagine the gun barrel, the ball and the elastic cushion to be replaced respectively by a telescope, a light wave and a mirror. Then if the wheel moved at such a speed that the returning light wave struck against the tooth following the space through which it issued, to an eye looking into the telescope all would be darkness. If the wheel moved a little faster and the returning light wave passed through the space succeeding that through which it issued, the eye at the telescope would perceive a flash of light, and if the speed was continuously increased a continual succession of eclipses and illuminations would follow each other according as the returning light was stopped against a tooth or passed through a space further and further behind that through which it issued. Under these conditions the time occupied by the light in traversing the space from the wheel to the mirror and back again would evidently be the same as the time required by the wheel to revolve through the angle between the space through which the light issued and that through which it returned, and thus the velocity of light would become known from the distance between the telescope and the mirror, together with the speed of the wheel. Of course the longer the distance traversed and the greater the velocity of the wheel the more accurate would be the result.

The revolving mirror method was first used by Foucault in 1862. Conceive the toothed wheel of Fizeau's apparatus to be replaced by a mirror attached to a vertical axis and capable of being put into rapid rotation. Then it will be possible so to arrange the apparatus that light issuing from the telescope shall strike the movable mirror and be reflected to the distant mirror, whence it will be returned to the movable mirror again, and being thrown back into the telescope will appear as a star in the center of the field of view. That adjustment being made, if the

mirror were caused to revolve at a speed of some hundred turns per second it would move through an appreciable angle while the light was passing from it to the distant mirror and back again, and in accordance with the laws of reflection, the star in the field of the telescope would move from the center by twice the angle through which the mirror had turned. Thus the deviation of the star from the center of the field would measure the angle through which the mirror turned during the time occupied by light in passing twice over the interval between the fixed and revolving mirrors, and from the magnitude of that angle, together with the known speed of the mirror, the velocity of the light could be calculated.

In applying either of these methods the resulting velocity is that of light when traversing the Earth's atmosphere, but what we want is its velocity in space, which we suppose to be destitute of ponderable material, and in order to obtain that the velocity in the atmosphere must be multiplied by the refractive index of air. The correct velocity so obtained can then be used to find the solar parallax, either from the time required by light to traverse the semi-diameter of the Earth's orbit, or from the ratio of the velocity of light to the orbital velocity of the Earth.

Any periodic correction which occurs in computing the place of a heavenly body, or the time of a celestial phenomenon, is called by astronomers an equation, and as the time required by light to traverse the semi-diameter of the Earth's orbit first presented itself in the guise of a correction to the computed times of the eclipses of Jupiter's satellites, it has received the name of the light equation. The Earth's orbit being interior to that of Jupiter, and both having the Sun for their centre, it is evident that the distances between the two planets must vary from the sum to the difference of the radii of their respective orbits, and the time required by light to travel from one planet to the other must vary proportionately. Consequently, if the observed times of the eclipses of Jupiter's satellites are compared with the times computed upon the asumption that the two planets are always separated by their mean distance, it will be found that the eclipses occur too early when the Earth is at less than its mean distance from Jupiter, and too late when it is farther off, and from large numbers of such observations the value of the light equation has been deduced.

The combination of the motion of light through our atmosphere with the orbital motion of the Earth gives rise to the annual aberration, all the phases of which are computed from its

maximum value, commonly called the constant of aberration There is also a diurnal aberration due to the rotation of the Earth on its axis, but that is quite small and does not concern us this evening. When aberration was discovered the corpuscular theory of light was in vogue, and it offered a charmingly simple explanation of the whole phenomenon. The hypothetical light corpuscles impinging upon the Earth were thought to behave precisely like the drops in a shower of rain, and you all know that their apparent direction is affected by any motion on the part of the observer. In a calm day, when the drops are falling perpendicularly, a man standing still holds his umbrella directly over his head, but as soon as he begins to move forward he inclines his umbrella in the same direction, and the more rapidly he moves the greater must be its inclination in order to meet the descending shower. Similarly, the apparent direction of on coming light corpuscles would be affected by the orbital motion of the Earth, so that in effect it would always be the resultant arising from combining the motion of the light with a motion equal and opposite to that of the Earth. But since the falsity of the corpuscular theory has been proved that explanation is no longer tenable, and as yet we have not been able to replace it with anything equally satisfactory based on the now universally accepted undulatory theory. In accordance with the latter theory we must conceive the Earth as plowing its way through the ether, and the point which has hitherto baffled us is whether or not in so doing it produces any disturbance of the ether which affects the aberration. In our present ignorance on that point we can only say that the aberration constant is certainly very nearly equal to the ratio of the Earth's orbital velocity to the velocity of light, but we cannot affirm that it is rigorously so.

The luminiferous ether was invented to account for the phenomena of light, and for two hundred years it was not suspected of having any other function. The emission theory postulated only the corpuscles which constitute light itself, but the undulatory theory fills all space with an imponderable substance possessing properties even more remarkable than those of ordinary matter, and to some of the acutest intellects the magnitude of this idea has proved an almost insuperable objection against the whole theory. So late as 1862 Sir David Brewster, who had gained a world wide reputation by his optical researches, expressed himself as staggered by the notion of filling all space with some substance merely to enable a little twinkling star to send its light to us; but not long after Clerk Maxwell removed that

difficulty by a discovery coextensive with the undulatory theory itself. Since 1845, when Faraday first performed his celebrated experiment of magnetizing a ray of light, the idea that electricity is a phenomenon of the ether had been steadily growing, until at last Maxwell perceived that if such were the fact the rate of propagation of an electro-magnetic wave must be the same as the velocity of light. At that time no one knew how to generate such waves, but Maxwell's theory showed him that their velocity must be equal to the number of electric units of quantity in the electro-magnet unit, and careful experiments soon proved that that is the velocity of light. Thus it was put almost beyond the possibility of doubt that the ether gives rise to the phenomena of electricity and magnetism as well as to those of light, and perhaps it may even be concerned in the production of gravitation itself. What could be apparently more remote that these electric quantities and the solar parallax? And yet we have here a relation between them, but we make no use of it because as yet the same relation can be far more accurately determined from experiments upon the velocity of light.

Now let us recall the quantities and methods of observation which we have found to be involved, either directly or indirectly, with the solar parallax. They are, the solar parallax, obtained from transits of Venus, oppositions of Mars and oppositions of certain asteroids; the lunar parallax, found both directly and from measurements of the force of gravity at the Earth's surface; the constants of precession, nutation and aberration, obtained from observations of the stars; the parallactic inequality of the Moon; the lunar inequality of the Earth, usually obtained from observations of the Sun, but recently found from heliometer observations of certain asteroids; the mass of the Earth, found from the solar parallax, and also from the periodic and secular perturbations of Venus and Mars; the mass of the Moon, found from the lunar inequality of the Earth, and also from the ratio of the solar and lunar components of the ocean tides; the masses of all the planets, obtained from observations of their satellites whenever possible, and when no satellites exist, then from observations of their mutual perturbations, both periodic and secular; the velocity of light, obtained from experiments with revolving mirrors and toothed wheels, together with laboratory determinations of the index of refraction of atmospheric air; the light equation, obtained from observations of the eclipses of Jupiter's satellites; the figure of the Earth, obtained from geodetic triangulations, measurements of the length of the seconds pendulum

in various latitudes, and observations of certain perturbations of the Moon; the mean density of the Earth, obtained from measurements of the attractions of mountains, from pendulum experiments in mines, and from experiments on the attraction of known masses of matter made either with torsion balances or with the most delicate chemical balances: the surface density of the Earth, obtained from geological examinations of the surface strata; and lastly, the law of distribution of density in the interior of the Earth, which in the present state of geological knowledge we can do little more than guess at.

Here then we have a large group of astronomical, geodetic, geological and physical quantities which must all be considered in finding the solar parallax, and which are all so entangled with each other that no one of them can be varied without affecting all the rest. It is therefore impossible to make an accurate determination of any one of them apart from the remainder of the group, and thus we are driven to the conclusion that they must all be determined simultaneously. Such has not been the practice of astronomers in the past, but it is the method to which they must inevitably resort in the future A cursory glance at an analogous problem occuring in geodesy may be instructive. When a country is covered with a net of triangles it is always found that the observed angles are subject to a certain amount of error, and a century ago it was the habit to correct the angles in each triangle without much regard to the effect upon adjacent triangles. Consequently the adjustment of the errors was imperfect, and in computing the interval between any two distant points the result would vary somewhat with the triangles used in the computation—that is, if one computation was made through a chain of triangles running around on the right hand side, another through a chain of triangles running straight between the two points, and a third through a chain of triangles running around on the left hand side, the results were usually all different. At that time things were less highly specialized than now, and all geodetic operations were yet in the hands of first rate astronomers, who soon devised processes for overcoming the difficulty. They imagined every observed angle to be subject to a small correction, and as these corrections were all entangled with each other through the geometrical conditions of the net, by a most ingenious application of the method of least squares they determined them all simultaneously in such a way as to satisfy the whole of the geometrical conditions. Thus the best possible adjustment was obtained, and no matter what triangles were used in passing

from one point to another, the result was always the same. That method is now applied to every important triangulation, and its omission would be regarded as proof of incompetency on the part of those in charge of the work

Now let us compare the conditions existing respectively in a triangulation net and in the group of quantities for the determination of the solar parallax. In the net every angle is subject to a small correction, and the whole system of corrections must be so determined as to make the sum of their weighted squares a minimum, and at the same time satisfy all the geometrical conditions of the net. Like the triangles, the quantities composing the group from which the solar parallax must be determined are all subject to error, and therefore we must regard each of them as requiring a small correction, and all these corrections must be so determined as to make the sum of their weighted squares a minimum, and at the same time satisfy every one of the equations expressing the relations between the various components of the group.

Thus it appears that the method required for adjusting the solar parallax and its related constants is in all respects the same as that which has so long been used for adjusting systems of triangulation, and as the latter method was invented by astronomers, it is natural to inquire why they have not applied it to the fundamental problem of their own science? The reasons are various, but they may all be classed under two heads. First, an inveterate habit of over-estimating the accuracy of our own work as compared with that of others; and second, the unfortunate effect of too much specialization.

The prevailing opinion certainly is that great advances have recently been made in astronomy, and so they have in the fields of spectral analysis and in the measurement of minute quantities of radiant heat; but the solution of the vast majority of astronomical problems depends upon the exact measurement of angles, and in that little or no progress has been made. Bradley, with his zenith sector a hundred and fifty years ago, and Bessel and Struve, with their circles and transit instruments seventy years ago, made observations not sensibly inferior to those of the present day, and indeed it would have been surprising if they had not done so. The essentials for accurately determining star places are a skilled observer, a clock and a transit circle, the latter consisting of a telescope, a divided circle and four micrometer microscopes. Surely no one will claim that we have to-day any more skilful observers than were Bessel, Bradley and Struve, and

the only way in which we have improved upon the telescopes made by Dollond one hundred and thirty years ago, is by increasing their aperture and relatively diminishing their focal distance. The most famous dividing engine now in existence was made by the elder Repsold seventy-five years ago; but as the errors of divided circles and their micrometer microscopes are always carefully determined, the accuracy of the measured angles is quite independent of any small improvement in the accuracy of the divisions or of the micrometer screws. Only in the matter of clocks has there been some advance, and even that is not very great. On the whole, the star places of to-day are a little better than those of seventy-five years ago, but even yet there is great room for improvement. One of the commonest applications of these star places is to the determination of latitude, but it is very doubtful if there is any point on the face of the Earth whose latitude is known certainly within one-tenth of a second.

Looking at the question from another point of view, it is notorious that the contact observations of the transits of Venus in 1761 and 1769 were so discordant that from the same observations Encke and E. J. Stone got respectively for the solar parallax 8.59 seconds and 8.91 seconds. In 1870 no one thought it possible that there could be any difficulty with the contact observations of the then approaching transits of 1874 and 1882, but we have found from sad experience that our vaunted modern instruments gave very little better results for the last pair of transits than our predecessors obtained with much cruder appliances in 1761 and 1769.

The theory of probability and uniform experience alike show that the limit of accuracy attainable with any instrument is soon reached; and yet we all know the fascination which continually lures us on in our efforts to get better results out of the famillar telescopes and circles which have constituted the standard equipment of observatories for nearly a century. Possibly these instruments may be capable of indicating somewhat smaller quantities than we have hitherto succeeded in measuring with them, but their limit cannot be far off, because they already show the disturbing effects of slight inequalities of temperature and other uncontrolable causes. So far as these effects are accidental they eleminate themselves from every long series of observations, but there always remains a residuum of constant error, perhaps quite unsuspected, which gives us no end of trouble. Encke's value of the solar parallax affords a fine illustration of this. From the transits of Venus in 1761 and 1769 he found 8.58

seconds in 1824, which he subsequently corrected to 8.57 seconds, and for thirty years that value was universally accepted. The first objection to it came from Hansen in 1854, a second followed from Le Verrier in 1858, both based upon facts connected with the lunar theory, and eventually it became evident that Encke's parallax was about one-quarter of a second too small.

Now please observe that Encke's value was obtained trigonometrically, and its inaccuracy was never suspected until it was revealed by gravitational methods, which were themselves in error about one-tenth of a second, and required subsequent correction in other ways. Here, then, was a lesson to astronomers, who are all more or less specialists, but it merely enforced the perfectly well known principle that the constant errors of any one method are accidental errors with respect to all other methods, and therefore the readiest way of eliminating them is by combining the results from as many different methods as possible. However, the abler the specialist the more certain he is to be blind to all methods but his own, and astronomers have profited so little by the Encke-Hansen-Le Verrier incident of thirty-five years ago that to-day they are mostly divided into two great parties, one of whom holds that the parallax can be best determined from a combination of the constant of aberration with the velocity of light, and the other believes only in the results of heliometer measurements upon asteroids. By all means continue the heliometer measurements, and do everything possible to clear up the mystery which now surrounds the constant of aberration, but why ignore the work of predecessors who were quite as able as ourselves? If it were desired to determine some one angle of a triangulation net with special exactness, what would be thought of a man who attempted to do so by repeated measurements of the angle in question while he persistently neglected to adjust the net? And yet, until very recently, astronomers have been doing precisely that kind of thing with the solar parallax. I do not think there is any exaggeration in saying that the trustworthy observations now on record for the determinination of the numerous quantities which are functions of the parallax could not be duplicated by the most industrious astronomer working continuously for a thousand years. How then can we suppose that the result properly deducible from them can be materially affected by anything that any of us can do in a life time, unless we are fortunate enough to invent methods of measurement vastly superior to any hitherto imagined? Probably the existing observations for the determin-

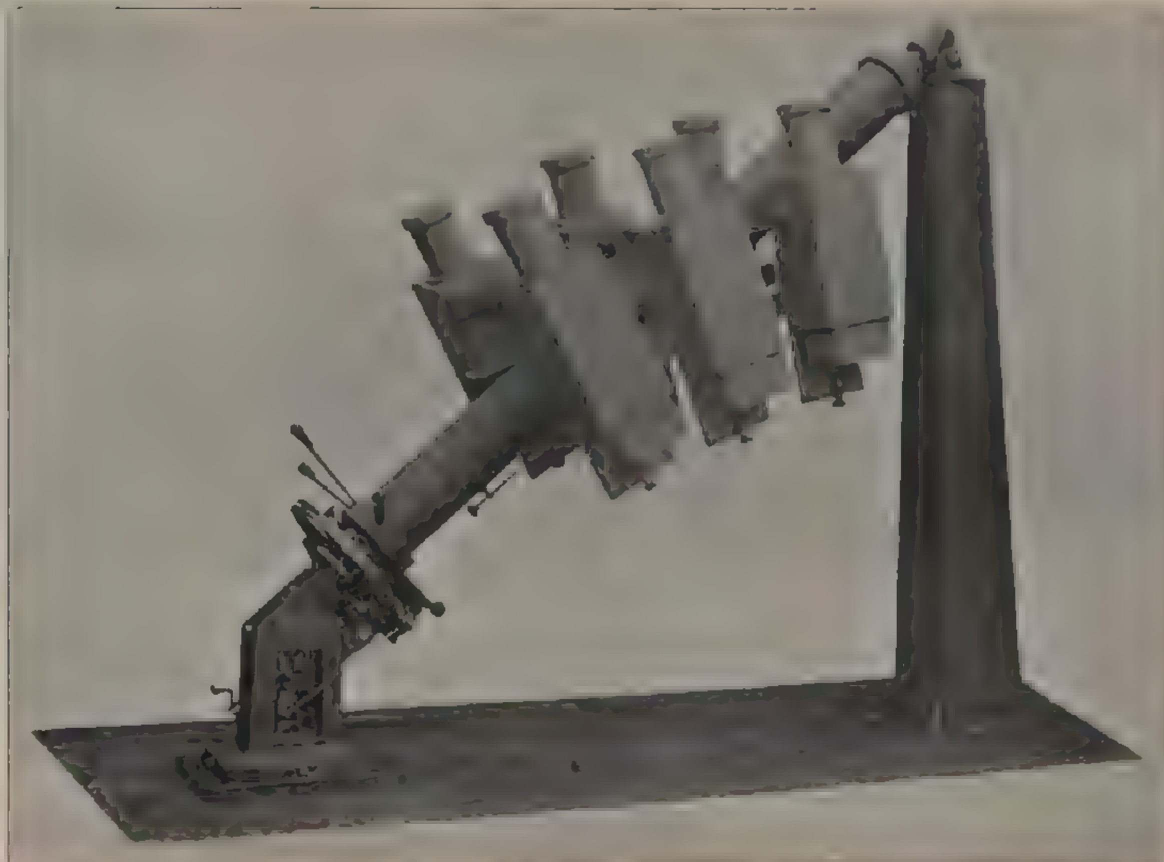
ation of most of these quantities are as exact as any that can ever be made with our present instruments, and if they were freed from constant errors they would certainly give results very near the truth. To that end we have only to form a system of simultaneous equations between all the observed quantities, and then deduce the most probable values of these quantities by the method of least squares. Perhaps some of you may think that the value so obtained for the solar parallax would depend largely upon the relative weights assigned to the various quantities, but such is not the case. With almost any possible system of weights the solar parallax will come out very nearly 8.809″ ± 0.0057″, whence we have for the mean distance between the Earth and the Sun 92,797,000 miles, with a probable error of only 59,700 miles; and for the diameter of the solar system, measured to its outermost member, the planet Neptune, 5,578,400,000 miles.

INSTRUMENT FOR THE PHOTOGRAPHY OF METEORS FOR THE YALE OBSERVATORY.

W. L. ELKIN

The experiments made at this Observatory last year seemed to show that if a sufficiently large field could be covered, it might be possible to secure quite a number of meteor tracks on photographic plates, during the August and December showers, at least. The incomparably greater accuracy, as against eye observations, with which these tracks locate the meteor and the radiant, has led us to consider the matter worth following up and accordingly application was made to the National Academy for an appropriation from the Lawrence Smith fund which is to be devoted to meteoric researches. From the grant awarded us the instrument represented in the cut has been constructed by Messrs Warner and Swazey. It is a polar axis of the "English" form, this seeming to be the most convenient and the best adapted mounting for carrying a number of cameras, and admitting of long exposures without break. The axis is of tubular form, about 12 feet long, the ends being pivots working in bearings which are adjustable on their supports. The southern support, or base, contains the clock-work, the northern support is a column containing the driving weights, the connection being made by a cord passing under the floor. The declination axis carries arms on either end

PLATE XX.



INSTRUMENT FOR THE PHOTOGRAPHY OF METEORS FOR THE YALE OBSERVATORY.

which serve as supports for the cameras. On the cut six dummy cameras are shown; it is not likely for the present, however, that we shall use more than four. Graduated circles and slow-motions for both coördinates are provided, and the clock-work has an electric control. The apparatus is now mounted here, and will be tried on the Perseids this year.

YALE UNIVERSITY OBSERVATORY.

THE LOCUS OF THE CENTRE OF GRAVITY FOR A HOMOGENEOUS ELLIPSOID OF REVOLUTION.*

T. J. J. SEE.

Herschel's "Outlines of Astronomy" and Airy's "Gravitation" have rendered a valuable service to science by furnishing the general reader, the student, the teacher, and even the theoretical astronomer, with a continuous representation of gravitational phenomena. The questions geometrically investigated by authorities like Newton, Herschel and Airy include the perturbations of the orbital motions of the heavenly bodies, and of their rotations about their centres of inertia; and Airy has even treated in a very elementary manner the attraction of an oblate planet. As the advantages of introducing geometrical representation into gravitational astronomy are so considerable that the procedure has been adopted by leading authorities in Celestial Mechanics, the writer is led to believe that a geometrical treatment of the attraction of a homogeneous ellipsoid of revolution will not be without interest.

It is to be understood here that the centre of inertia (or centre of mass) is a perfectly definite point, depending only upon the relative positions of the particles composing the body, and is fixed so long as the particles are relatively fixed. But the centre of gravity, being the point at which the whole mass might be collected and the attraction on a given point would be unchanged, is not fixed in the attracting mass, but depends upon the position of the attracted point relative to the attracting mass.

In case of symmetrical masses, the centre of inertia and the centre of gravity will occasionally coincide, but in general the centre of gravity will not be in the same direction as the centre of inertia, nor at the same distance.

* Read before the Chicago Academy of Sciences, Sept. 1, 1894.

As a concrete example, we may consider the attraction of an oblate planet: the body will rotate about its centre of inertia, or centre of figure (when homogeneous), but the force of gravity is normal to the surface of equilibrium, and therefore does not generally pass through the centre of figure, or centre of inertia.

It is well known from the theory of the figures of the planets that oblateness renders the attraction in the direction of the pole less than if the whole mass were collected at the centre of figure, while the attraction in the direction of the equator is rendered correspondingly greater; and from this it follows that at some intervening latitude the attraction must be of the same intensity as if the whole mass were collected at the centre. For a homogeneous ellipsoid of revolution differing but little from a sphere this latitude is found to be

$$\psi = 35^\circ\ 15'\ 52''$$

(See Laplace's *Mécanique Céleste*, Tome II, p. 110, or Tisserand's *Mécanique Céleste*, Tome II, p. 66). But we observe that as the attraction is always normal to the surface of equilibrium it is not directed toward the centre of the ellipsoid, but in the tangent to the evolute. As the body is supposed to be an ellipsoid of revolution, we need regard only a section of the meridian, which will be an ellipse whose equation is

$$\frac{\xi^2}{a^2} + \frac{\eta^2}{b^2} = 1,$$

where a is the semi major and b the semi-minor axis of the ellipsoid. If we introduce $\lambda = \tan\varphi$, where φ is the angle of the eccentricity determined by $e = \sin\varphi$, we may write the equation for the ellipse in the form

$$\frac{\xi^2}{1+\lambda^2} + \eta^2 = b^2. \tag{1}$$

The evolute of the ellipse is given by the equation

$$\frac{\xi^{\frac{2}{3}}}{A^{\frac{2}{3}}} + \frac{\eta^{\frac{2}{3}}}{B^{\frac{2}{3}}} = 1, \tag{2}$$

where

$$A = \frac{a^2 - b^2}{a}, \quad B = \frac{a^2 - b^2}{b}.$$

(See *Salmon's Conic Sections*, 5th edition, p. 220).

By means of this equation we may draw the curve for an ellipse of any given eccentricity.

If now we put σ for the density of the ellipsoid, $\lambda = \tan\varphi$, and

$$P = 4\pi\sigma \frac{1+\lambda^2}{\lambda^3} (\lambda - \text{arc tan } \lambda), \tag{3}$$

then, it is shown in Tisserand's *Mecanique Celeste*, Tome II, p. 88, that the attraction at any point on the surface of the ellipsoid is given by the equation

$$g = \frac{Pb^3}{\delta}, \tag{4}$$

where δ is the perpendicular distance from the centre of the ellipsoid to the plane tangent to the ellipsoid at the given point.

It is easily shown that δ is given by the equation

$$\delta = \frac{b^2}{\sqrt{\eta^2 + \frac{\xi^2}{(1+\lambda^2)^2}}} \tag{5}$$

Now by equation (3) we see that P is constant, and of course b is constant; therefore by (4) we learn that the force of gravity in different latitudes varies inversely as the perpendicular distance δ from the centre to the tangent plane; and hence it is evident that the attraction in the direction of the pole will be greater than that in the direction of the equator.

This result seems inconsistent with that previously stated, viz.: The attraction in the direction of the pole is less than if the whole mass were collected at the centre of the figure, while in the direction of the equator it is greater.

The apparent discordance is due to the increase of distance at the equator due to oblateness, which makes the *centre of gravity* there more remote from the surface than it is at the pole, notwithstanding the fact that at the equator the centre of gravity is (as we shall see hereafter) between the centre of figure and the surface, while at the pole it is beyond the centre of figure.

We shall now deduce the locus of the center of gravity as the attracted point moves along the surface from the pole to the equator.

In the first place we know the centre of gravity will be on the normals at the different points of the quadrant

As a graphical illustration is desired, we chose an ellipsoid of considerable oblateness, so that the evolute and locus may be sufficiently large when drawn to scale. We take the oblateness as 0.1, which about corresponds to the planet Saturn, and then the eccentricity $e = 0.436$; with a semi major axis of convenient length (10 inches) we draw an ellipse which will represent a meridianal section of the ellipsoid.

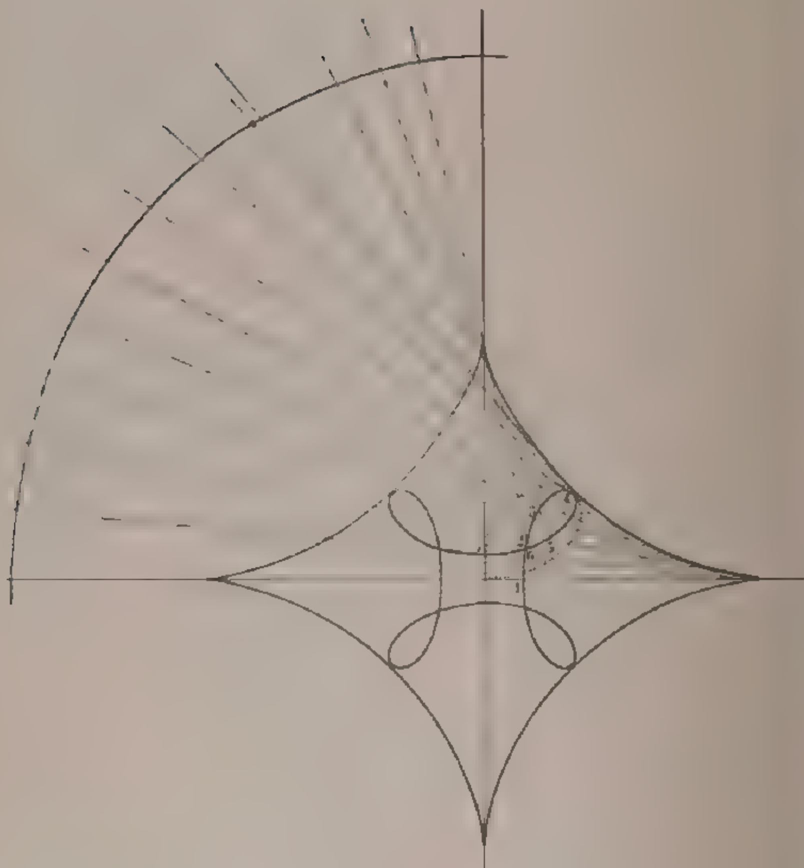
By means of equation (1) we compute the values of η corresponding to convenient arguments of ξ; and with these values of the co-ordinate η and ξ, we compute the evolute by means of equation (2), and plat it to scale. The normals at the different points of the quadrant are then drawn tangent to the evolute.

It remains to compute δ from equation (5), and thus we obtain tabular members which are inversely proportional to the attraction at the different points.

Now
$$g = \frac{Pb^2}{\delta}$$
and we learn from the theory of the attraction of an ellipsoid (Tisserand's *Mecanique Celeste*, Tome II, p. 66) that in latitude
$$\psi = 35^\circ\ 15'\ 52'',$$
$$g = \frac{Pb^2}{\delta_1} = \frac{M}{r_1^2},$$
where M is the mass of a sphere having the same volume and density, and r_1, its radius. Since δ_1 and r_1, are both perpendicular to the tangent plane, and therefore parallel, and r_1 is also sensibly equal to the distance of the attracted point from the centre of the ellipsoid (Tisserand's *Mecanique Celeste*, Tome II, p. 66; quantities of the order λ^4 are here neglected); it follows that r_1 and δ_1 are sensibly equal.

Now we may take the average force of gravity ($\psi = 35^\circ\ 15'\ 52''$) as the unit, and then
$$g_1 = \frac{Pb^2}{\delta_1} = \frac{M}{r_1^2} = 1$$
Since δ is at this particular point sensibly equal to r_1 we may unite
$$\frac{M}{\delta_1^2} = \frac{Pb^2}{\delta_1},\ \text{or}\ \delta_1 = \frac{M}{Pb^2} = 1;$$
and hence
$$\left(\frac{\delta}{\delta_1}\right) = \frac{r^2}{r_1^2} = \frac{r^2}{\delta_1^2},\ \text{or}\ \delta\delta_1 = r^2;$$
but since $\delta_1 = 1$, we have $r = \sqrt{\delta}$, where δ is expressed in terms of r_1 or δ_1, as the unit. That is, the distance of the centre of gravity from the surface along the normals is equal to the square root of δ expressed in terms of the mean radius r_1 as the unit. In case of the ellipsoid illustrated in the figure, $a = 10$, and $r_1 = 9.6549$; the numerical values of r found from $r = \sqrt{\delta}$, must therefore be multiplied by $9.6549 = r_1$ in order to reduce them to the scale

PLATE XXI.



LOCUS OF THE CENTRE OF GRAVITY FOR A HOMOGENEOUS ELLIPSOID OF REVOLUTION.

ASTRONOMY AND ASTRO-PHYSICS, No 138

$a = 10$. The quadrant of the ellipse shown in the figure is reduced by the factor $\frac{1}{3}$, so that it is on a smaller scale than the other curves.

When the values of r have been found, we plat the resulting points along the normals and obtain the looped curve indicated in the figure. The course of the curve in the other three quadrants is similar, and we find for the entire locus the beautiful curve with four loops.

It will be seen that the curve is very symmetrical and regular, but of a peculiar shape. Now as the point moves from the pole to the equator, the centre of gravity moves along the curve 1, 2, 3, 4, 5, 6, 7.

Thus we see that for points near the pole (where $\psi > 35° 15' 52''$), the centre of gravity is remoter than the centre of figure, and therefore the attraction is less than if the mass were collected at the centre of figure; for points near the equator (where $\psi < 35° 15' 52''$) the centre of gravity is nearer than the centre of figure, and hence the attraction is greater than if the whole mass were collected at that point. The distance of the points on the surface from the centre of the ellipsoid, when measured on the normals, gives the pointed line, which is seen to approach the locus at the point most remote from the centre, which corresponds to $\psi = 35° 15' 52''$. The distance of the pointed line from the locus measured along the normals, represents the amount by which the centre of gravity is nearer or remoter (relative to points on the surface) than the centre of figure.

By this simple geometrical device we are enabled to study the law of attraction of a homogeneous ellipsoid or planet, and it only remains to add that the locus of the centre of gravity for the planet's *surface* will be the *surface* resulting from the revolution of the curve here drawn around the axis of revolution of the ellipsoid.

This doubly-intersecting surface is rather complex; and we easily see that its complication is due to the fact that the locus of the centre of gravity is a function of the surface of the ellipsoid, which in turn depends upon the law of gravitation. Though we have thus far referred the locus of the centre of gravity to the surface of the ellipsoid, it is easy to refer the curve to the rectangular axes in the following manner:

If ψ denote the angle made by the normal with the plane of the equator, and r be the distance of any point (ξ, η) on the surface from the centre of gravity, and ξ' and η' denote the coördinates of the centre of gravity referred to the centre of the ellipse; then we shall have

$$\begin{array}{ll} \xi - \xi' = r \cos \psi, & \xi' = \xi - r \cos \psi, \\ \eta - \eta' = r \sin \psi; \text{ or} & \eta' = \eta - r \sin \psi \end{array} \Big\} \qquad (6)$$

The angle ψ may be found by means of its tangent, which results from dividing the ordinate η by the subnormal. The subnormal of the ellipse is $-\frac{b^2}{a^2}\xi$, (the negative sign may here be omitted, as it merely indicates that the subnormal is to be taken towards the origin), and hence we have

$$\tan \psi = \left(\frac{a}{b}\right)^2 \cdot \frac{\eta}{\xi} \qquad (7)$$

Hence ψ is at once found from the values of ξ and η; and r being found as previously indicated, we at once find the coördinates ξ' and η', which give the locus relative to the rectangular axes. Equations (6) are therefore of interest, even if they do not diminish the numerical work involved in tracing the curve.

By such a figure we see not merely the variation in the direction of gravity, but also its intensity relative to its average value, and hence it may be hoped that some new light is thrown upon the attraction of the planets which could not be gathered from purely analytical formulæ.

THE UNIVERSITY OF CHICAGO, 1894, Aug. 23.

SCHIAPARELLI'S LATEST VIEWS REGARDING MARS.*

WILLIAM H. PICKERING

It is probable that the astronomer whose name is most closely linked with the planet Mars at the present time is Giovanni Schiaparelli. And yet although nearly everybody has heard of Schiaparelli's canals, very few astronomers even, outside of France and Italy, had until recently more than a very vague notion what were really his ideas in regard to them. This is due probably to the fact that he has written exclusively in Italian, a language which very few American astronomers, and I believe very few English ones, understand. To this fact chiefly I think is due the great incredulity with which his observations have been treated, at least until recently, in both of these countries. Astronomers could understand his maps, they knew therefore what he had done, but they could not understand his descriptions of his observations, and so were incredulous regarding their accur-

* Communicated by the author.

acy. Moreover, such a mass of detail appeared upon his maps, which had not before been seen by others, that it completely masked the more striking features of the planet, thus rendering its appearance entirely different from that which it presented in the telescope under ordinary atmospheric conditions.

But within the last few years a change has occurred. Flammarion has translated a large part of Schiaparelli's writings into French, a language with which most English speaking astronomers are familiar, and moreover the canals have been seen by a number of astronomers whose descriptions of them in English and French could be understood, and were found to agree with those of Schiaparelli.

But errors are still frequently made by people who might be expected to know better. Thus, many people suppose that Schiaparelli was the original discoverer of the canals, a claim which he never made for himself. In point of fact some of them appear upon maps of the planet published more than fifty years ago. The former English incredulity in the matter seems the more strange, since many of the canals were seen by Dawes in 1864, and by Burton and Dreyer in 1879. Schiaparelli however has discovered far more canals than anyone else, and he is also the discoverer of their gemination.

In this connection, it may be that a brief chronological statement of the more important facts and discoveries relating to Mars will not be without interest. In compiling it I have been chiefly indebted to Flammarion's classic work "La Planète Mars," although other sources have also been consulted.

272 B. C. The first known observation of Mars is recorded in Ptolemy's Almagest.

1610. The phases of Mars were discovered by Galileo.

1659. The first sketch showing surface detail was made by Huyghens. He also suggested a rotation in 24 hours.

1666. Cassini determined the rotation of Mars to take place in 24 hours 40 minutes. He also observed the polar caps, and "he distinguished on the disc of Mars, near the terminator, a white spot advancing into the dark portion, and representing without doubt, like those of the Moon, a roughness or irregularity of the surface." This latter statement is curious, but the effect was undoubtedly due to irradiation, since his telescope was entirely inadequate to enable him to observe such a delicate phenomenon.

1777. With the exception of Huyghens, Hooke, and possibly Maraldi, no one succeeded in making recognizable sketches of the

surface detail upon Mars for over a century, until Sir William Herschel took the matter up in this year.

1783. Sir William Herschel detected the variation of the size of the polar snow caps with the seasons, measured the polar compression, and determined the inclination of the axis of the planet to its orbit.

1785-1802. Schrœter made an extended study of the planet. His drawings are upon the whole rather better than those of Herschel. He discovered among other things the very dark spots on which I have referred in my publications as the Northern and Equatorial Seas. He however supposed them to be clouds.

1840. Beer and Maedler published the first map of the planet, assigning latitudes and longitudes to the various markings. On this map are indicated the first canals, and the first of the small lakes, so many of which have been discovered during the last few years. The canals are *Nectar* and *Agathodaemon* and portions of *Hades* and *Tartarus*. The lake is *Lacus Phoenicis*. Their map is the first satisfactory representation of the entire surface of the planet. The only region which previous observers had clearly distinguished was that in the vicinity of the *Syrtis Major*.

1858. Secchi made a careful study of the colors exhibited by the planet.

1862. Lockyer made the first series of really good sketches of the planet, showing all the characteristic forms with which we are now so familiar. His drawings, and also those of some of the other observers, give the first indications of the appearance of the central branch in the Y, so called by Secchi.

1864. Dawes detected eight or ten of the canals.

1867. Huggins detected lines due to the presence of water vapor in the spectrum of Mars.

1867. Proctor determined the period of rotation of Mars within 0.1 second.

1877. Hall discovered the two satellites of Mars.

1877. Green made a very excellent series of drawings of the planet, superior to anything which had preceded them.

1877. Schiaparelli made the first extensive triangulation of the surface of the planet, and added very largely to the number of known canals.

1879. Schiaparelli detected the gemination of *Nilus*,—the first known double canal.

1882. Schiaparelli discovered numerous double canals, and announced that the appearance formed one of the characteristic phenomena of the planet.

The most reliable confirmation of this phenomenon hitherto reported has come from Perrotin of Nice, and A. Stanley Williams in England. If Schiaparelli's theory is correct, that the duplication occurs only between the spring and autumn equinoxes of the northern hemisphere, the last opportunity to witness it was in 1890, and the next will be in January and February of 1895, unless the planet proves to be too remote at that period.

Very few of Schiaparelli's writings have ever been translated into English, and none so far as I know, hitherto, without the intervention of some other language, such as German or French. The following translation is from "Natura ed Arte" for February 15, 1893. It gives the latest expression of his views upon the periodical inundations experienced by the planet, upon the nature of the seas, the canals, and the gemination of the latter.

LOWELL OBSERVATORY, Flagstaff, Arizona,
August 25, 1894.

THE PLANET MARS.

GIOVANNI SCHIAPARELLI.

*Many of the first astronomers who studied Mars with the telescope, had noted on the outline of its disc two brilliant white spots of rounded form and of variable size. In process of time it was observed that whilst the ordinary spots upon Mars were displaced rapidly in consequence of its daily rotation, changing in a few hours both their position and their perspective, that the two white spots remained sensibly motionless at their posts. It was concluded rightly from this, that they must occupy the poles of rotation of the planet, or at least must be found very near to them. Consequently they were given the name of polar caps or spots. And not without reason is it conjectured, that these represent upon Mars that immense mass of snow and ice, which still to-day prevents navigators from reaching the poles of the Earth. We are led to this conclusion not only by the analogy of aspect and of place, but also by another important observation.

* * * * * * * * * * * * *

As things stand, it is manifest, that if the above mentioned white polar spots of Mars represent snow and ice, they should continue to decrease in size with the approach of summer in those places, and increase during the winter. Now this very fact is observed in the most evident manner. In the second half of the year

1892 the southern polar cap was in full view; during that interval, and especially in the months of July and August, its rapid diminution from week to week was very evident, even to those observing with common telescopes. This snow, (for we may well call it so,) which in the beginning reached as far as latitude 70°, and formed a cap of over 2000 kilometers (1200 miles) in diameter, progressively diminished, so that two or three months later little more of it remained than an area of perhaps 300 kilometers, (180 miles) at the most, and still less was seen later in the last days of 1892. In these months the southern hemisphere of Mars had its summer; the summer solstice occuring upon October 13. Correspondingly the mass of snow surrounding the northern pole should have increased; but this fact was not observable, since that pole was situated in the hemisphere of Mars which was opposite to that facing the Earth. The melting of the northern snow was seen in its turn in the years 1882, 1884 and 1886.

These observations of the alternate increase and decrease of the polar snows are easily made, even with telescopes of moderate power, but they become much more interesting and instructive when we can follow assiduously the changes in their more minute particulars, using larger instruments. The snowy regions are then seen to be successively notched at their edges; black holes and huge fissures are formed in their interiors; great isolated pieces many miles in extent stand out from the principal mass, and dissolving, disappear a little later. In short, the same divisions and movements of these icy fields present themselves to us, at a glance, that occur during the summer of our own arctic regions, according to the descriptions of explorers.

The southern snow, however, presents this peculiarity, that the center of its irregularly rounded figure does not coincide exactly with the pole, but is situated at another point, which is nearly always the same, and is distant from the pole about 300 kilometers (180 miles) in the direction of the *Mare Erythraeum* From this we conclude that when the area of the snow is reduced to its smallest extent, that the south pole of Mars is uncovered; and therefore perhaps, the problem of reaching it upon this planet is easier than upon the Earth. The southern snow is in the midst of a huge dark spot, which with its branches occupies nearly one third of the whole surface of Mars, and is supposed to represent its principal ocean. Hence the analogy with our arctic and antarctic snows may be said to be complete, and especially so with the antarctic one.

The mass of the northern snow-cap of Mars is on the other

hand centered almost exactly upon its pole. It is located in a region of yellow color, which we are accustomed to consider as representing the continent of the planet. From this arises a singular phenomenon which has no analogy upon the Earth. At the melting of the snows, accumulated at that pole during the long night of ten months and more, the liquid mass produced in that operation is diffused around the circumference of the snowy region, converting a large zone of surrounding land into a temporary sea, and filling all the lower regions. This produces a gigantic inundation, which has led some observers to suppose the existence of another ocean in those parts, but which does not really exist in that place, at least as a permanent sea. We see then, (the last opportunity was in 1884), the white spot of the snow surrounded by a dark zone, which follows its perimeter in its progressive diminution, upon a circumference ever more and more narrow. The outer part of this zone branches out into dark lines, which occupy all the surrounding region, and seem to be distributary canals, by which the liquid mass may return to its natural position. This produces in these regions very extensive lakes, such as that designated upon the map by the name of *Lacus Hyperboreus*; the neighboring interior sea called *Mare Acidalium* becomes more black, and more conspicuous. And it is to be remembered as a very probable thing, that the flowing of this melted snow is the cause which determines principally the hydrographic state of the planet, and the variations that are periodically observed in its aspect. Something similar would be seen upon the Earth, if one of our poles came to be located suddenly in the center of Asia or of Africa. As things stand at present, we may find a miniature image of these conditions in the flooding that is observed in our streams at the melting of the Alpine snows.

Travellers in the arctic regions have frequent occasion to observe how the state of the polar ice at the beginning of the summer, and even at the beginning of July is always very unfavorable to their progress. The best season for exploration is in the month of August, and September is the month in which the trouble from the ice is the least. Thus in September our Alps are usually more practicable than at any other season. And the reason for it is clear, the melting of the snow requires time, a high temperature is not sufficient, it is necessary that it should continue, and its effect will be so much the greater, as it is the more prolonged. Thus, if we could slow down the course of our seasons, so that each month should last sixty days instead of

thirty, in the summer in such a lengthened condition, the melting of the ice would progress much further, and perhaps it would not be an exaggeration to say that the polar cap at the end of the warm season would be entirely destroyed. But one cannot doubt in any case, that the fixed portion of such a cap would be reduced to much smaller size, than we see it to-day. Now this is exactly what happens in Mars. The long year, nearly double our own, permits the ice to accumulate during the polar night of ten or twelve months, so as to descend in the form of a continuous layer as far as parallel 70°, or even further. But in the day which follows of twelve or ten months, the Sun has time to melt all, or nearly all, of the snow of recent formation, reducing it to such a small area, that it seems to us no more than a very white point. And perhaps this snow is entirely destroyed, but of this there is at present no satisfactory observation.

Other white spots of a transitory character, and of a less regular arrangement are formed in the southern hemisphere, upon the islands near the pole, and also in the opposite hemisphere, whitish regions appear at times surrounding the north pole, and reaching to 50° and 55° of latitude. They are perhaps transitory snows, similar to those which are observed in our latitudes. But also in the torrid zone of Mars are seen some very small white spots more or less persistent, amongst others one was seen by me in three consecutive oppositions (1877-1882) at the point indicated upon our chart by longitude 268°, and latitude 16° north. Perhaps we may be permitted to imagine in this place the existence of a mountain capable of supporting extensive ice-fields. The existence of such a mountain has been supposed also by some recent observers, founded upon other facts.

As has been stated, the polar snows of Mars prove in an incontrovertable manner, that this planet, like the Earth, is surrounded by an atmosphere capable of transporting vapor from one place to another. These snows are in fact precipitations of vapor, condensed by the cold, and carried with it successively. How carried with it, if not by atmospheric movement? The existence of an atmosphere charged with vapor has been confirmed also by spectroscopic observations, principally those of Vogel; according to which this atmosphere must be of a composition differing little from our own, and above all *very rich in aqueous vapor.* This is a fact of the highest importance, because from it we can rightly affirm with much probability, that to water, and to no other liquid is due the seas of Mars and its polar snows. When this conclusion is assured beyond all doubt, another one

may be derived from it, of not less importance,—that the temperature of the Arean climate, notwithstanding the greater distance of that planet from the Sun, is of the same order as the temperature of the terrestrial one. Because, if it were true, as has been supposed by some investigators, that the temperature of Mars was on the average very low, (from 50° to 60° below zero!) it would not be possible for water vapor to be an important element in the atmosphere of that planet, nor could water be an important factor in its physical changes; but would give place to carbonic acid, or to some other liquid whose freezing point was much lower.

The elements of the meteorology of Mars seem then to have a close analogy to those of the Earth But there are not lacking, as might be expected, causes of dissimilarity. From circumstances of the smallest moment, nature brings forth an infinite variety in its operations. Of the greatest influence must be the different arrangement of the seas and the continents upon Mars, and upon the Earth, regarding which, a glance at the map will say more than would be possible in many words. We have already emphasized the fact of the extraordinary periodical flood, which at every revolution of Mars inundates the northern polar region at the melting of the snow. Let us now add that this inundation is spread out to a great distance by means of a network of canals, perhaps constituting the principal mechanism (if not the only one) by which water (and with its organic life) may be diffused over the arid surface of the planet. Because on Mars it rains very rarely, *or perhaps even, it does not rain at all.* And this is the proof.

Let us carry ourselves in imagination into celestial space, to a point so distant from the Earth, that we may embrace it all at a single glance. He would be greatly in error, who had expected to see reproduced there, upon a great scale, the image of our continents with their gulfs and islands, and with the seas that surround them, which are seen upon our artificial globes. Then without doubt the known forms, or part of them, would be seen to appear under a vaporous veil, but a great part (perhaps one half) of the surface would be rendered invisible, by the immense fields of cloud, continually varying in density, in form and in extent. Such a hindrance, most frequent and continuous in the polar regions, would still impede nearly half the time the view of the temperate zones, distributing itself in capricious and ever varying configurations. The seas of the torrid zone would be seen to be arranged in long parallel layers, corresponding to the

zone of equatorial and tropical calms. For an observer placed upon the Moon, the study of our geography would not be so simple an undertaking as one might at first imagine.

There is nothing of this sort in Mars. In every climate, and under every zone, its atmosphere is nearly perpetually clear, and sufficiently transparent to permit one to recognize at any moment whatever, the contours of the seas and continents, and more than that, even the minor configurations. Not indeed that vapors of a certain degree of opacity are lacking, but they offer very little impediment to the study of the topography of the planet. Here and there we see appear from time to time a few whitish spots, changing their position and their form, rarely extending over a very wide area. They frequent by preference a few regions, such as the islands of the *Mare Australe*, and on the continents, the regions designated on the map with the names of *Elysium* and *Tempe*. Their brilliancy generally diminishes and disappears at the meridian hour of the place, and is reinforced in the morning and evening, with very marked variations. It is possible that they may be layers of cloud, because the upper portions of terrestrial clouds, where they are illuminated by the Sun, appear white. But various observations lead us to think that we are dealing rather with a thin veil of fog, instead of a true nimbus cloud, carrying storms and rain. Indeed it may be merely a temporary condensation of vapor, under the form of dew or hoar frost.

Accordingly, as far as we may be permitted to argue from the observed facts, the climate of Mars must resemble that of a clear day upon a high mountain. By day a very strong solar radiation hardly mitigated at all by mist or vapor, by night a copious radiation from the soil towards celestial space, and because of that a very marked refrigeration. Hence a climate of extremes, and great changes of temperature from day to night, and from one season to another. And as on the Earth at altitudes of 5,000 and 6,000 meters (17,000 to 20,000 feet), the vapor of the atmosphere is condensed only into the solid form, producing those whitish masses of suspended crystals, which we call cirrus clouds, so in the atmosphere of Mars, it would be rarely possible (or would even be impossible) to find collections of cloud capable of producing rain of any consequence. The variation of the temperature from one season to another would be notably increased by their long duration, and thus we can understand the great freezing and melting of the snow, which is renewed in turn at the poles at each complete revolution of the planet around the Sun.

(TO BE CONTINUED.)

ELECTRICAL CONTROL OF EQUATORIALS IN PHOTOGRAPHY.

W. C. GURLEY.

In the January number of this journal appeared an article by Professor W. H. Pickering upon the subject of "Telescope Mountings and Domes," in which the author calls attention to a method employed at Greenwich for correcting the rate of the Standard Mean Time clock in that Observatory, and suggests that a somewhat similar arrangement might be advantageously adopted for the correction of driving clocks of equatorials when used for photographic work.

Acting upon the suggestion of Professor Pickering the writer during the past winter has made a number of experiments looking to this end.

The clock placed under electric control was one attached to the refractor of Marietta College Observatory, an exceptionally well made Bond Spring Governor by the Howards of Boston.

Much abuse has been heaped upon this time-honored form of driving-clock regulation, but it has always seemed to the writer that for accuracy of results and thorough reliability this governor has never been surpassed.

In the final trial of this electrical method of control two permanent bar magnets, three inches long, five eighths of an inch wide, and one quarter inch thick were attached vertically to opposite sides of the pendulum bob—their north and south poles reversed; half an inch below the lower end of the permanent magnets, two electro-magnets were placed and so wound that a current sent in a given direction would render them north and south seeking poles respectively—Four Leclanché cells connected in series, a switch for reversing the battery current, a pear push button included in the circuit, and brought to the eye-end of the telescope, completed the arrangement.

It was found upon sending a current in such a direction as to cause the electro-magnets to assume unlike polarity with the permanent magnets above them, that the clock would be accelerated two beats, or one second in sixty. A reverse current producing an opposite effect, and causing the clock to lose one second per minute. All this can be done easily, without jar or tremor, and is completely under the control of the observer.

Of course the change of position of the image of an object upon the sensitive plate due to refraction in declination is not affected by this control, but in case of the smaller instruments this correction can be applied by means of the clamp and tangent.

PHOTOGRAPH OF SWIFT'S NEBULA IN MINOCEROS. N. G. C. 2237.*

E. E. BARNARD

I send for reproduction in ASTRONOMY AND ASTRO-PHYSICS an enlargement of my photograph of this mixture of nebulosity and stars.

The picture was made with the 6-inch Willard lens, 1894, January 11^d 7^h 47^m to 11^h 3^m standard Pacific Time and is enlarged 2.8 times.

It is a fine example of the free mixture of stars and nebulosity to which I have called attention in my paper on "Photographic Nebulosities and Star Clusters Connected with the Milky Way" in ASTRONOMY AND ASTRO-PHYSICS for March, 1894.

Though the cluster (G. C. 1420) is apparently involved bodily in this nebula it will be readily seen that there is no tendency of the nebulosity to condense about the individual stars. It will be interesting to compare this picture with any recent photograph of the Pleiades, that for instance of Dr. Wilson, recently printed in ASTRONOMY AND ASTRO-PHYSICS. This will show the striking contrast between the two classes of nebulous clusters.

In the Pleiades the nebulosity is strongly condensed about the individual bright stars, while in the present picture there is no such tendency to condensation—the stars are seemingly freely mixed with the nebulosity. Of course it is possible their apparent intermixture may be due only to projection of the stars on the nebulosity, but this is highly improbable.

I send with this also for reproduction my sketch of this object that appeared in *A. N.* Vol. 122. This is to be reduced to $\frac{1}{2\frac{1}{4}}$ its size to be on the same scale as the photograph. For completeness, I will also quote what I have previously written about this nebula in ASTRONOMY AND ASTRO-PHYSICS for March, 1894.

"In *A. N.* 2918 I have given an account, along with a sketch, of a large nebulous ring enclosing the cluster G. C. 1420, the nebula itself being N. G. C. 2237. The place of G. C. 1420 for 1860.0 is 6^h 23^m 29^s + 5° 2′.5.

The nebula (2237) was discovered by Swift very many years ago, and was independently found by me in January, 1883.

The sketch referred to was made with the 12-inch in 1889.

I have recently (Jan. 9, 1894,) secured a fine photograph of this object with the Willard lens. As stated in *A. N.* 2918, from its

* Communicated by the author

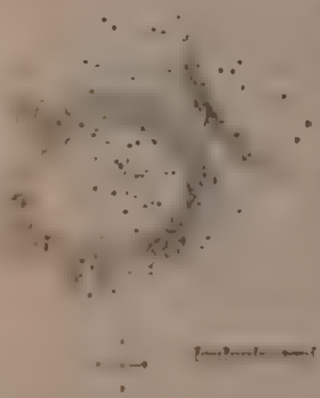
PLATE XXIa.

PHOTOGRAPH OF SWIFT'S NEBULA IN MONOCEROS,

1894. January 11, $7^h\,47^m - 11^h\,3^m$ S. P. T.

Made with the 6 in. Willard lens of the Lick Observatory, by E. E. Barnard.

diffused nature this nebula is specially suited for photography. This photograph verifies the statement most emphatically and shows how utterly impossible it is to adequately deal with such an object visually.



I will quote from my previous article on this subject in *A. N.* 2918, in speaking of its appearance with the 12-inch in 1889:

"What I had seen previously and what Swift had sketched, was simply a brightish knot in a vast nebulous ring that entirely surrounded the cluster. By estimation the average outer diameter of the ring is 40′ and the inner diameter 20′. The inside of the ring is apparently free of nebulosity, the stars of the cluster shining on a perfectly dark sky. The outer edge of the ring is somewhat diffused and irregular, some projections occurring near the following portion. The inner edge is more definite and especially so following—it is less definite in the preceding part. In the north preceding section of the ring are several knots, the largest of which, *a*, is the one previously seen by Swift and myself. I am not sure that there is not a very small break in the continuity of the ring at the point *b*. South following the ring and close to it is the nebulous section of a large ellipse which seems to be a portion of another great ring; I am not sure that this is not connected with the first by a nebulous strip."

Comparing the photograph with my sketch, I find my sketch is correct so far as it goes, but with the 12-inch I had grasped only the brighter details—the great mass of it not being seen.

The photograph shows the nebula to be about 1° in diameter and very irregular in brightness and outline. It is a mass of unequally condensed nebulosity involving the star cluster and specially heavy north of the bright stars. The nebulous knots or condensations, shown in my sketch, are conspicuous on the photographs, as is also the "nebulous section of a large ellipse," which is connected with the main mass—the full extent of this section was shown in the sketch.

The photograph shows that there is no nebulosity—or if any it is very feeble—immediately about most of the bright stars. They apparently shine in a vacant space in the south part of the nebula.

The entire object seems to be definitely terminated and to leave no suggestion of a greater extent being revealed through a prolonged exposure.

One degree south of the center of the nebula, and free of it, and following about ¼°, is a very thin nebulous strip 10′ or 12′ long extending north and south with a faint star in its south end, like a slender comet with a nucleus.

THE REGION OF LACUS SOLIS ON MARS.

J. M. SCHAEBERLE.

During some unusually fine seeing on the morning of Sunday Sept. 2, 1894, at 2:30 A. M., Locus Solis was very clearly shown to be composed of three separate areas. Each area was very dark. The two preceding areas are elongated in a north and south direction and are enveloped and connected by a penumbral shade; the third area (following) is round and quite disconnected from the general mass. The five very small and black circular areas nearer to the planet's equator (shown in the sketch inclosed within the bounding square) were connected by an intensely black and very narrow continuous line which passed centrally through each area, but did not extend to the familiar black circular area from which several so-called "canals" radiate —only the usual faint and diffused marking forming the continuation of the line.

In this connection attention should be called to the fact that in June, 1890, Schiaparelli saw Locus Solis divided into two parts the advancing area being the smaller of the two. (See *La Planéte Mars*, by Camille Flammarion, page 475.)

LICK OBSERVATORY, Sept. 8, 1894.

CORRECTIONS TO THE N.G.C. OF NEBULÆ. *H. Kobold.*

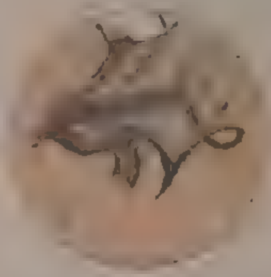
It is suggested that N.G.C 3760, only seen once by d'Arrest, and not found again nor seen by anyone else, may be affected by an error of one hour in R. A. and be = 3301 = II. 46. The group 3745, 46, 48, 50, 51, 53, 54, should all be corrected by + 1^m 32^s and − 15′.9. They were found by Copeland, with Lord Rosse's telescope, but an error was afterwards made in identifying the comparison star.—*A. N.* 3241.

ASTRONOMY AND ASTRO-PHYSICS.

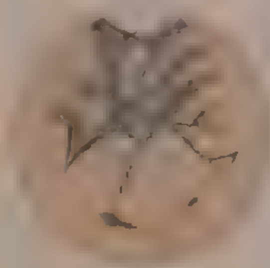
PLATE XXII.

1892 Aug. [illegible] P. s. t. λ [illegible], β [illegible]

1892 Aug. [illegible] P. s. t. λ [illegible], β [illegible]



1892 Aug. 24 [illegible] P. s. t. λ [illegible], β [illegible]



1892 Aug. 20, [illegible] P. s. t. λ [illegible], β [illegible]

[illegible] 1894 Sept. [illegible]
1892 Aug. 11, [illegible] P. s. t. λ [illegible], β [illegible]

1892 Aug. 8, [illegible] P. s. t. λ [illegible], β [illegible]

1892 Aug. 7, [illegible] P. s. t. λ [illegible], β [illegible]

1892 Aug. 7, [illegible] P. s. t. λ [illegible], β [illegible]

1892 [illegible] P. s. t. λ [illegible], β [illegible]

PLATE XX. — DRAWINGS OF MARS 1892

By Professor SCHAEBERLE.

PLATE XXIII

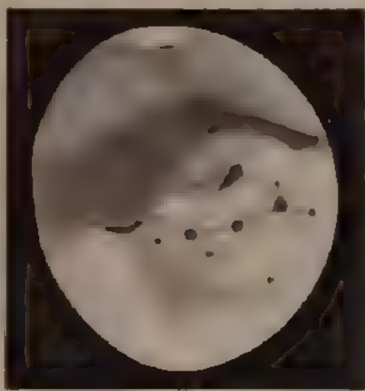


FIG. 1. July [illegible] 1894 [illegible]

Power 420 to 60 Seeing [illegible]

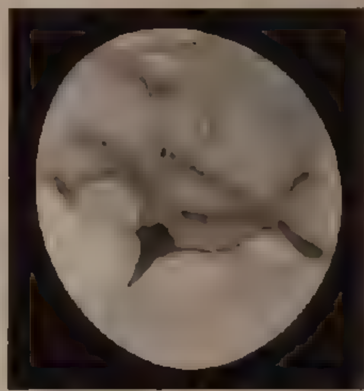


FIG. 2. Aug. [illegible] 1894 [illegible]

Power 420 Seeing [illegible]

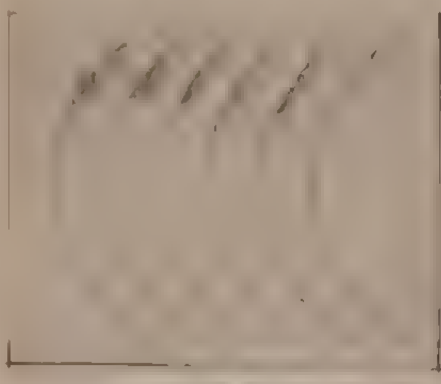


FIG. 3. A series of [illegible] marked elevations and [illegible]
All are drawn [illegible] to scale [illegible] Aug. [illegible] 1894 Seeing [illegible]

FIG. 4. July [illegible] 1894 [illegible]

Power 420 Seeing [illegible]

DRAWINGS OF MARS

BY WILLIAM H. PICKERING, LOWELL OBSERVATORY, FLAGSTAFF, A. T.

Scale $\frac{1}{[illegible]}$ or 1mm = 160 km

Astronomy and Astro-Physics, No. 128

MARS.*

PERCIVAL LOWELL.

During the past three months Mars has been observed here every night with but few exceptions; and although it is still (Sept. 10th) a month and a half to opposition the results already obtained are very encouraging, amply confirming the importance of choosing as good air as possible for an Observatory site.

In this preliminary account of some of them I may with a certain propriety begin, so to speak, at the flood, inasmuch as the prediction which I ventured to make in my last paper with regard to the Martain vast spring freshet has already been fulfilled —although whether it be a surface freshet or an aerial one still remains in a degree doubtful. But the fact that in the planet's southern hemisphere at this season (from two months after the vernal equinox to the summer solstice) a wholesale transference of water takes place from the pole to the equator, is practically beyond question. Whether what we see be the water itself or only the effects of it is more uncertain.

On referring back to my previous paper it will be seen how large an area the dark regions then occupied and how conspicuous by their absence were those singular, tilted peninsulas that are so generally represented connecting the continents with the islands to the south. At that time one continuous belt of bluish-green stretched unbroken from the Hour-glass Sea to the columns of Hercules or rather to where this pass should have been, for it was not visible. Now the continuity is cut. Hesperia has reappeared as will be seen from one of my drawings (Plate XXV, Fig. 1) as also in the other plates by Professor Pickering and Mr. Douglass and it has done this in just the way we should expect it to show were it land drying off by a sinking of the general water level. For it will be noticed that a strait still severs it from the coast. Simultaneously, the region formerly occupied by the polar sea and the region to the north of it from having been blue, has now become for the most part reddish yellow. That this reappearance of Hesperia and change of color of the regions farther south is not due to increasing distinctness of vision consequent upon the nearer approach of the two planets is evident at a glance from this drawing and the earlier one. Had Hesperia been then of anything like the brightness it is now it could not have been invisible. Furthermore Eridania is at present one of the brightest parts of the disk not only as it comes round into

* Communicated by the author.

The second kind of irregularities are projections or notches such as are visible upon the lunar terminator; that the Martian ones are much less pronounced. They are bably due to mountains which seem to be of no great he The first of these was observed by Mr. Douglass on June An especially prominent one he noted on August 19th. It sisted of a projection flanked by a long shadow cutting int planet obliquely. He measured the shadow's length at Taking the obliquity into account this seems to imply a the length of whose projection would be about .2″. It is cult to say how much of this is due to irradiation; especial each observer differs. The best tests I have been able to give a probable average of about five-sevenths of a tenth second of arc with the power then applied, about 640. C the terminal projection of this range therefore .13″ we hav its height about 3,700 feet. But the smallness of the qua measured and the uncertainty of the factor of irradiation re the result largely indefinite.

A consequence of the slope on the effect of these mounta interesting. For an elevation need not appear as such. would show as a projection on the nether side of the termi would appear as a depression on the hither one.

Interesting plateaus were observed on two occasions by fessor Pickering, one of which figures in his drawing (Plate X Fig. 2). The other, a quite similar one, of which after see I computed the position, turns out to be in an interesting for it lies in Phætontis not far from the columns of He which thus seem to have been most appositely named. plateaus rise abruptly, are surprisingly level on top, and at about the same height, a height which from the reduced urements does not probably exceed 2,600 feet.

On Mars the second kind of irregularity is less common the first and the elevations indicated apparently never wh should call high. We may therefore conclude that the Ma surface is, as compared with our terrestrial one, relatively t

Certain whitish patches have been observed on the planet by Professor Pickering on August 16 and subsequently se times by both Professor Pickering, Mr. Douglass and m Professor Pickering calls them clouds, a nomenclature in I do not wholly concur. To my eye appearances he thu signates are of two kinds. The one, certain whitish, fl patches not far from the pole, may possibly be cloud, the tainly present a peculiar aspect, not like snow nor yet like *firma*. No motion, however, has been seen in them. The o

PLATE XXIV.

S

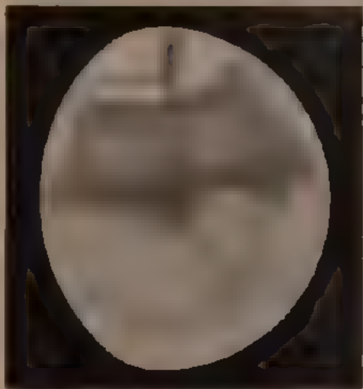


Fig. 1. June 13, 1894, 16h 15m.
Long. 165°, Diam. 9″.6. Seeing 7 to 8.

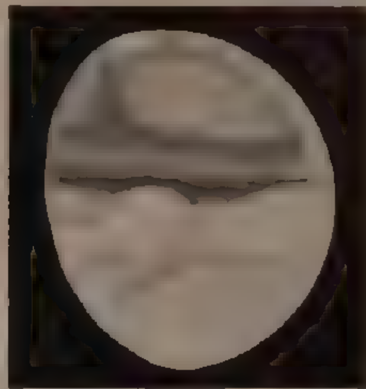


Fig. 2 July 22, 1894, 17h 5m.
Long. 154°, Diam. 12″.62. Seeing 4 to 7

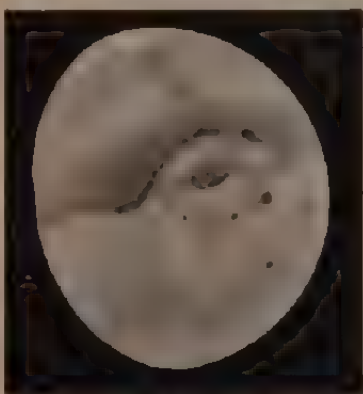


Fig. 3 July 26, 1894, 16h. 07m
Long. 70°, Diam. 13″.7 Seeing 4 to 10

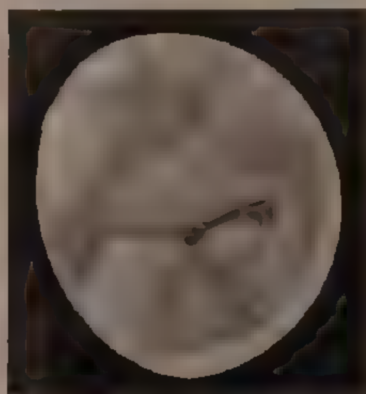


Fig. 4 Aug. 11, 1894, 18h. 22m
Long. 338°, Diam. 14″.8. Seeing 4 to 8

Fig. 6 Aug. 11, 1894, 16h. 02m
Seeing 6 to 8.

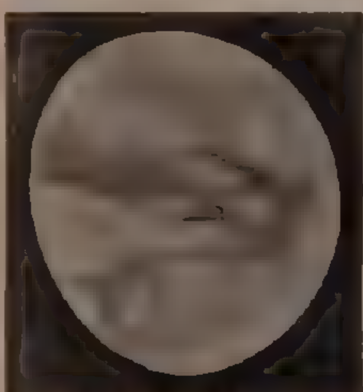


Fig. 5. Aug. 21, 1894, 16h. 56m
Long. 223° Diam. 16″.0 Seeing 6 to 8

N

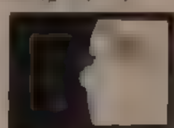


Fig. 7 Aug. 19, 1894, 13h. 21m
Near Mare Sirenum.

DRAWINGS OF MARS

BY A. E. DOUGLASS, LOWELL OBSERVATORY, FLAGSTAFF, A. T.

Scale $\frac{1}{[illegible]}$ or 1 mm. = 160 km.

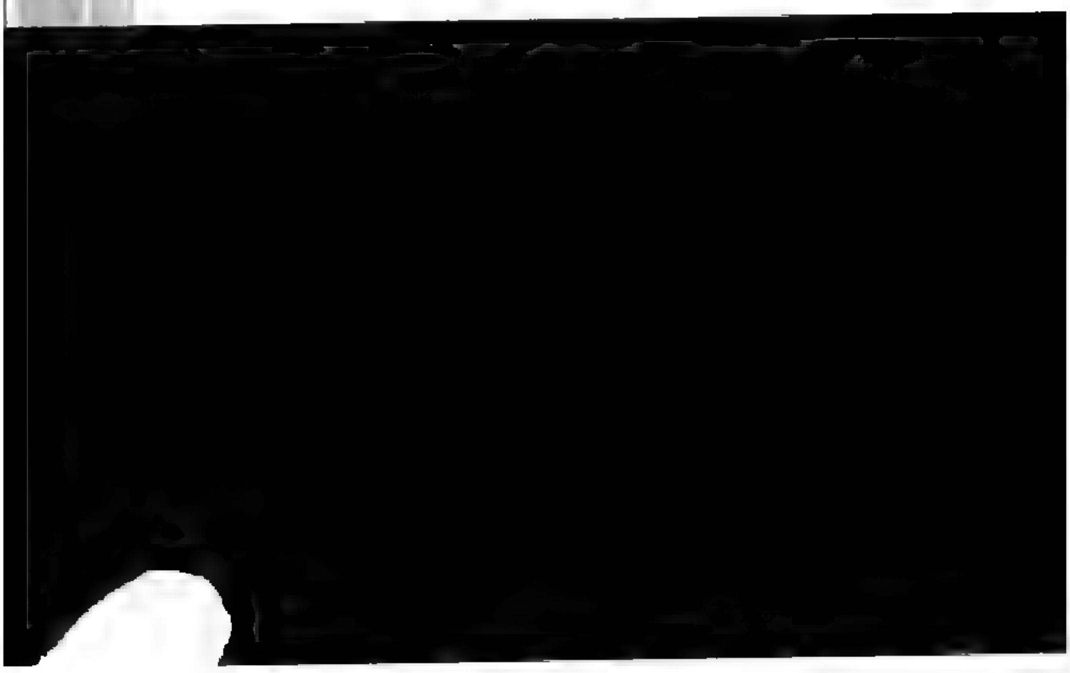


PLATE XXV.

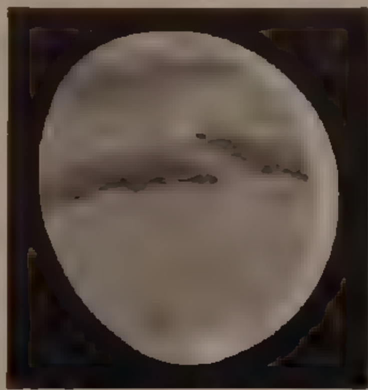


FIG. 1. Aug. 20, 1894. 15h 25m.
Long. Schia. 225°, Marth 210°, Power 370

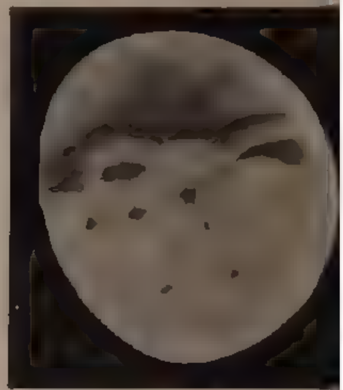


FIG. 2. Aug. 29, 1894. 13h
Long. Schia. 100°, Marth 100°, P

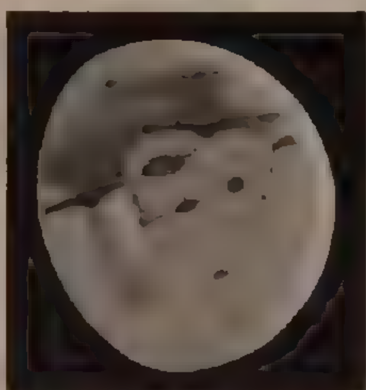


FIG. 3. Aug. 31, 1894. 13h 45m.
Long. Schia. 88°, Marth 8 Power 420

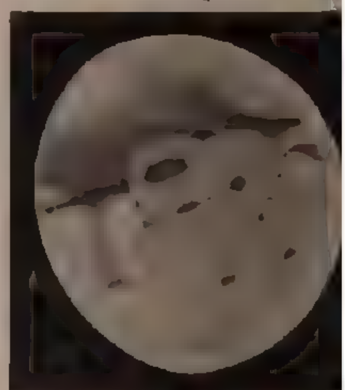


FIG. 4. Lake of the Sun. Lo
Aug. 27 - Sept. 3, 1894.

DRAWINGS OF MARS

BY PERCIVAL LOWELL, LOWELL OBSERVATORY, FLAGSTAFF, A. T.

Scale [illegible] or 1 mm. = 160 km.

are merely certain bright spots on the general surface of the planet. These to me are not whitish but yellowish and will probably do very well for the more arid, dried up tops of the land They likewise do not move, and furthermore show always the same appearance day after day as regularly as their regions come round. Many of them were equally conspicuous at previous oppositions and have been chronicled by various observers. Their contours are neither shifty nor indistinct but as sharp-cut as those of any other region. Elysium, Eridania and the islands to the south of it, the western part of Memnonia and the land of Ophir are of this category. Of these Elysium is perhaps the most vivid, and Eridania the least so. If conspicuous aridity be the cause of this their brilliancy, Elysium would seem antipodally named.

Most suggestive of all Martian phenomena are the canals. Were they more generally observable, the world would have been spared much scepticism and more theory. They may, of course, not be artificial but observations here indicate that they are; as will I think, appear from the drawings. For it is one thing to see two or three canals and quite another to have the planet's surface mapped with them upon a most elaborate system of triangulation.

In the first place they are at this season bluish-green, of the same color as the seas into which the longer ones all eventually debouch In the next place they are almost without exception geodetically straight, supernaturally so, and this in spite of their leading in every possible direction. Then they are of apparently nearly uniform width throughout their length. What they are is another matter. Their mere aspect, however, is enough to cause all theories about glaciation fissures or surface cracks to die an instant and natural death.

But it is their singular arrangement that is most suggestively impressive. They have every appearance of having been laid out on a definite and highly economic plan (Plate XXV, Figs. 2, 3, 4). They cut up the surface of the planet into a net-work of triangles instantly suggestive of design. What is more at each of the junctions there is apparently a dark spot. This feature seems to be invariable as on closer approach, junction after junction turns out to have one. The larger of these appear on Schiaparelli's chart as lakes. But there would seem to be a small infinity of smaller ones. A short half hundred of them were seen at Arequipa in 1892 and others have recently been detected here. For example an important new canal, which runs from the western end of the sea of the Sirens to Ceraunius and which in view of its point of

departure I am tempted to call the Ulysses, passes through t of these small dark spots on the way, one at each junction. of these was seen at Arequipa and elsewhere in 1892; the o two are new—by which I mean that they have not been s not that they did not exist, before. The region of the Lake of Sun is especially fertile in canals. In Plate XXV, Fig. 4, will be 31 of them counting each line between junctions as a separ canal. Of these 17 are among those in Schiaparelli's ch while 14 are not. Of the 12 lakes in the figure, 5 are not do on his chart. This is not in general due to change

Changes, however, there apparently have been after all due count has been made for difference of observations and of dr ing. First and foremost the Golden Chersonese has vanish the land of Ophir now forms the continental coast-line. Secon Icaria has entirely altered in contour, resembling now an open about the Phœnix lake for pivot. Phætontis has shrunk to third of its former width—as represented in Schiaparelli's ch Eosphoros no longer enters Phœnix lake at the point oppo Pyriphlegethon but farther to the west. But the strangest tr formation of all is that of the Phasis, which has apparently ligingly become two (not geminated in the technical sense) suit both the old and the new state of things. There is no canal running in the same direction as the old Phasis but no the southern end of Phætontis and there is another one runn to the southern end of Phætontis, but not in the same direct as heretofore. This attempt to carry out two apparently imp ant ends by self-multiplication is not a common characteristi inanimate nature—a point worth consideration.

To my eye the Araxes is perfectly straight although to h Professor Pickering and Mr. Douglass it appears curved. owing to my having observed pretty unintermittently the reg about the lake of the Sun at this last presentation neither of th observers has yet had the chance to see the new upper Pha debouching at the southern end of Phætontis and it is, perh to this canal confused with the Araxes that the observed cu effect is due.

In an interesting drawing (Plate XXIV Fig. 6), Mr. Doug gives a couple of canals cutting each other at right angles u Oenotria just as that island began to show differentiated f the dark area. This is a second rectangular instance of ca arrangement for an island too small for more general subd sion; Hellas being the first

Just as this account leaves my hands, Mr Douglass has (Sept. 10th) Deimos and Phobos at elongation.

Lowell Observatory, Sept. 11th.

Astro-Physics.

THE SPECTRUM OF α HERCULIS.*

T. E. ESPIN

The spectrum has been frequently observed, and is given by Dunér in his memoir *Sur les étoiles à spectres de la troisième classe*, as the second form of a third-type star. Secchi observed it in detail with the following results (Birmingham, *Red Stars*, First edition, p. 321):

"Magnificent columns, as usual; columns all resolved into the finest lines; vivacity of light extraordinary, though the star appears scarcely 3 mag. to the naked eye; 10th, 15th July, 1868 *Mem. II.* Singular and magnificent object, appearing like a series of convex columns illuminated by the Sun. The lines that separate the columns are profoundly black, and wonderfully distinct. The greatest light appears towards one side of the interval between the black lines, and there is a gradation of shade as in columns represented in a drawing; measuring easy; with the compound spectroscope magnesium line determined; sodium probable but not certain; only feeble indications of decomposition with simple spectroscope and power 400; but decided decomposition into fine lines, with compound spectroscope and power of 600—*Catalogo*, etc., 1867. June, 1870: No decomposition with objective prism; tried cylindrical lens with no other result; impossible to believe it a metallic spectrum.—*Sugli Spettri*, etc., Marzo, 1872. Dunér says "the bands 1-10 are excessively large and black." He also made measures of the bands, and a drawing of the spectrum is given. In coming across the star in 1894, June 29, I was so much struck by the additional detail that I made some notes on the subject. The instrument used was the 17¼-inch Calver equatorial. The spectroscope consisted of a compound prism 3 inches long before the eyepiece, without any slit or cylindrical lens. A second prism, the length of which is 4¼ in., was also used. The prisms can be made to slide away from the eye piece, thus increasing the dispersion. Powers of 200 and 500 were used.

The following are my results.

1894, June 29. Band 4 is fainter than 5, and 8 deeper than 7. Band 7 is divided by brighter parts, not lines. Band 8 is double as in Mira. Band 9 is triple, and the space from 10 to 9 is occu-

* Communicated by the author.

pied by bright and dark bands. Band 1 is divided into t
bands 2 and 3 seem resolved into lines. Band 1 is deeper th
and 3. Star pale orange. The results were so interesting th
made preparations for drawing the spectrum at the first op
tunity. For this purpose the position of Dunér's bands w
carefully laid down on paper. On the next night, June 30, th
companying drawing was made. The following are my obse
tions on that night:

Band 1 is double; 2 and 3 are very dark; 5 is, I believe, dou
6 and 7 are separated by a bright band; between 5 and 6 t
are certainly two bright bands, and probably more. The s
seems occupied with bright bands and dark shadings. On
more refrangible side of 7 is a faint dark line. Band 8 is div
into two. The second band is almost as strong as the first
bright part between the two is not very conspicuous. At ba
there are two bright parts and probably more. At band 10
two bright flutings of about equal intensity, and the whole s
between 9 and 10 seems full of bright and dark spaces, prob
at least six. Beyond this the spectrum is not well seen but se
to be broken up into numerous bright and dark bands.

The drawing, although rough, will serve to indicate the
eral appearance of the spectrum. The additional detail was
down by estimation as I have no apparatus for making di
measurements. It would, however, appear that the spectrum
α Herculis and probably of many of the stars of Type III is
more complex than has been generally supposed.

TOWLAW, Darlington, 1894, Aug. 4th.

STELLAR PHOTOMETRY.*

HENRY M. PARKHURST.

Properly speaking, the art of measuring light has not been
covered. We cannot subtract from the given light a determi
amount and subject the remainder to independent determinat
What is called measuring light consists in either reducing
given light in a known proportion, making it apparently eq
to another standard light, or in reducing the given light i
known proportion, making it too faint to be perceptible. B
these methods are subject to so many disturbing causes th

* Read at the Brooklyn Meeting of the American Association for the Adv
ment of Science. Communicated by the author.

PLATE XXVI.

10 9 8 7 6 5 4 3 2 1

THE SPECTRUM OF α HERCULIS. (1894, JUNE 30.)

ASTRONOMY AND ASTRO-PHYSICS, No. 138.

is not surprising that many astronomers are inclined to adhere to the ancient method of estimation, by following which Argelander in the northern hemisphere, Gould in the southern hemisphere, and Schönfeld in the intermediate region, have furnished us standard magnitudes for all the lucid stars, and all the brighter telescopic stars, of marvelous consistency. A series of twenty standards, half a magnitude apart, being firmly impressed upon the mind, the observer made his comparison of each star with the corresponding standard, with such interpolation of brightness as was practicable; and those who use their catalogues have many hundreds of thousands of stars to assist them in adapting their own estimations to the same system.

In a perfect photometry, each magnitude corresponds to a certain proportion of the light of the next brighter magnitude. A system in which each magnitude has a certain proportion of the light of the first magnitude, as in the astrometric scale of Sir John Herschel, or a corresponding system in which the first magnitude should be excessively faint, and the brighter stars represented by magnitudes of larger numbers, vary too much from the received scale and are too cumbrous for practical use. Sensation varies in geometric proporton; and a photometric scale founded upon geometric progression has many advantages. One of these is that it reduces the form of each quadrant of the light-curve of a regularly changing variable, nearly to a parabolic curve. But this is a subsidiary matter, the magnitudes accurately fixed in geometric progression being readily reduced to light ratios of equal precision. So far as precision is concerned, the ratio of the magnitude is unimportant. Pogson's ratio of 2½, or more accurately the number corresponding to the logarithm [0.4000] has been extensively used and is remarkably convenient. If a different ratio should be adopted by any observer, his results would only need to be multiplied by a constant to reduce them to Pogson's ratio, adding another constant to allow for difference of standard. Pogson's ratio has the merit of corresponding well with the scales or rather the scale of Argelander, Gould and Schönfeld, down to the 8th magnitude. Below this point there is a divergence in the estimation of different observers, increasing with fainter magnitudes, until they are entirely discordant. A change of ratio would not remedy this, while it would destroy the accordance with the brighter stars.

The fundamental question then is, Shall the photometric scale be fixed by unassisted estimation, or shall photometric apparatus be employed, to vary the light of the observed star, as a

means of assisting and correcting the estimation? Dr. Men hall, at a meeting of this Section some years ago, gave us a cellent illustration of the advantage of correcting estima even by means crude in the extreme. One of the earlier probl in geometry was the determination of π, the ratio of the cir ference to the diameter. It can be seen at a glance that π m be greater than 2 and less than 4; but estimation would not able us to say whether it is greater or less than 3; still to say how much greater than 3 it is. The estimation thousand or a million people, especially if they attempted agree, would not be materially better than that of one. Di ing the sum of the estimations by their number would not to increased accuracy. But by "tossing a stick" over a gratin method in which each individual trial was far less accurate t rough estimation, every additional toss adds to the accurac the determined ratio. Tossing it several thousand times he tained a good approximation to the value of π; and if the had been a mathematical line, and the grating of mathemat lines accurately placed, and if a microscope had been use doubtful cases, it is probable that millions of trials would carried the approximation much further. He carried it enough to ascertain that he had a personal bias in his est tions which finally became appreciable. Now in the mod dividing the photometric scale by unassisted estimation, the division creates a personal bias in favor of its repetition. H ing fixed upon a star of the 1st magnitude, and another of 9th, the astronomer desires to fix the 5th magnitude midway tween them. It would be superhuman to hit upon it exactly the first assumed 5th magnitude star is too faint, the tend will be to make the next also too faint; and if there is a pers bias that way, the more trials are made the more definitely standard will be settled at a point fainter than it should be. 3d magnitude is estimated midway between the 1st and th roneous 5th. Again an error of bias is introduced. And every other standard, there will be a bias. Another astronom using Argelander's stars, attempts to learn his scale; and ev his own bias should be in the opposite direction, he would ass it to be from his lack of experience, and would train his judgm to adopt Argelander's scale. As a final result the scale would as it undoubtedly is, consistent with itself, and yet there migh a large error in the uniformity of the scale. That is, the rati light might be different in different parts of the scale; and t would be no means of detecting it.

Suppose then we adopt photometric apparatus like the meridian photometer, in which the observed star is compared with the Pole-star, and the instrument gives the proportion of the light of the two stars when equality is apparently reached. We have here room for as much bias as before, in each individual observation. One observer may habitually make the right hand star the brighter, and another the fainter, in obtaining equality. But each individual would have an average bias, which would become more and more uniform with practice; and if the stars B, C, D, E, &c., were compared under precisely similar conditions with A, whatever bias there might be in the original comparisons, that would be eliminated when we compare the stars B, C, D, E, &c., with each other.

So in observing with the deflector, my own pet device, where the stars are reduced to invisibility with uniform illumination; if different stars are reduced to invisibility under precisely similar circumstances, whatever the point of invisibility may be, and however it may vary between one evening and another, the stars observed at the same time are free from personal bias, when compared with each other.

Other modes of assisting the judgment in like manner, if they have no inherent defect, producing systematic error, will give results continually approximating to perfection. The underlying question is whether systematic errors are guarded against; for a systematic error in a photometric instrument is as liable to mislead as a bias in estimation.

One source of systematic error in the use of the early method of diminishing apertures, and also in the use of the wedge, is the difference of illumination. As I have pointed out, (*H. C. O. Annals*, Vol. XXIX), a very large error results in the method of diminishing apertures, especially if a low magnifying power is employed, from the greater darkness of the field in the extinction of bright stars. So in the use of the wedge, the background, where the bright stars are extinguished, is much darker than where the faint stars are extinguished, and this is only partially compensated in determining the value of the wedge. This particular error does not affect observations with the meridian photometer or with the deflector. It has been cynically remarked of the Harvard observers, that their apparatus permitted an endless variety of errors, and that they had utilized them all. Perhaps the same may be said of my method. But whatever errors there may be in any method, so long as they are not systematic errors, they will tend to counterbalance each other in repeated observations.

In my judgment the most important source of error in photometric observations is from the irregularity of the sky. individual errors may be very large, but there is no tendency systematic error from this source. We have a remarkable p of the existence of this same error in Argelander's estimati where whole zones were affected. Proctor called attention the dark streaks following circles of declination, in his project of the Durchmusterung stars, and remarked that it was impo ble that they should actually exist in the heavens. The expla tion is easy. An average variation of .1 mag. in the stars served in any particular zone, would cause so many of the fain stars to disappear as to account for the zones so conspicu Certain zones were observed on one or two evenings when sky was unusually clear, and the adjoining zones on evenings w the sky was less transparent; the effect being that those zones served on the clearer nights, even if also observed once or tw on average nights, contain many more stars of the lower li than the adjoining zones. The same cause of error exists in Harvard photometric zones. There are very many of these s which were only observed twice, and if the sky near the pole h pened to be either more or less clear than in the observed zo the whole of the stars were affected with the difference. I satisfied that there is in many places a discrepancy of half a m nitude or more in the mean scale of magnitudes in the Harv Zone Catalogue, resulting from this cause.

My own observations have been purely differential, the c parison stars being almost invariably so close that no allow need to be made for difference of obscuration. But this ver cumstance has affected them to a great extent with the local rors of the Harvard Zones, from which the standards were ta There have been two checks. I have taken pains, especi where an error was suspected, to compare with other zone away, so as to divide the error. And wherever groups of st were compared with asteroids, those asteroids tended to red errors of standard.

Another source of local errors in the Harvard Zones, is to found in the correction for atmospheric obscuration. In photometric observations of 1869, when I independently dis ered the existence and general law of the atmospheric obscu tion, I ascertained that it varied on different evenings, someti being at least three times the average. In the Harvard Zones atmospheric obscuration has been assumed to be always of average value. Consequently, on any evening when it was tw

its average value, the whole zone of stars observed would have a correction applied only half as great as was necessary, and the whole zone of stars would be affected with a constant error equal to the whole amount of the obscuration correction applied.

A fruitful source of error in observing with the meridian photometer has been the misidentification of stars. I have found many apparent instances of this in the ordinary work of the meridian photometer, where the estimated magnitude of the star was known in advance and used as a guide to assist in the identification. Chandler has found that in the observation of variable stars, where the brightness of the star was not known in advance, the errors which I attribute to misidentification were so great as to destroy or nearly destroy the value of the observations. It is not necessary for my purpose that I should question this conclusion. If it is admitted as well-founded, it only shows that in the two large catalogues, one supplementary table occupying nine pages should be discarded. When an error of misidentification occurs, in observation with the meridian photometer, it affects only the one star misidentified. The neighboring stars are absolutely unaffected. My own observations with the deflector are almost absolutely free from this danger. Each star is identified by the configuration of the group, every time it is measured. When I observe a series of ten stars, obtaining my standard from meridian photometer stars, if there has been an error of a magnitude from the misidentification of one of those stars used as standards, my resulting standard will be only .1 mag. in error, the misidentified star coming out .9 mag. different from the standard. This is a sufficient notice to reject that star and avoid the whole error, although it is only recently that I have taken the responsibility of doing this.

Since the completion of my Catalogue of Comparison Stars, I have adopted the plan of using only my own results after having obtained the general standard from the average of a sufficient number of meridian photometer stars: so that my new catalogue now forming, is free from the influence of errors of misidentification, excepting so far as they may have affected the average standard in the neighborhood. Comparison of these results in my new catalogue with the corresponding magnitudes in the meridian photometer observations, will identify the stars which have varied In some of these cases I have no doubt that the variation has been in the stars themselves: in other cases there may have been misidentification or other error. In either case the stars are selected which are doubtful, and the rest of the stars

may be accepted as reliable. It may be that some of my o
results are affected with error: I do not claim to be infallible
only claim that my new results are so far independent of Pick
ing's that whenever they agree each confirms the other, a
when they disagree each throws doubt upon the other
upon the constancy of the star. I do not suppose that eit
Cambridge or Brooklyn was selected for photometric obser
tions in consequence of the clearness of its atmosphere. I
fully satisfied that the probable error of my own observatio
consists chiefly of the variations of the sky; and I have no do
this is equally true of the Harvard observations. This opinion
confirmed by learning that Müller and Kempf, at Potsdam, cla
for their photometric observations a probable error much l
than I had ascertained resulted in my own observations fr
variations of the sky alone, after eliminating errors purely of
servation. I have found that on especially clear evenings
probable error does not exceed that claimed by Müller and Kem
while on the average it equals that of the meridian photome
But this is not a systematic error; it only increases the numbe
observations necessary to obtain accuracy, and increases the
certainty of the individual observations of variable stars, wh
there is nothing to counterbalance it. This confirms the conc
sion I arrived at long ago, that under the atmospheric conditio
it is better to multiply observations on different evenings, th
to seek to increase the accuracy of individual observations.

Since the completion of my Catalogue of Comparison St
[*H. C. O. Annals*, vol. XXIX], I have started anew, cutting my
aloof from all errors of individual stars in the Harvard ca
logues, or in my own earlier observations. As yet I have only
a very slight extent eliminated the errors affecting zones of sta
But a comparison of my results with each other, and with
meridian photometer results, and with the results of Müller a
Kempf at Potsdam, will already test the extent of the liability
errors of misidentification or other errors affecting individ
stars. My observations, so far as reduced and transferred,
about 16 months following Jan. 1, 1893, include 936 stars, e
observed from once to more than 30 times, (excluding, of cou
known and suspected variables), contained in 110 groups. Fr
these I have selected such as were suitable for this investigati
from having been observed with the meridian photometer, a
with the deflector often enough for comparison, upon the ba
of two meridian photometer observations or eight extinctions
each summarized observation. This gave me for investigat

191 stars, contained in 63 groups. The mean difference in comparing meridian photometer observations with each other, was .13 mag.; the mean difference in comparing deflector observations with each other, was .14 mag. Although the last decimal may not be precise, from the inclusion among the summarized observations in both cases of stars observed more than the standard number of times, this demonstrates that the errors with either instrument are so exceptional that they do not seriously affect the catalogue as a whole. But when I compared the meridian photometer observations with my own, the mean difference was .22 mag., or at least 60 per cent greater than in comparing either set with each other. The difference, amounting to more than the mean error of either catalogue, I attribute to two causes: First, the change of my values in certain localities from equalizing the obscuration errors before referred to; and second, changes in the stars themselves. The progress of equalization has necessarily been so slow that I am disposed to attribute the principal part of this special discrepancy to changes in the stars. Professor Harkness, in his address last night, spoke of the advantage of employing different methods, from the fact that the constant errors of one become accidental errors in another. As soon as I shall have sufficient material, I intend to investigate whether there may not be systematic errors in one or the other of these two photometric methods, since such errors would tend to produce just such a discrepancy as is here manifested. In 4 per cent of the stars rejected by my criterion as fluctuating, this variation may amount to half a magnitude or more. It will require a more careful examination than I have considered my present materials to warrant, to determine the minor actual changes of the stars generally. It is enough for my present purpose if I have shown from independent observation, that the criticisms of Dr. Chandler are not applicable to the main body of the observations with the meridian photometer, but chiefly relate to the attempt to use the meridian photometer in the observation of variable stars, where there is no guide in the identification of the observed star in the knowledge of its approximate brightness

In the recent volume of photometric observations by Müller and Kempf at Potsdam, there is a repeated expression of the opinion that the Harvard observations were conducted with too great haste, and that this led, not only to general inaccuracy but also to the misidentifications already spoken of. Upon that question I wish to make some suggestions with regard to the advantages for which the Harvard observers hazarded inaccuracy and even occasional errors

An observer with a meridian circle in a first class Observat takes up the Durchmusterung and at once exclaims, "This w has been done with excessive haste; the positions are several onds of arc in error; sometimes neighboring stars are confound two being observed as one; for an astronomer who wants obtain accurate positions of comparison stars for a comet or teroid, your work is valueless." The reply is that it is better have a comparison star close by, whose position is roug known, than one several degrees away which is exact; for comparisons can be made and the position of the compari star subsequently ascertained as accurately as need be. In con quence of the irregularity of the sky, it is necessary that m photometric observations should be differential, and for the sa reason it is important that the standards should be close hand. Under these circumstances it was far more important the time the Zone Catalogue was preparing, that it should c tain many stars than that it should contain a few stars ac rately observed. If three or more consistent zone stars are co bined to form a standard, it is easy to weed out errors, inaccuracies will tend to counterbalance each other. Picker has done for photometry what Argelander, Gould and Schönf have done for the determination of positions, a work to be s plemented only by the new photographic survey; while it may that Müller and Kempf stand more nearly in the relation of meridian circle observers, to determine with accuracy select stars.

THE MAGNESIUM SPECTRUM AS AN INDEX TO THE TEMPERAT OF THE STARS.

JAMES E. KEELER.

In a paper recently published in the *Sitzungsberichte* of Berlin Academy of Sciences*, Professor Scheiner calls attentio the opposite behavior, under varying conditions of temperat of two lines in the spectrum of magnesium, and shows that t lines, taken together, give a means of estimating the appr mate temperature of the absorptive atmospheres of the st The line λ 4482 is not found in the flame or arc spectrum of m nesium, but is very broad and strong in the spectrum of spark with Leyden jar; the line λ 4352, on the other hand either invisible or very faint in the spark spectrum and strong

* Translated in ASTRONOMY AND ASTRO-PHYSICS, August, 1894.

the spectrum of the electric arc. In stellar spectra similar differences in the relative intensity of these lines are found, and these differences are in harmony with the supposition that the temperature of the absorbing layer of stars of class I*a* is approximately that of the spark with Leyden jar, and the temperature of stars of class III*a* is approximately that of the electric arc, the opposed characteristics of the lines enabling us to discriminate between the effects of temperature and pressure. The method proposed by Professor Scheiner is certairly a plausible one, and it agrees well with what is known of the temperature of the stars on other grounds, but as the electric spark marks the limit of temperature which can at present be produced in the laboratory, it gives us no means of recognizing stellar temperatures which exceed that limit.

In this connection the behavior of the characteristic magnesium triplet *b*, which is below the lower limit of Professor Scheiner's photographs, is of very great interest. In a recently published paper on the spectra of the Orion Nebula and the Orion Stars,* I have referred to the circumstance that this group is absent from the spectrum of Rigel, in which the magnesium line λ 4482 is conspicuous, and it becomes of importance to ascertain the conditions on which the disappearance of the *b* group depends. So far as my own experience goes, and so far as I can gather from the published observations of others, the *b* lines are strong in the flame, arc, and spark spectrum of magnesium. The range of temperature which we can command in laboratory experiments is therefore not sufficient to decide the question; but from the appearance of these lines in star spectra, considered in relation to the lines mentioned by Scheiner, their disappearance seems to be the result of a temperature higher than any that can be obtained artificially.

Thus, the *b* lines are strong (apparently somewhat stronger than the solar lines) in Betelgeuse, Antares and other stars of class III*a*; they are of about solar strength in Capella and Arcturus, and weak in Sirius and Vega; they fail altogether† in α Cygni, in which the line λ 4482 is remarkably conspicuous, and in Rigel.

The following considerations seem to apply to these facts: neither of the lines referred to by Scheiner belongs to one of the series of characteristic magnesium triplets; the *b* lines, on the

* ASTRONOMY AND ASTRO-PHYSICS, June, 1894.

† By this I mean that they fail to appear on my photographs, which, nevertheless, show quite faint lines. In α Cygni there are several lines in the vicinity of the *b* group, but the *b* lines themselves are not represented.

other hand, belong to a series which, from analogy with the spectra of the alkalies, is called by Kayser and Runge the second subordinate series of magnesium. For reasons which are set forth in their memoir, it is probable that the molecular structure indicated by lines having the characteristics of a subordinate series cannot exist at a very high temperature, and Kayser and Runge have in this way accounted for the absence from the solar spectrum of all the sodium pairs which do not belong to the principal series containing the *D* lines. The same reasoning applies to the magnesium spectrum, the only difference being that a higher temperature is required to cause the disappearance of the series to which the *b* group belongs.

If this reasoning is correct, the aspect of the *b* lines in stellar spectra gives us an extension of the method proposed by Scheiner, and it shows that the temperature of certain stars exceeds that of the most powerful electric spark. That Rigel should be one of these stars is somewhat surprising, considering the place which it probably occupies in the scale of development, but the reversal of the D_3 line in its spectrum seems to point to the same conclusion, and without further observation it cannot be said that the high temperature assigned to this star by the proposed method is a real difficulty.—*A. N. 3245.*

ALLEGHENY OBSERVATORY, 1894, June 15.

The note printed above requires a slight correction. α Cygni is there mentioned as a star of class I*b* whose spectrum does not contain the *b* lines, although the magnesium line λ 4482 is very conspicuous. Recent photographs taken with high dispersion show that the *b* lines are present, although they are faint, and not easily recognized on account of the presence of much stronger lines in the same region. One of these lines is nearly coincident with b_1, and is probably identical with a strong line which I have described in the spectrum of Rigel.

ON SOME ATTEMPTS TO PHOTOGRAPH THE SOLAR CORONA WITHOUT AN ECLIPSE *

GEORGE E. HALE

PREVIOUS INVESTIGATIONS.

The search for a method to render the corona visible to the eye, or to secure its image upon a photographic plate, without

* Read at the Brooklyn Meeting of the American Association for the Advancement of Science, Aug. 17, 1894. Communicated by the author.

aid of a total eclipse, has been prosecuted for many years. Professor Langley has observed the Sun from the slopes of Mount Etna, from Pike's Peak* and from Mount Whitney, but even when the atmospheric conditions were at their best he could see nothing of the true corona. At Mount Whitney the sky was of a deep violet blue, and absolutely cloudless, with only a slight orange tint about the horizon at sunset. "Carrying a screen in the hand between the eye and the Sun, till the eye is shaded from the direct rays, it can follow this blue up to the edge of the solar disk without finding in it any loss of this deep violet or any milkiness as it approaches the limb. It is an incomparably beautiful sky for the observer's purpose, such as I have not seen equalled in the Rocky Mountains, in Egypt, or on Mount Etna. . . . I found that I could choose a position on the north of the cliff, along whose edge the Sun was moving nearly horizontally; so that the shadow was fixed as regards the observer, and so sharp that, though I must have been over one-quarter of a mile from the portion of the cliff casting it, I could, without moving from my place, and by only a slight motion of the head, put the eye in or out of view of the Sun's north limb. The rocks were, in these circumstances, lined with a brilliant silver edge, due to diffraction. This I had anticipated, but now I saw what could not be seen by screening the Sun with a near object, that the sky really did not maintain the same violet blue up to the Sun, but that a fine coma was seen about it of about 4° diameter, nearly uniform, though it was sensibly brighter through the diameter of 1½°. Upon bringing to bear upon it an excellent portable telescope, magnifying about thirty times, I found it was composed of motes in the sunbeam, between the diffracting edge and the observer's eye. This result, if disappointing, is also interesting in another point of view, as showing that the dust-shell, which, as I have elsewhere stated, encircles our planet, exists at an altitude of at least 13,000 feet, and under favorable conditions for the purity of the atmosphere."† Other unsuccessful attempts to observe the corona without an eclipse have been made by Professor Bond in the Alps, Dr. Copeland‡ at Puno at a height of 12,940 feet, Professor Tacchini on Mount Etna, and Professor Todd on Fuji-san. Professor Wright has tried various colored media in the hope of rendering the corona visible, and Professor Harkness§ and Dr. Pupin have

* *Reports on the Total Solar Eclipses of July 29, 1878 and January 11, 1880*, p. 207.

† S. P. Langley. *Report of the Mount Whitney Expedition*, p. 11.

‡ *Copernicus*, vol III p. 212

§ *Bul. Phil. Soc. of Washington*, vol. X, p. 13

Astronomy and Astro-Physics, vol. XII, 1893, p. 362.

devised special apparatus for the same purpose. But the most serious attempt to solve the problem is that of Dr. Huggins, and this calls for more detailed consideration.

After trying in vain a variety of optical devices, including the crossed prisms afterward employed by Harkness and Deslandres, Dr. Huggins decided to avail himself of the power of the photographic plate to render visible smaller differences in brightness than are directly perceptible to the eye. By experiments in the laboratory he satisfied himself "that under suitable conditions of exposure and development a photographic plate can be made to record minute differences of illumination existing in different parts of a bright object, such as a sheet of drawing paper, which are so subtle as to be at the very limit of the power of recognition of a trained eye, and even, as it appeared to me, those which surpass that limit."* Professor Schuster's photographs of the coronal spectrum obtained at the eclipse of May 17, 1882, showed a maximum brightness between G and H, and it seemed not improbable that an exclusive use of this portion of the spectrum might make it possible to photograph the corona without an eclipse. Later a further argument in favor of the employment of this part of the spectrum was found in the results of Professor Vogel's measures of the absorption of the photospheric light at the center and limb of the Sun. It was discovered that the violet suffered much more absorption than the red, the percentage brightness at the limb compared with the center of the disk being 13 and 30 respectively. As most authorities agree that the rapid increase in absorption toward the limb would indicate that the absorbing gas rises to no great height above the photosphere, the light of the corona, which lies outside the low region of absorption, would reach the Earth without undergoing this absorption. Thus it should be relatively rich in the violet, when compared with the photospheric light which is scattered in our atmosphere.† The advantages offered by photography, especially for work in the more refrangible region of the spectrum, were thus sufficient to lead to its substitution for the visual methods previously employed.

The first experiments were made with photographic lenses, but these were soon abandoned for a Newtonian reflector of 6 inches aperture and about 3½ feet focus. The solar image was received upon a photographic plate, in front of which were placed the absorptive media. The exposing shutter, supported before the end

* *Proc. Roy. Soc.*, vol. XXXIV, 1882, p. 411.
† *Proc. Roy. Soc.*, vol. XXXIX, 1885, p. 111.

of the telescope, was of adjustable rapidity, and reduced the aperture to three inches. The telescope was not provided with a driving clock, on account of the shortness of the exposures. For absorbing media a special violet glass in the form of flat polished plates, and later a glass cell with polished sides containing a strong fresh solution of potassic permanganate, were employed. The gelatine dry plates were backed with a solution of asphaltum in benzole. The exposures ranged from that just sufficient for the Sun itself, to one so long as to reverse the photographic image out to some distance from the Sun's limb.

Between June and October, 1882, twenty plates were secured which showed forms closely resembling the corona, not merely in a general brightening near the Sun, but in the presence of distinct coronal forms and rays. The plates taken on different days with different absorptive media, and with the Sun in different parts of the field, agreed so well among themselves that they could not be attributed to any instrumental effect. The very short exposures showed only the inner corona, but with increased exposure the inner corona was lost in the outer corona, which exhibited the curved rays and rifts observed at eclipses. The corona was most easily seen, however, in those plates which were exposed long enough to photographically reverse both the photospheric and coronal images. A careful comparison of the photographs with those secured at the Egyptian eclipse of the same year showed so perfect an agreement of general features and even details as to convince both Dr. Huggins and Captain Abney that the problem had been solved.*

In his later work† Dr. Huggins did away with the second reflection of the Newtonian telescope, and by slightly inclining the speculum metal mirror caused the solar image to fall directly upon a silver chloride plate, which Captain Abney had shown to be most sensitive to light from h to a little beyond H. The absorbing media were thus considered unnecessary. A long tube with numerous diaphragms was placed in front of the telescope, and every care was taken to avoid difficulties arising from diffuse or reflected light. Photographs taken with this apparatus immediately before and after the eclipse of 1883 showed coronal forms very similar to those obtained at Caroline Island during totality, a dark rift near the north pole of the Sun being particularly noticeable in both cases.

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* *Proc. Roy. Soc.*, vol. XXXIV, 1882, pp. 411-414.

† *Report B. A. A. Science*, 1883, p. 346, also *Proc. Roy. Soc.*, vol. XXXIX, 1885, p. 113.

In 1884 a committee appointed by the Royal Society sent Mr Ray Woods to the Riffleberg, near Zermatt, for the purpose of continuing Dr. Huggins' experiments at an elevation of 8,500 feet. In spite of the continued presence of a great aureole around the Sun, due to the presence of minute particles of matter in the higher regions of the atmosphere, Mr. Woods obtained coronal images which were very much alike on the same day, but showed variations when separated by longer periods. The use of a disk to cover the Sun's image seemed to be of no advantage. The best results were obtained on the clearest days.*

During the next two years no experiments in this direction were made in England, owing to the prevalence of whitish skies In 1886, however, the method was put to the test at the time of the total eclipse of that year. Captain Darwin photographed the Sun on the day before the eclipse, but the coronal images obtained showed no similarity with the true coronal images photographed during totality. On the day of the eclipse exposures made during the partial phases showed a false corona, part of which was in front of the Moon. In no case did the images show the Moon eclipsing the corona. Instantaneous exposures made during totality gave no trace of the corona. Although the conditions were most unfavorable during this eclipse Captain Darwin concludes "that the result tends to show that a *practical* method of obtaining photographic records of the corona during sunlight is not likely to be obtained," at the same time remarking that he does not consider it proved that the method is impossible.†

At Cape Town Dr. Gill photographed the partial phases of the same eclipse with a Huggins coronagraph, but the results were equally disappointing. A long series of experiments made at the Cape Observatory with the coronagraph has not led to results of a definitive character.‡ Nor have the attempts in this direction of Dr. Lohse,§ Herr von Gothard‖ and M. Deslandres¶ resulted more satisfactorily.

Professor Tacchini's observations on Mount Etna in 1876** led to the establishment of the Bellini Observatory at the base of the great crater a few years later. With a Huggins coronagraph

* *Observatory*, vol. VII, 1884, p. 378.

† *Proc. Roy. Soc*, vol XLI, 1886, p 470.

‡ *Report of H. M. Astronomer at the Cape of Good Hope for the period 1879, May 26, to 1889, July 21*, p. 9.

§ *Astronomische Nachrichten*, vol CIV, 1883, p. 209.

‖ Von Konkoly's *Himmelsphotographie*, p. 229.

¶ *Comptes rendus*, Jan. 23, 1893

** *Memorie della Societa degli Spettroscopisti Italiani*, vol V, 1876, p. 151.

constructed by Grubb, Professor Riccò has made a large number of photographs of the Sun's surroundings. Many of these show distinct coronal forms, extending to great distances from the solar limb. During the partial eclipse of April, 1893, Professor Riccò made several exposures, but the coronal forms shown on the negatives cross the dark body of the Moon without interruption, thus demonstrating that they do not represent the true corona.* Moreover, the exposures with which these and other coronal images have been secured are much shorter than those given with similar instruments during total eclipses. It is universally admitted that the corona at a distance of a solar radius from the limb is much fainter than the Moon. In Professor Riccò's photographs, which were made with an exposure of less than half a second, the coronal forms extend to a distance of nearly a solar diameter. Using the same apparatus and equally sensitive plates we obtained no traces of the Moon (which was well past its first quarter) with an exposure of four seconds, during our recent expedition to Mount Etna. It is, therefore, highly probable that the coronal images obtained with the Huggins apparatus on Mount Etna were of terrestrial origin.

Although I have not succeeded in photographing the corona in full sunlight, I present herewith an account of my efforts in this direction, in the hope that some of the details may prove of service to others at work in the same field.

A New Method of Coronal Photography.

At the Rochester Meeting of the American Association I described my investigations in solar photography with monochromatic light. By the aid of the spectroheliograph I had succeeded in photographing the chromosphere and prominences as well as faculæ and other phenomena on the solar disk. On June 10, 1892, I made two attempts to photograph the corona with the spectroheliograph, the exposure being much longer than that required for prominences. The sky showed a considerable brightening near the Sun on the plates, but no certain indication of the corona was obtained. The possibilities of the spectroheliograph in its application to the photography of the corona were carefully studied, and in August of the same year the construction of a special instrument for this purpose was undertaken by the mechanician of the Kenwood Observatory. It was at first proposed to isolate light of any desired wave-length by means of a spectroheliograph, and thus to photograph the sky surrounding the Sun,

* *Memorie della Societa degli Spettroscopisti Italiani*, July, 1893

employing such a region of the spectrum as experiment proved to be best adapted to show the corona on the background of the sky.* Reasoning from Professor Vogel's measures of the solar absorption, as Dr. Huggins had previously done, and also from the comparative brightness of the coronal spectrum in the violet region, it would appear that the upper part of the spectrum might be advantageously employed, except when the blueness of the sky extends to the very limb of the Sun. Professor Langley's observation at Mount Whitney, which has been quoted at length above, would make it appear probable that this latter condition is never fully realized. M. Deslandres† has recently called attention to Lord Rayleigh's comparison of the sky and solor spectrum, from which was deduced the law that the brightness of the sky spectrum (near the zenith) is inversely proportional to the fourth power of the wave-length of the region observed. With an atmosphere free from large particles of dust and vapor the law would probably hold for the light of the sky very near the Sun's limb, and in such a case the less refrangible region could be advantageously employed with the spectroheliograph. Under ordinary conditions, however, the whiteness of the sky near the Sun is so intense as to render advisable the use of the blue or violet rays, partly on account of the lack of really satisfactory photographic plates sensitive to the red or ultra-red rays.

But further reflection convinced me that the mere isolation of a particular region of the spectrum by means of the spectroheliograph would not suffice for the difficult task of photographing the corona in full sunlight, and it was soon concluded that the most promising means of attaining this end was offered by the dark lines of the solar spectrum. As the light of the sky is merely reflected sunlight, the dark lines of its spectrum indicate that light of the corresponding wave-lengths is but feebly represented in our atmosphere. If, then, a dark line (other than telluric) in the sky spectrum is set on the second slit of a spectroheliograph, and the slit narrowed sufficiently to exclude all light except that of the line, it is evident that the protection to the photographic plate will be the same‡ as that which would result from a reduction of the brightness of the whole sky to that of the line. Whether or not the coronal light will be reduced in anything like the same proportion is the next question to be considered.

The presence of reflected sunlight in the spectrum of the cor-

* *Astronomy and Astro-Physics*, 1893, p. 260.
† *Bulletin astronomique* February, 1894.
‡ Neglecting the effect of diffuse light in the apparatus.

ona has long been recognized, but opinions as to its amount vary widely. The principal Fraunhofer lines have been seen and photographed at many eclipses, but frequently they are so faint as to be barely perceptible. After an exhaustive examination of all observations made up to 1883, Professor Hastings remarks that "the conclusion is inevitable that the proportion of true solar light in that of the corona within 5′ or 6′ of the Moon's limb is so small that all but the strongest of the Fraunhofer lines are invisible in any spectroscope which has hitherto been employed." By mixing light giving a continuous spectrum with sunlight Professor Hastings succeeded in producing a spectrum in which the principal Fraunhofer lines were of about the same intensity as in the corona when the proportion of light giving a continuous spectrum to solar light was about two or three to one.* This is direct proof that the percentage of reflected sunlight in the coronal spectrum is small. Recent eclipses furnish evidence leading to the same conclusion, for in some photographs made with a slit spectrograph the Fraunhofer lines are nearly as well seen in the sky outside the corona as in the corona itself. Thus it appears that by setting a dark line on the second slit of a spectroheliograph the brightness of the sky spectrum may be considerably reduced, while the brightness of the coronal spectrum at the same point is not seriously diminished.

In the choice of a line for this purpose it is evident that one should be selected which is (1) of solar origin, (2) of sufficient width, (3) very dark compared with the continuous spectrum in which it lies, and (4) situated in that part of the spectrum in which the contrast between the continuous spectrum of the corona and the spectrum of the sky near the Sun is the greatest possible. Of these conditions the first can of course be fulfilled. The second is of practical importance, for on account of the feebleness of the coronal light the dispersion must be low, and thus the greater part of the Fraunhofer lines will be too narrow to afford complete protection to the plate, as the second slit cannot be closed beyond a certain point for mechanical and optical reasons, and also because with smaller slit-width the exposure would be too much prolonged. The third condition is also of great importance, but as many of the darkest lines are too narrow to be successfully employed it simply remains to choose the darkest of the sufficiently wide lines. A further complication may result from the position of the line in the spectrum. It has

* *Report of the Eclipse Expedition to Caroline Island, May, 1883*, p. 116.

been pointed out that the less refrangible rays may offer peculiar advantages when the atmospheric conditions are such that the sky is blue close to the Sun's limb. But as this condition is rarely, if ever, realized the special advantages of the lower part of the spectrum may be offset by the practical difficulties peculiar to this very region. Hitherto, in spite of the successful use of such dyes as cyanine and alizarine blue for increasing red sensitiveness no plates have been obtained which, in point of sensitiveness to red light and freedom from fogging, can compare with the action of ordinary commercial plates in the more refrangible part of the spectrum. When truly isochromatic plates have been obtained the red or ultra-red may perhaps be advantageously employed for photographic work on the corona. But on account of our incomplete knowledge of coronal radiation and atmospheric diffusion experiment may be regarded as the best guide in the choice of the most suitable region of the spectrum.

In all of my experiments at Chicago, Pike's Peak and Mount Etna I have employed the broad dark K band of the solar spectrum, for practical rather than for theoretical reasons. With the spectroheliograph used on Mount Etna this band was but [illegible] of an inch wide, and in work on the corona it is not advisable to use a second slit much narrower than this. In fact, even this slit-width requires that the exposure be greatly prolonged. It will be seen, however, that this difficulty may be diminished by employing a spectroheliograph of large effective aperture. K is preferred to H because it is somewhat broader and darker, but neither of these bands is nearly as dark as many of the lines in the solar spectrum. K was chosen simply because the feeble light of the corona does not permit the employment of a dispersion sufficiently great to make the narrower dark lines wide enough to protect the plate. If the dispersion were increased enough to allow the narrow lines to be used, or, what amounts to nearly the same thing, if the second slit were made equal in width to these narrow lines, the exposure would have to be so much increased that the advantage gained by the greater blackness of the line would probably be more than offset.

It will be noticed that the procedure here recommended for photographing the corona without an eclipse is identical with that employed daily at the Kenwood Observatory in photographing the chromosphere and the solar disk. The principle of the method is, however, very different. In the case of the chromosphere photography is rendered possible by the presence in the chromospheric spectrum of the bright K line of calcium, but while this

line is frequently spoken of as belonging also to the corona, most of the evidence is opposed to this view. It is true that eclipse photographs made with a slit spectrograph show the bright H and K lines extending far out into the corona, but these same bright lines also frequently appear across the dark body of the Moon: a result of diffusion in the Earth's atmosphere of the brilliant calcium radiation of the chromosphere and prominences. Professor Hastings has shown that diffraction at the Moon's limb may also have a part in producing the extension of the reversals into the corona,* and as the result of his observations at the eclipse of January 1, 1889, Professor Keeler concludes that superposed upon the corona, "and blended in with it, is a more or less uniform ring of light caused by diffraction. This diffracted ring is necessarily rich in edge light of the Sun, particularly light derived from the chromosphere, and to it is due the appearance of bright lines in the corona at a considerable height above the Sun."† It is needless to multiply references to the various eclipse reports in which similar conclusions are reached, and we may merely add that Captain Abney and Professor Schuster believe that if the presence of the H and K lines in the spectrum of the corona cannot always be accounted for by atmospheric diffusion or diffraction, the calcium vapor which must then give them rise is carried up into the corona by eruptive prominences, and does not exist there in the normal condition of things.‡

But even if it could be shown in the face of all this evidence that the H and K reversals properly belong to the coronal spectrum, it would still be impossible to photograph the corona without an eclipse by their means. For if we may trust the observations of Tennant and others, the lines seen in the coronal spectrum are as bright in the dark rifts as in the brilliant streamers. Thus—supposing these lines to indicate the existence of glowing vapors at a distance from the Sun's limb—a photograph taken with a bright line would show a uniform halo about the Sun, with no traces of true coronal structure. It is evident, therefore, that we must depend upon the continuous spectrum of the corona to furnish the needed light, and the advantages offered by the spectroheliograph are the choice of light of any desired wave-length, and particularly the protection afforded to the photographic plate by shielding it from all light except that coming

* *Report of the Eclipse Expedition to Caroline Island, May, 1883*, p. 121.
† *Lick Observatory Report on the Total Eclipse of January 1889*, p. 54.
‡ See *Phil. Trans.*, vol. 180 (1889), (A) p. 328, and other eclipse reports.

through a relatively dark line in the superposed spectra of the corona and sky.*

I have already pointed out† that the same method may perhaps be employed in photographing the "white prominences," discovered by Professor Tacchini at the Egyptian eclipse in 1882, and observed subsequently at Caroline Island and Grenada in 1883 and 1886.‡ These remarkable objects give a continuous spectrum, and as the hydrogen lines are absent they cannot be seen in full sunlight observations with the spectroscope. Photographs of the spectrum of the great white prominence, which was so conspicuous an object at the 1886 eclipse, seem to show the presence of the bright H and K lines§ and this gave reason to hope that they might be successfully photographed without an eclipse with the ordinary spectroheliograph employed for prominences. Not all of the Kenwood Observatory photographs have been reduced, but on those hitherto examined, no such objects have been found. But even if the H and K reversals do not belong to the white prominences, the method recommended above for the corona may prove to be successful for them as well, on account of the comparative brightness of their continuous spectrum.

Of first importance in the design of any apparatus to be used in photographing the corona without an eclipse is the telescope with which the solar image is formed on the first slit of the spectroheliograph. M. Deslandres has suggested the use of a single lens of crown glass or quartz for this purpose, and remarks that the doubly reflected light could be stopped by a diaphragm placed near the lens. If a lens were to be selected in preference to a mirror it would seem to me advisable to give to the surfaces such a curvature and such an inclination to the optical axis of the telescope that all of the doubly reflected light would be concentrated in an image some distance to one side of the direct image of

* M. Deslandres' claims to priority on the proposed use of the spectroheliograph for photographing the corona without an eclipse (*Comptes rendus*, v. CXVI, p. 1184) are admittedly based on a somewhat obscure reference in a foot-note to the employment of the *bright* H and K lines to this purpose (*Comptes rendus*, v. CXIII, p. 307). My own use of the method followed naturally from my work on the prominences, and had nothing to do with the foot-note in question. Moreover, I have never advocated the use of the *bright* H and K lines, as there is no reason to suppose that they could be successfully employed in this work.

† *Sidereal Messenger*, June, 1891, and elsewhere.

‡ For a full account of Professor Tacchini's observations of the white prominences see his interesting work, *Eclissi totali di sole del 1870, 1882, 1883, 1886 e 1887* (Roma, Tipografia Eredi Botta, 1888).

§ W. H. Pickering, *Annals of Harvard College Observatory*, vol. XVIII, No. V, p. 99.

‖ *Comptes rendus*, vol. CXVI, p. 1180.

the Sun. The chromatic aberration of the single lens would be an advantage rather than a disadvantage, and the difficulties arising from the spherical aberration and the inclination of the lens to the normal plane would hardly be sufficient to so seriously injure the coronal image as to make it unfit for our present purpose. But on account of the difficulty of completely doing away with the doubly reflected sunlight from a lens, I have preferred to follow the example of Dr. Huggins, and employ mirrors of speculum metal or silver-on-glass in all of my experiments. I wish to express my admiration for Mr. Brashear's inimitable skill in making and polishing such specula, as well as my indebtedness to him for the pains he has taken to furnish me with surfaces of the highest excellence.

In each of the three instruments successively employed at Chicago, Pike's Peak and Mount Etna the mirror has been mounted in the Herschelian form, previously adopted in the Huggins coronagraph. I have not been unmindful of the dangers of diffuse light, but from the very outset every possible precaution has been taken in the way of numerous diaphragms, dead black surfaces, perfectly polished mirrors, prisms and objectives, etc. It has been found in practice that dust is one of the principal obstacles to success, and great pains have been taken to make the apparatus as nearly as possible dust-proof, and to protect as well as might be such surfaces as were necessarily exposed to the air.

Experiments at the Kenwood Observatory.

Fig. 1, Plate XXVII, shows in outline the apparatus used in the series of experiments made at the Kenwood Observatory in the spring of 1893. A is a silver-on-glass mirror of $5\frac{1}{2}$ inches aperture and 48 inches focal length. At B the solar image is formed on a small metallic disk, slightly exceeding in diameter the image of the photosphere. The mixed light of the corona and sky near the Sun then passes into the spectroheliograph through a slit at B, while the direct sunlight is excluded. If this light were allowed to enter the spectroheliograph it would greatly reduce the chances of success. The diverging pencil of sky and coronal light falls at C upon a silvered glass mirror of 4 inches aperture and 24 inches focal length. As the mirror is separated from the slit by a distance equal to its own focal length, and mounted with the normal to its surface making a small angle with the axis of collimation, the parallel pencil is reflected to D, where it falls upon a crown glass prism of 30° angle, the second surface of

which is silvered. The rays are thence returned to the collimating mirror, and the prism and mirror are so adjusted that the image of the spectrum is formed on the second slit, immediately below the first slit. The K line is made to pass through the second slit, and falls upon a photographic plate, the surface of which is in the focal plane. As the first slit is moved across the coronal image by means of water pressure in a small clepsydra, the second slit is adjusted to move in the opposite direction at such a speed as to keep the K line constantly between its jaws. Fig. 1 Plate XXX illustrates the arrangement of the slits. The frames on which the slits are carried move on steel balls, and the lever connecting the slit carriages is adjustable in length. The second slit is, of course, curved to correspond with the curvature of the K line The mirrors A and C are adjustable for inclination and focus, and the prism can be rotated by means of a tangent screw outside the box. The framework of the apparatus is made of gas pipe, and the covering of wood, painted dead black, and provided with numerous diaphragms and screens to diminish diffuse light. With the exception of the mirrors and prism, which were made by Mr. Brashear, the apparatus was constructed in the workshop of the Kenwood Observatory. When in use it was attached to the tube of the 12-inch telescope, and the excellent driving clock would keep it directed at the Sun during any desired exposure.

The use of a single mirror in place of the ordinary objectives of the collimator and observing telescope of a spectroscope was first suggested by Lippich,* and has since been reinvented by Ebert† and Wadsworth.‡ It offers important advantages in many classes of spectroscopic work, and will no doubt share in the future some of the popularity which spectroscopes with two mirrors in place of objectives in the collimator and telescope are now beginning to enjoy.

On April 8 the apparatus was attached to the telescope, and on April 13 and 14 the various adjustments were made. These included focussing the solar image on the first slit, focussing the collimating mirror and adjusting it and the prism so that the K line fell on the second slit, setting the jaws of the second slit parallel to the K line, adjusting the length of the lever so that the K line remained on the second slit during its movement across the Sun, etc. The performance of the two mirrors was satisfactory,

* *Zeitschrift für Instrumentenkunde*, 1884, p. 1

† *Sitzber. physikal. med. Soc. Erlangen*, 8 July, 1889.

‡ *Phil. Mag.*, 1894

in spite of their necessary inclination, but I was not satisfied with the arrangement and motion of the two slits, and the clepsydra was so small that on account of the large and variable element of friction the motion was not sufficiently uniform. The K line stayed on the slit fairly well, but there was a certain amount of lost motion which made it uncertain whether the slit always followed the line accurately throughout the exposure.

On April 16, the day of the total eclipse in Africa and South America, some attempts were made to photograph the corona, but the sky was far too bright to permit a sufficiently long exposure to be given. With a motion of the slits such that the half-inch solar image was crossed in eight minutes, the sky was greatly overexposed, and another photograph showed that one minute was still too long. As my calculations had indicated that we could not hope to obtain the corona in much less than 25 minutes, the results were far from encouraging. Further experiments only served to demonstrate more clearly the hopelessness of coronal photography beneath a Chicago sky, and the shortcomings of the apparatus also became more apparent with continued use. It was therefore decided to repeat the experiments with better apparatus and the more favorable atmospheric conditions to be expected on a lofty mountain peak.

Through the kindness of Professor Harrington, Chief of the Weather Bureau, I was enabled to examine the meteorological records of many elevated stations in the West, and after careful consideration of the relative advantages of these points, I decided to make an expedition to the summit of Pike's Peak at the time of the summer solstice of 1893.

THE EXPEDITION TO PIKE'S PEAK.

The apparatus constructed in our workshop for this expedition is outlined in Fig. 2, Plate XXVII, and Plate XXIX is from a photograph taken by Professor Keeler on Pike's Peak. The same mirror used in the previous experiments forms an image of the Sun on a metallic disk at B. The mixed light of the corona and sky enters the collimator by the first slit, and is rendered parallel by an achromatic objective (C), of 2 inches aperture and 20 inches focus. The dispersion piece is a Brashear 60° prism (D) of light crown glass, perfect in purity and freedom from color. The objective (E) of the telescope is exactly similar to that of the collimator. Both were made of carefully selected glass by Mr. Brashear, and castor oil is used in each objective between the lenses. The first and second slit carriages run on steel balls, and are connected with each

other and with a clepsydra by a lever system similar to that in constant use on the large spectroheliograph of the Kenwood Observatory. The second slit carriage is provided with a totally reflecting prism attached to a microscope, which can be racked into or out of the field of view for the purpose of setting the K line on the slit, and also to enable the observer to maintain it there, if necessary, during the exposure, by means of a screw arranged to move the second slit relatively to its carriage. The spectroheliograph and sheet-iron mirror-tube are supported by a skeleton iron frame, which was bolted to the declination axis of a 6-inch equatorial mounting by Grubb, for the use of which I am indebted to the kindness of Professor H. A. Howe, Director of the Chamberlin Observatory.

Professor James E. Keeler, whose experience gained at Mount Whitney and on other important expeditions rendered his advice and assistance of incalculable value, was good enough to volunteer his services, and the many difficulties encountered on the Peak were lessened in no small degree by his presence. I desire here to express to him my grateful acknowledgments.

On June 16, 1893, our party, consisting of Professor Keeler, Mrs. Hale and the writer, left Chicago on the Chicago, Rock Island and Pacific R. R., and arrived at Manitou, Colorado, on June 18. The following extracts from our note-book will sufficiently outline the events of our stay in Colorado

JUNE 19 Had a wooden stand made to carry the Grubb equatorial head, and arranged to leave for the Peak next morning

JUNE 20 Went up the Peak on the morning train, taking a trunk filled with apparatus, and the wooden stand for the telescope. L and I returned to Manitou; K remained at the summit, and set up the stand in front of the Weather Bureau Station, levelled it and anchored it with rocks

JUNE 21 Went up on the morning train, taking the Grubb mounting and a box of apparatus As soon as we reached the summit, K and I placed the mounting on the wooden stand, and roughly adjusted the polar axis In the evening we put the spectroheliograph together

Suffered considerably from headache due to the altitude

JUNE 22 Continued work on the adjustments of the mounting and spectroheliograph The first time the R A clamp was used the steel pin broke short off, but it was found possible to use it by employing a screw driver for clamping.

L's severe headache continued to grow worse, and it became impossible for her to stay on the Peak (We afterwards found

that about two-thirds of the tourists who came up the mountain on the train each morning were affected by the altitude, and during our stay we saw one or two very serious cases of mountain sickness. While not much troubled, K. and I found prolonged hard work very fatiguing, and any slight extra exertion at once increased the action of the heart). I went down with L., and in the afternoon had a rough R. A. clamp made in Colorado Springs.

JUNE 23. Left L. at Manitou, and went up on the morning train, taking with me an express box containing the telescope tube to be used with the spectroheliograph. In the afternoon fitted spectroheliograph to mounting, and prepared it for use. In the evening found that the silvered glass mirror, which was in perfect condition a few days before, was badly tarnished. Telegraphed Brashear for silvering materials.

JUNE 24. Arranged a canvas cover to protect apparatus when not in use. Commenced adjusting mirror. Snow storm came up, so went down to Manitou with K. on afternoon train.

During our stay on the Peak we found that the sky was usually cloudless in the early morning, but every day with great regularity cumulus clouds commenced to form about the Peak between nine and ten o'clock, and by eleven o'clock the sky was generally covered with them. In going up from Manitou I observed the sky near the Sun at altitudes ranging from 8,000 to 14,000 feet, and found a steady decrease in the brightness as the summit was approached. At times during the first few days the sky appeared blue nearly up to the Sun's limb, but later extensive fires in the forests to the south sent up great volumes of smoke, which spread over the sky, and increased its whiteness. Fires also appeared in other directions, and when we left for home on July 4 the Peak was encircled with fires, and the sky was very white.

JUNE 25. At Manitou House. Sky cloudy in the morning, storm in the afternoon.

JUNE 26. Went up on monrning train. When we reached the summit the sky was clouded over, and soon after hail began to fall. Storm continued with hail and snow all the afternoon, and at times wind velocity was over 70 miles per hour. Sky cleared at sunset. Improvised a dark room in the kitchen of the Weather Bureau Staton.

JUNE 27. Worked on adjustments of apparatus. Cloudy most of the afternoon.

JUNE 28. Continued work on spectroheliograph adjustments. Optically the instrument leaves nothing to be desired, but there

is some spring in the levers, and as our only source of pressure was a pail of water a few feet above the instrument on the roof of the Station, the clepsydra did not work well. As the prism is no longer at position of minimum deviation for K when the slits are out of the center of the field it was difficult to adjust levers so that this line would stay on the second slit, but at last this was accomplished. Made first exposure at 12 M. Received silvering materials from Brashear, and re-silvered mirror in the evening.

JUNE 29. Cloudy all day. K. and I walked down to Manitou in the afternoon through a heavy snow storm, with much thunder and lightning. We could hear the continual hissing of the brush discharge from the corners of the metallic roof of the Station, and from the pointed wires set in the top of every telegraph pole. The storm was local, as we emerged into bright sunshine about two miles below the summit. As seen from Manitou the Peak was covered with clouds the rest of the day.

JUNE 30. Went up with K. on morning train. Clouds came up so rapidly that we had time for but one exposure. Sky rather white. In the evening the telescope was left uncovered for photographing the Moon, but a cloud suddenly came over the mountain and every part of the instrument was drenched before it could be covered.

JULY 1. Made third exposure at 9h 15m, sky hazy, mirror in bad condition. Plate 4, 9h 40m; sky milky near Sun, tried several speeds of clepsydra. Plate 5, 10h 20m, sky fair; passing clouds. Plate 6, 10h 45m, sky good, clouds passing over Sun. Plate 7, 11h 15m, sky good at times, but clouds passing over Sun during exposure, as in case of Plates 5 and 6. Further work prevented by clouds.

An attempt was made to photograph the spectrum of the Moon in the evening, but a cloud covered the mountain just after the exposure commenced.

JULY 2. It was found that the heavy canvas used to cover the telescope so disturbed the adjustments by pressure on the levers that much time was required each morning to re-adjust the spectroheliograph. Made several exposures; sky very white. During last exposures swarms of insects (also seen on previous day) were seen above the mountain in the direction of the Sun. These added considerably to brightness of sky. At 11h 15m, when work was stopped by clouds, the sky was very carefully examined. The Sun was seen to be surrounded by a great white haze, which grew rapidly brighter near the limb.

JULY 3. Smoke from forest fires increased so rapidly that nothing more could be done. Packed up apparatus.

JULY 4. Went down to Manitou, passing very near a forest fire on the way. Left the same afternoon for Chicago.

Whatever one's confidence in the method employed in these experiments, it is not very surprising that the negatives obtained under such conditions failed to show any trace of the corona. Not only was the purity of the sky destroyed by the smoke from the forest fires, but the lack of sufficient water pressure for the clepsydra made the motion of the slits very irregular, and the variations in friction and speed caused such springing in the levers that the K line must have been frequently off the second slit. The clepsydra had been designed for use with a much higher pressure, and when this could not be obtained it failed to communicate to the slits the necessary smooth and uniform motion. There can also be no doubt that it is undesirable, with a spectroheliograph of these dimensions, to move the slits far out of the position of minimum deviation. As I have already mentioned, the spectroheliograph was extremely satisfactory from an optical point of view, and the amount of diffuse light was very small. The dust constantly blowing into the telescope tube and on to the mirror, was very troublesome, and the frequent use of a soft camel's hair brush did not suffice to keep the mirror free, as much dust accumulated during the exposure. Silver is so soft that microscopic scratches are always cut in it when polishing. On account of its comparative hardness and freedom from liability to tarnish speculum metal is to be preferred to chemically deposited silver for work on the corona. A still better form of mirror will be mentioned below.

I desire to express the thanks of our party to R. R. Cable, Esq., President of the Chicago, Rock Island and Pacific R. R., through whose courtesy we were supplied with round-trip tickets from Chicago to Manitou, and to Messrs. McGuiness and Myers, the representatives of the Weather Bureau at the Pike's Peak Station, for their constant kindness and frequent assistance during our stay on the Peak. To Mr. Cable, Manager of the Pike's Peak R. R., we are also indebted for many favors.

A word as to the suitability of Pike's Peak as a site for astronomical observation. When free from the disturbing effect of forest fires the sky is of a very deep blue at the zenith, and when the conditions are very favorable the blueness persists up to within a short distance of the Sun, losing, however, much of its depth of color. During the entire time of our stay the stars appeared to be little or no brighter when seen from the Peak than when seen from Manitou, 8,000 feet below. The scintillation,

even near the zenith, was always very marked, and at no time during our stay would the seeing have been even fair. In this regard our experience agrees closely with that of the Harvard Observatory party, which visited the Peak some years ago. The altitude of the summit (14,147 feet) is not greatly inferior to that of Mont Blanc (15,780 feet), and the railroad which ascends from Manitou is a great convenience. For such observations as require a blue sky rather than good seeing, Pike's Peak (when not surrounded by forest fires) would seem to offer some important practical advantages over other mountains of equal altitude. But if good seeing is essential the Peak is not to be recommended.

THE EXPEDITION TO MOUNT ETNA.

During Professor Tacchini's visit to Chicago in August, 1893, I discussed with him the problem of coronal photography, and described our unsuccessful expedition to Pike's Peak. His observations made on Mount Etna had convinced him that the Bellini Observatory (altitude 2942^m) would be a suitable place for the continuation of my experiments, especially as the 12-inch equatorial, mounted under an excellent dome, would serve admirably to carry the apparatus. The cordial invitation which he extended on the part of Professor Riccò and himself led to our decision to make an expedition to Mount Etna in the spring or summer of 1894.

It had been my intention to employ on Mount Etna the apparatus used at Pike's Peak, and some changes were made in it for the purpose of correcting the defects discovered in the course of our previous experiments. But during the winter Mr. Ranyard visited us in Berlin, and was kind enough to propose that I make use of a spectroheliograph to be built by Otto Toepfer, of Potsdam, for the 18-inch reflector of his London Observatory. I take this opportunity to express to Mr. Ranyard my warmest thanks for his kindness in allowing me to design the spectroheliograph as well as to employ it in the work at Mount Etna.

Experience with many forms of spectroheliograph has clearly demonstrated that the instrument should be so constructed that the line of the spectrum can be most easily kept on the second slit during the exposure. I have already pointed out* the best means of accomplishing this, and in designing the instruments used in the first experiments in coronal photography the mechanical advantages of this type were fully recognized. It was feared, how-

* *Astronomy and Astro-Physics*, March, 1893, p. 250.

PLATE XXVII

Fig. 1

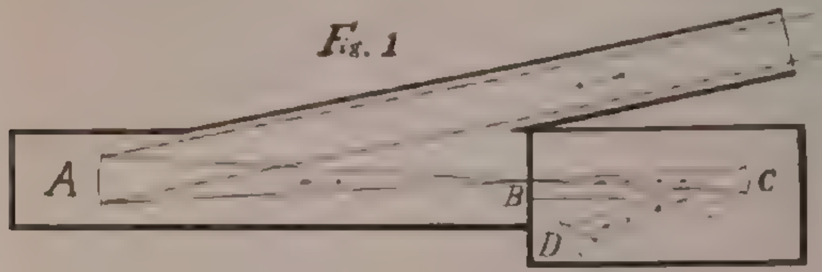


Fig. 2.

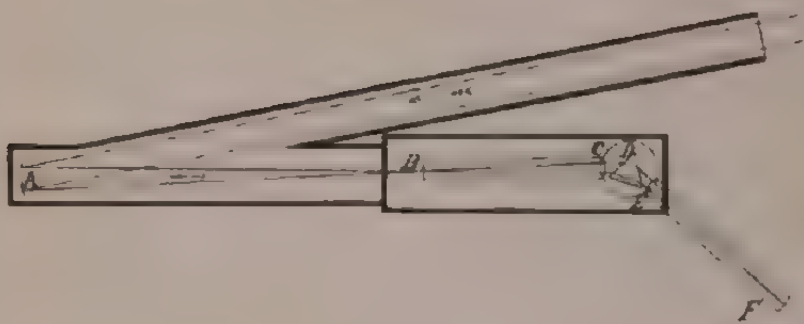
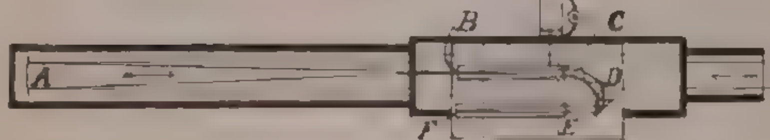


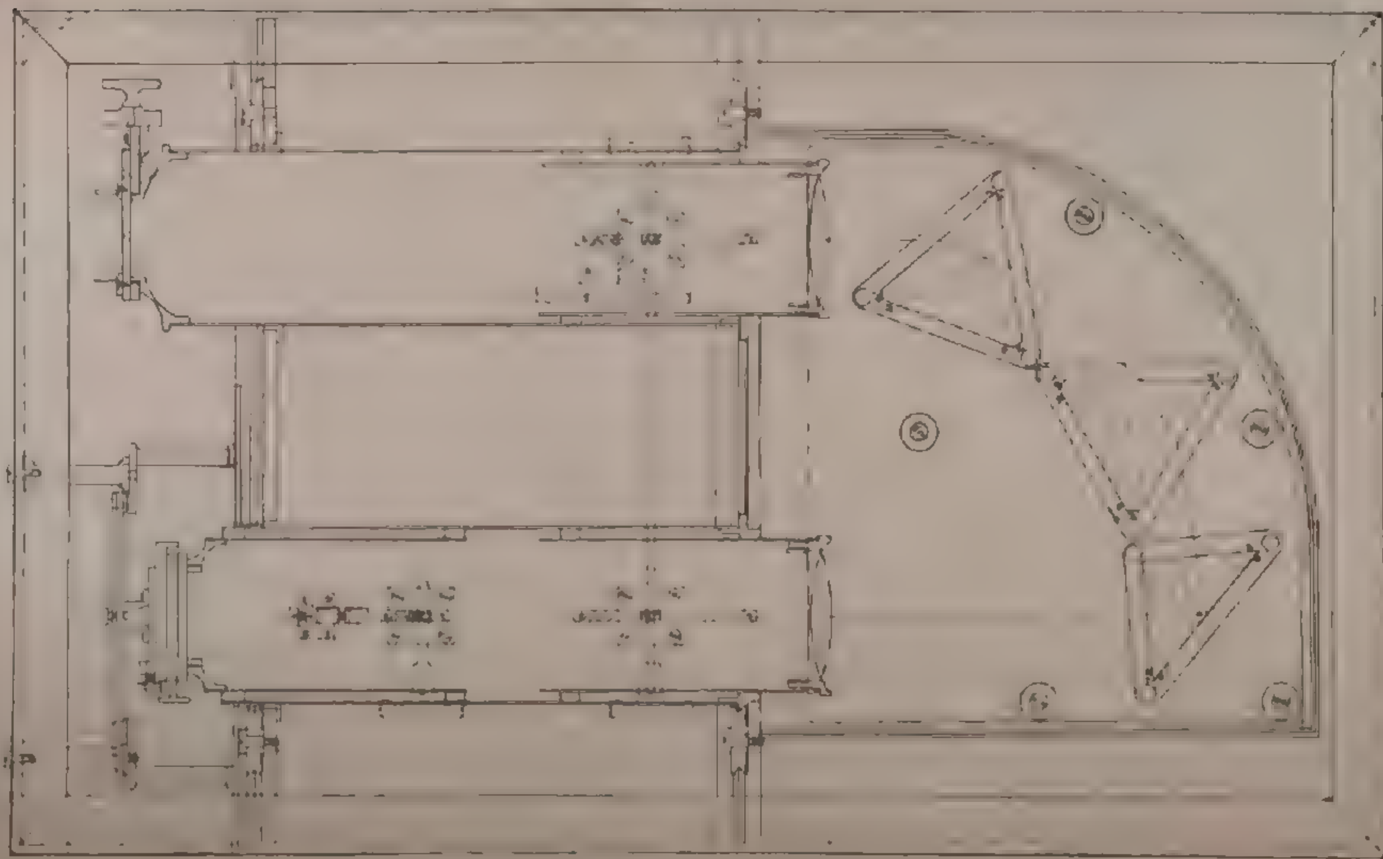
Fig. 3.



THE KENWOOD, PIKE'S PEAK AND MOUNT ETNA CORONAGRAPHS

(SCHEMATIC)

PLATE XXVIII.



Mr A. Cowper Ranyard's Spectroheliograph

Astronomy and Astro-Physics No. 128

ever, that the use of a reflecting surface in the prism train would be seriously disadvantageous in the delicate work on the corona, though not objectionable for investigations of the chromosphere and prominences. But the necessarily short focal-length of the collimator and telescope introduced such difficulties in the use of moving slits that it was finally decided that the mechanical advantages of the spectroheliograph with fixed slits would outweigh the slight objections that might be urged against it from an optical point of view. This reason, and the fact that the apparatus was ultimately intended for prominence work, decided me in favor of the design outlined in Fig. 3, Plate XXVII, and shown in more detail in Plate XXVIII.

The collimator and telescope are of $2\frac{1}{4}$ inches clear aperture and $11\frac{3}{4}$ inches focal length, and are rigidly fixed with their axes parallel. The objectives are focussed simultaneously by turning a steel rod carrying both pinions, and their position is given by a millimeter scale on the collimator tube. The frame which carries the telescopes supports also the tight metallic box containing the prism train (which is permanently fixed at the position of minimum deviation for K), and the whole can be moved in the plane of dispersion on eight grooved wheels running on four knife-edge rails attached to the top and bottom of the outside supporting steel frame. The first slit is $1\frac{3}{8}$ inches long, with one jaw fixed, and the other adjustable in width by means of a micrometer head. A narrow steel arm, carrying a blackened disk slightly exceeding the solar image in diameter, can be swung into or out of place, thus providing a means of excluding direct sunlight from the spectroheliograph. The second slit is of the same length, as the first, and has blackened jaws curved to correspond with the curvature of the K line. By means of a fine screw the slit can be moved as a whole in the focal plane, thus allowing the dark K line to be brought centrally between the jaws. The slit is then narrowed down to the proper width, both jaws being moved together toward the center by means of another micrometer screw. As the slit cannot conveniently be observed directly when the apparatus is in use, it is viewed through a microscope of low magnifying power with diagonal prism. The metallic plate-holder carries plates $2\frac{3}{8} \times 3\frac{5}{16}$ inches, and is held in a light-tight box with the sensitive surface of the plate in the focal plane. After the slide is drawn the second slit is brought almost into contact with the plate by means of a rack and pinion on the telescope tube. The objectives and prism train are of carefully selected glass, and were specially made for this apparatus by the Zeiss Optical Co.,

under the direction of Professor Abbe and Dr. Czapski, to whom I am indebted for undertaking the calculation of the curves of objectives best suited to the purpose in view. The large size of the prisms was rendered necessary by the short focal length of Mr. Ranyard's reflector. The advantages of this form of spectroheliograph are very evident in this connection, for with moving slits the prisms would have to be much larger. I should, of course, have preferred to use a collimator and telescope of twice the focal length of those here employed, but this, for obvious reasons, was out of the question. Had it been possible, one 60° prism, with the reflecting prism, would have been sufficient. With the short collimator and telescope, and the low dispersion of the light crown glass of the prisms, the K line is only about $\frac{1}{180}$ of an inch wide, and it could not advantageously be narrower. The clepsydra, which is not shown in the drawing, is similar to those employed at the Kenwood Observatory. It is attached to one side of the outer frame, and the piston rod is screwed to a projection on the side of the collimator near the objective. A cord is attached to a ring on the movable frame, and after passing around two pulleys on the fixed frame, and one hanging from the dome of the Observatory, it terminates in an iron weight. This furnishes the moving power, and the clepsydra acts merely as a regulator of the speed, the liquid (a mixture of 1 part glycerine and 20 parts water) being forced through a brass pipe from one end of the cylinder to the other. A micrometer valve in the pipe regulates the size of the opening and consequently the speed of the piston. This simple means of producing a uniform rectilinear motion is very satisfactory, as the movement is smooth, and its rapidity can be varied within wide limits.

The spectroheliograph was carried in the skeleton iron frame previously used with the Pike's Peak instrument, and the same telescope tubes and support for the mirror were used. The silver-on-glass mirror was replaced by a 4-inch mirror of speculum metal by Brashear of the same focal length (48 inches). The telescope tubes, and the collimator, telescope and prism train of the spectroheliograph were provided with a large number of diaphragms to guard against diffuse light.

Through the kindness of Dr. Rubens an attempt was made at the Physical Institute of the University of Berlin to deposit a film of platinum and gold on my glass mirror by an electrical process used with great success in silvering smaller mirrors. The advantages of such a mirror for the Mount Etna expedition

would lie in its freedom from attack by the sulphurous fumes, and in the perfect polish of the surface, for with the electrical process no hand polishing is necessary, and the mirrors are consequently wholly free from microscopic scratches. A large number of experiments were made, but it was found impossible to get a satisfactory deposit of platinum and gold on so large a surface. Of course silver could not be used, on account of the sulphurous fumes of the volcano. My thanks are due to Dr. Rubens for the numerous attempts he was kind enough to make in the hope of securing the desired deposit. It is to be hoped that future improvements of this process may render possible the production of perfect platinum and gold films. The silvered mirrors are to be highly recommended when circumstances permit of their use.

Our party, consisting of Professor Riccò, Director of the Bellini Observatories of Catania and Etna, Signorina Riccò, Antonino Capra, mechanician of the observatories, Mrs. Hale and the writer, left Catania on July 7, 1894. After a drive of three hours we arrived at Nicolosi, where we spent the night. I refer to our note-book for the following:

JULY 8. Left Nicolosi at 6 A. M. Our party had been joined by Antonio Galvagno, custodian of the Etna Observatory, and Santo Messina, assistant. Ten mules managed by three muleteers were needed to carry the members of the party and the apparatus, provisions, etc. Arrived at Casa del Bosco (1450^{m}) at $8^{h}\ 30^{m}$. Examined sky frequently, and found slight decrease of whiteness as we ascended. Crossed lava stream of 1892, and had excellent view of the craters of that year, the latest of which still emits vapor. Arrived at the Observatory at $1^{h}\ 35^{m}$. The temperature had fallen to 9° C., and the sky was nearly covered with clouds. Half an hour later we were enveloped in cloud, which surrounded us until evening, when sky was whitish, with marked halo around Moon. Stars unsteady, even in zenith.

JULY 9. Sky clear, with strong wind blowing the smoke from the great crater (which rose behind the Observatory to an altitude of 3312^{m}) away from the direction of the Sun. Half the Island of Sicily was dimly visible from the Observatory through a great brown bank of thick haze, the upper surface of which seemed to be nearly on a level with us. Cumulus clouds commenced to form at 9^{h}, and soon the sky was nearly covered. At 12^{h} the Sun was seen between passing clouds to be surrounded with a bright halo. Unpacked and cleaned apparatus, put part of it together and made fittings for attaching it to tube of 12-inch telescope. Wind changed to west in the afternoon, and sky became much whiter.

PLATE XXIX.

JULY 10. Wind blew smoke of great crater over Sun, making sky very white. Observed Sun with Professor Riccò by projection with 12-inch telescope. Image rather better than at Catania, but became unsteady later. At 10^h some small cumulus clouds had formed, and Sun was surrounded by bright halo. Clouds of insects were also noticed in direction of Sun, as on Pike's Peak. Observed prominences with Professor Riccò, but images were no better than at Catania. Attached spectroheliograph to telescope tube (see Fig. 2, Plate XXX). At sunset watched shadow of Etna from the Torre del Filosofo. Whole sky covered with dense haze. At 7^h 30^m made exposures on Moon with Huggins coronagraph, but obtained nothing with 4 seconds.

JULY 11. Sky very white, bright ring around Sun. Observed atmospheric lines with direct vision spectroscope. Balanced telescope, and observed Sun by projection. Seeing excellent, granulation, spots and faculæ well defined. Strong odor of sulphur. Attached clepsydra and found motion of spectroheliograph to be very uniform. Adjusted mirror, and found effective aperture to be 3¼ inches. Adjusted spectroheliograph; definition of spectrum excellent; very little diffuse light. Made several photographs of spectrum for focus. With 3 seconds exposure all came out positives (using direct sunlight). Made an exposure for faculæ, motion perfect, and exposure uniform clear across image. At sunset visited Valle del Bove. Sky filled with haze, and almost too bright for the eye 10° from Sun. Professor Riccò made several exposures on Moon with Huggins coronagraph. Nothing shown with 4 seconds. Photographed lunar spectrum with spectroheliograph, and obtained fair result with 40 seconds exposure (Schleussner plate). If the lower corona were no brighter than the Moon in first quarter and the second slit were $\frac{1}{16}$ of an inch wide, the movement of the spectroheliograph should be 1 inch in 166 minutes. The diameter of the solar image is less than ½ inch and a motion of the slits of perhaps ¼ inch might suffice to indicate the presence of coronal structure. As the full Moon is considerably brighter than the Moon at first quarter, and as the inner corona is probably brighter than the full Moon, a motion of ½ inch in 25 minutes should suffice.

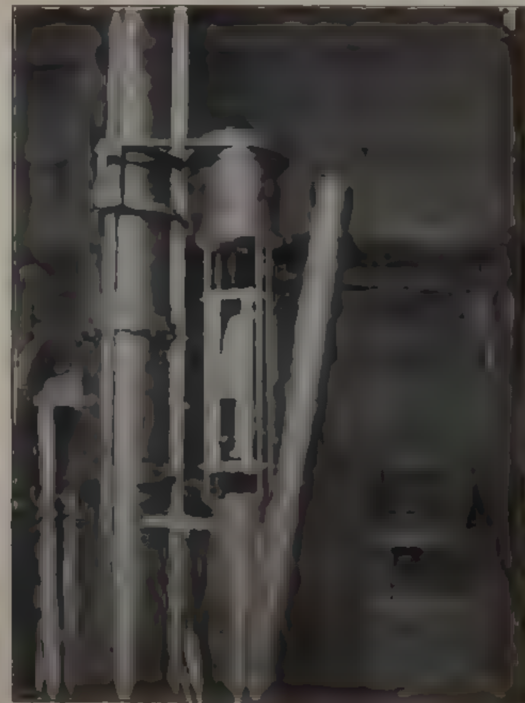
JULY 12. Sky very white. Wind still blowing smoke from crater over Sun. Bank of haze above level of Observatory. Observed Sun by projection with Professor Riccò; image unsteady. Made several photographs of spectra and facula plates with spectroheliograph. Improved adjustment of mirror for focus. Climbed to top of great crater, and found sky in zenith

PLATE XXX.

FIG. 1

MOVING SLITS OF THE KENWOOD OBSERVATORY CORONAGRAPH.

FIG. 2.



CORONAGRAPH ATTACHED TO THE 12-INCH EQUATORIAL OF THE MOUNT ETNA OBSERVATORY.

of deeper blue than when seen from Observatory. Whole Island enveloped in haze. Descended to Observatory by moonlight, double halo around Moon. Observed Moon, Saturn and several stars with the 12-inch, using powers up to 430. Seeing *magnificent*; images almost perfectly steady with highest power. Both Moon and Saturn were very low, but images were remarkably good. With naked eye scintillation was hardly perceptible in stars higher than 30°.

JULY 13. Wind blowing from direction of crater, but sky best since July 9. Sky cloudless and generally whitish, but increase in brightness toward Sun was gradual. Much dust. Telescope in use until 9^h 40^m by Professor Riccò for daily record of chromosphere. Prominences very well seen. At 9^h 50^m broad and brilliant ring of whiteness around Sun, making it useless to try for corona. Smoke blowing directly over Sun, and diffusing through entire sky. Solar image observed by projection; definition very poor. At 11^h sky had improved, and preparations were made to photograph corona, but five minutes later more smoke blew over Sun, and sky became very white. Mirror found to be dewed, and surface badly tarnished by the sulphurous fumes, though it had been tightly covered every moment it was not in use. Sky around Sun remained bright, and wind was so violent that no photographs could be made. Strong sulphurous odor.

JULY 14. Smoke blowing across Sun. Strong sulphurous odor. Whole eastern sky white. Prominences fairly well seen at 7^h 45^m. Professor Riccò made several facula plates with spectroheliograph. Took out the tarnished mirror, but left apparatus attached to telescope, carefully wrapped up to protect it from the sulphurous vapors. Left Observatory at 3^h, and arrived at Catania about midnight.

JULY 15. Left Catania for Rome. During the journey to Messina heavy clouds came up from the west, and when we last saw the Island after leaving Reggio it was almost hidden by clouds, with Etna faintly visible through a thick haze.

Before leaving Catania I cleaned and polished the mirror as well as I could, but the result of the action of the sulphurous vapors of the volcano remained plainly visible in spite of my efforts to remove it. The apparatus had been left on the mountain, as Professor Riccò had kindly consented to attempt to photograph the corona with it in August, if the weather and sky were favorable. It had been hoped that the platinum-gold mirror mentioned above would be ready for use on this occasion, but failure to produce a perfect surface left him only the tarnished

speculum metal mirror, and good results could hardly be expected from this.

As I am assured by Professors Tacchini and Riccò that the sky is frequently very good on Etna I conclude that the difficulties we encountered were exceptional. During the entire time of our stay in southern Italy and Sicily the atmosphere was very hazy, and the sky was rarely of a deep blue. I was told by Galvagno, the custodian of the Etna Observatory, that the smoke this year has been much more noticeable than usual. If the wind had blown it away from, instead of toward us, the sky would probably have been good, though I think by no means equal to the sky seen on Pike's Peak during the first part of our stay.

I take pleasure in acknowledging our sense of deep indebtedness to Professor Tacchini, and to Professor Riccò and his assistants, for them any favors shown us during our stay in Italy.

SUGGESTIONS FOR FUTURE WORK.

While it can hardly be said that the results of my various attempts to photograph the corona without an eclipse have been at all encouraging, I have by no means abandoned hope that the method, if fairly tried under good conditions, may yet be successful. In choosing a site great care should be taken. On Pike's Peak the dust was very troublesome, and a small bellows was therefore provided to be used on Mount Etna to blow the dust from the mirror during the exposure. There was much more dust on Etna than on the Peak, and the bellows would have been useful had it been possible to carry out the proposed experiments on the corona. A snow-covered peak might offer important advantages in this and in other particulars. The season chosen should naturally be not far from the summer solstice, but the local meteorlogical records should be consulted before fixing the time. An altitude of at least 13,000 feet should be selected, and the higher one goes the better. A low latitude would naturally be preferred to a high one, on account of the altitude of the Sun.

In conclusion, I may say that the investigation has been a fascinating one, in spite of its succession of failures. On account of the importance of discovering some means of observing the corona without an eclipse, it is to be hoped that others may think it worth their while to continue the attack on this difficult problem.

KENWOOD OBSERVATORY, University of Chicago.
Sept. 7, 1894.

ADDENDUM. The following extracts are from a letter received from Professor Riccò just as the article is going to press.

CATANIA, Aug. 31, 1894.

"I did not return from the Etna Observatory until Monday (Aug. 27) as the weather continued unfavorable after your apparatus was once more ready for use. On the 24th, however, the sky was absolutely blue up to the Sun, and around it there was no halo; on the 25th it was cloudy, and on the 26th the Sun was surrounded by a faint whitish halo on a blue sky. The gold-platinum mirror did not reach me, and the other, as you know, was very considerably tarnished, and perhaps still more so after the experiments I had made in photographing the corona. I attempted to polish it, as you had done, with distilled water and alcohol applied with absorbent cotton; I carried this operation to such a point that in the shade the mirror appeared quite clean, but in the sunlight a veil of oxide or sulphur was still visible, and I had to use the mirror in this condition.

I made about twenty photographs of the corona—seven of them on the 24th, when the sky near the Sun seemed to be perfect. I saw immediately that the necessary exposure was much less than the estimated value: 37 minutes for 11 mm. With an exposure of 37^m for the entire run of the spectroheliograph the sky around the Sun is so dark that no trace of the corona can be seen. But with exposures ranging from 1 to 10 minutes for the whole run of $0^m.05$, coronal images are obtained. As you will see in the plates which I send, the corona is plainly visible, and is better than that obtained with the Huggins apparatus, a copy of which—one of the last attempts I made on Etna when the sky was very good—I also send you. But this advantage may be only apparent, and due to the smaller size of the solar image in your apparatus. I have not been able to see details or indications of structure in the corona, and this leads me to suspect that we are still dealing with an atmospheric corona. It is, however, remarkable that while nothing was visible to the eye near the Sun, your apparatus gives a corona marked by a rift which is obscure in the light of the atmosphere!"

The photographs of which Professor Riccò speaks have not yet arrived, but the short exposures with which they were obtained seem to be insuffiicient to show the true corona. On account of the large amount of diffuse light which the tarnished mirror must have given rise to, it is not surprising that the sky around the Sun was fogged with the longer exposures.

PROFESSOR FROST'S TRANSLATION AND REVISION OF DIE SPECTRALANALYSE DER GESTIRNE *

JAMES E. KEELER

When Dr. Scheiner's "Spectralanalyse der Gestirne" appeared, about the end of 1890, it took its place at once as the standard treatise on astronomical spectroscopy. There was, in fact, no other book that met the requirements of the student and the specialist. The well-known treatise of Schellen, excellent of its kind and in its day, was of too popular a character for this purpose, and notwithstanding several revisions by competent authorities, it had been left far behind by the advance of knowledge, so that in 1890 it represented very imperfectly the state of a science whose growth has been so phenomenal as that of celestial spectroscopy. The value of Dr. Scheiner's book was at once recognized. It was thorough, well arranged and well balanced; it adequately represented the latest stage of astro-physical inquiry; and it was written in a clear and simple style, especially marked by the absence of the "wounded snake" sentences and intricate nests of dependent clauses which make some German works such exasperating reading for the foreigner.

All these good qualities are preserved in Professor Frost's translation, which follows the original quite closely in such portions as have not required modification in view of recent discovery. The English work is, however, very far from being a mere translation. Much new matter has been added, and some portions of the original, relating to subjects in which the advance of knowledge has been most rapid, have been largely rewritten. Notwithstanding this amount of additional matter, the size of the the book has not been increased, partly on account of the somewhat smaller type in which it is printed, but mainly owing to the fact that about one page in ten was saved in translating from German into English. (The conclusion which Professor Frost draws from this as to the relative compactness of the two languages is perhaps somewhat too general). The book is thus brought fully up to the date of publication, and since the proof was submitted to Professor Scheiner for criticism before printing, it may be regarded as a revised edition in English of the "Spec-

* *A Treatise on Astronomical Spectroscopy*: being a translation of "Die Spectralanalyse der Gestirne," by Professor Dr. J. Scheiner, Assistant at the Royal Astrophysical Observatory at Potsdam. Translated, revised and enlarged by Edwin Brant Frost, M. A., Assistant Professor of Astronomy in Dartmouth College. Ginn & Company, Boston and London, 1894.

tralanalyse der Gestirne." The treatment of all recent matter is, however, Professor Frost's. Owing to the difficulties and delays of communication, it was possible to submit only the electrotype proofs to Professor Scheiner, and in these no changes could be made. When the translator and the author differed in their views, a note was made by the author, expressing his dissent from the statements in the text, and the few pages in which these notes are collected are not the least interesting part of the book. The conservative attitude of Professor Scheiner may be noticed here, and in some cases his caution seems to be carried to an extreme. In view of the evidence accumulated by Lockyer and other observers, the measurements of Rowland, and the photographic comparisons by Kayser and Runge, most spectroscopists would regard the presence of carbon in the Sun as pretty well established. Some adverse comments on recent observations have been slightly softened down by the translator, while the brief review of Lockyer's meteoritic hypothesis in the original has been altogether omitted.

An important change, to which Professor Scheiner has given his assent, and which cannot fail to meet with general approval, is the substitution of Rowland's scale of wave-lengths for the Potsdam scale employed in the first edition. The absolute values are probably as nearly correct in one system as in the other, but for spectroscopic purposes the absolute values of wave-lengths are of far less importance than the relative ones, and while the Potsdam measurements were made with all the precision which was possible with the instruments available at the time, the large concave gratings and the photographic methods employed by Rowland give his measures a very great superiority in this respect. A further reason for preferring this system is that it has been adopted by all recent investigators of metallic spectra, and there can be no doubt that it will be exclusively used by spectroscopists in the future. At the same time a complete catalogue of the solar lines, from measurements of the original negatives, would be very useful, and it is to be hoped that such a catalogue will soon be forthcoming.

We now pass to a more detailed comparison of the translation with the original, in which, however, it is only possible to notice the more salient features. Comparatively few changes have been made in the chapters relating to spectroscopic apparatus. Schuster's method of adjusting the collimator and observing telescope is briefly described, and there is a paragraph on Lord Rayleigh's investigations on the resolving power of a spectroscope. A better

figure has been introduced to illustrate the action of the cylindrical lens. The method of studying the chromatic aberration of an objective by means of its color curve is given as in the original, but a knowledge of the color curve of a large telescope is so useful in practical work with the spectroscope that a more complete account of its determination, and of the precautions necessary to ensure accuracy in the observations, might well have found a place in a book designed for students.

In the second chapter the treatment of the concave grating is considerably expanded, and various practical details relating to its use are given. To the same chapter have been added a description of Michelson's interferential refractometer and its application to the study of spectral lines, and an account of Hale's spectroheliograph, illustrated by a plate. Another plate, from ASTRONOMY AND ASTRO-PHYSICS, shows the details of the Lick spectroscope.

Part II, on spectroscopic theories (Kirchhoff's law and Doppler's principle) has been left unchanged, except that two pages have been added on special applications of Doppler's principle, such as measurements of the Sun's rotation, the discovery of spectroscopic binaries, and motions of gases in the atmosphere of the Sun.

The changes in the chapter on the solar spectrum consist mainly in the substitution of more accurate wave-lengths for the values given in the earlier edition. The remarks by Dr. Scheiner on the unsatisfactory state of the wave-lengths of metallic lines now fortunately admit of considerable modification. Certain identification of solar with metallic lines has in many cases taken the place of conjecture, and Rowland's tables of comparison are given at length. In this connection the bearing of Kayser and Runge's researches on the identification of solar lines might have been referred to. Among the important additions is an account of Langley's work in the infra-red region of the spectrum. The recent investigations of Hale are of course explained at length, and Young's table of chromospheric lines has been considerably extended toward the ultra-violet as one of the results of these researches and those of Deslandres.

The chapter on the spectra of the planets remains unchanged, as no important observations have been made in this field since the publication of the first edition

Extensive changes have been made in the chapters devoted to the spectra of the stars and nebulæ, corresponding to the great advances which have been made in those departments, and in

fact the matter in this portion of the book has been largely recast. Vogel's classification of stellar spectra, as modified by Scheiner, is naturally the one adopted, though Professor Frost has added the following comment (p. 238): "[This system of classification has necessarily been followed by the translator, but it is proper to state that many of the leading spectroscopists are of the opinion that the time has not yet come for an attempt at a classification along the lines of stellar development, and that any classification must for the present be regarded simply as provisional.]" These remarks, being enclosed in brackets, do not seem to meet with the unqualified approval of the author; nevertheless, the most recent, as well as the earlier investigations, justify their insertion. Only the upper portions of star spectra, where photography is applicable, have been adequately studied, and it would be easy to mention special cases in which a classification based on the appearance of this region would be in error, or at least fail to represent the entire truth. The stars of class I*b* are too few in number to be regarded as a general transition form, and no classification except Lockyer's attempts to trace the connection between Secchi's third and fourth types; while the facts brought out by the researches of Campbell on the Wolf-Rayet stars, the character of the spectra of the nuclei of planetary nebulæ, and the association of stars of class I*a* and class I*c* with extended nebulæ show that there is still much doubt as to the position which should be assigned to the various kinds of bright-line stars in a general classification based on a theory of development.

Among the more important additions to the subject of stellar spectroscopy, we naturally find full discussions of β Lyræ and Nova Aurigæ. Belopolsky's photographic observations of the former star, and the conclusions which he draws from them are given; those of Vogel at Potsdam probably appeared too late to be included in the text, and are only referred to by Professor Scheiner in a note. In view of these latter observations, Professor Scheiner considers, no doubt justly, that Belopolsky's comparatively simple hypothesis respecting the system of β Lyræ is inadequate to explain the facts. The Potsdam observations, as well as those of Lockyer, still more recently published, make it probable that the system contains more than two bodies.

An excellent abstract is given of the large mass of material obtained by observations of Nova Aurigæ, and the various hypotheses advanced to account for the observed phenomena are impartially discussed. In one of his notes on page ix, Professor

Scheiner singularly enough makes the objection to Seeliger's hypothesis that it does not at all relieve the difficulty of accounting for the high relative velocity of the bodies composing the system; whereas most persons who have followed Professor Seeliger's arguments would probably agree that his hypothesis accounts satisfactorily for the high relative velocities, but does not explain the very great absolute velocity of 600 or 700 kilometres per second which the body giving the absorption spectrum must have possessed, or the changes in its velocity indicated by the observations at Mt. Hamilton. The importance of the remarkable fact that the spectrum of Nova Normæ was almost identical with that of Nova Aurigæ, not only with regard to the bright and dark lines, but to the amount and direction of their relative displacement, has probably not yet been fully recognized. Should other new stars be found to have the same spectrum, our ideas as to the nature of these apparitions would undergo a profound change. Several plates illustrating the spectrum of Nova Aurigæ have been introduced, among them an enlargement of a photograph taken by the translator with the small spectrograph at Potsdam.

A considerable amount of new material has been added to the account of the Wolf-Rayet stars, and Campbell's measurements of the bright lines in their spectra are given in full.

The important chapter on the displacements of spectral lines has been considerably enlarged. Campbell's convenient tables for reducing spectroscopic observations of motions in the line of sight are reproduced, but the English miles have been changed into kilometres, as German geographic miles and other units have been at other places throughout the book. The photographic methods used at Potsdam, with which Professor Frost is personally familiar, are briefly but clearly described, and Vogel's results for the motions of fifty-one stars in the line of sight are given, while the detailed comparison with the Greenwich results in the original has been omitted. At the end of the chapter is an account of Keeler's observations of nebulæ with the Lick refractor, and his table showing the motions of various nebulæ in the line of sight.

In Part IV, devoted to spectroscopic tables, Rowland's table of standard wave-lengths replaces the Potsdam tables in the first edition. Abney's wave-lengths of lines in the ultra-red are given as originally printed, on Angström's scale, the factors for reduction being unknown in this part of the spectrum. The wave-lengths of iron lines in Table III are taken from Kayser and Runge's memoir, but only such lines are given as were included in

the lists of Thalén or (for the ultra-violet portion) of Cornu. About 230 stars, mainly derived from the observations of Espin and Pickering, have been added to the catalogue of stars of classes III*a* and III*b*. Table V contains a partial revision by Professor Young of his earlier list of chromospheric lines, the wave-lengths being given on Rowland's scale; and finally the bibliography of spectroscopic literature has been revised and brought up to date.

Only the more prominent features are touched upon in the above comparison, for it is impossible to specially notice the great number of small additions, liberally interspersed throughout the original matter, which greatly increase the value of the work. It must suffice to say that full justice has been done to the labors of recent observers and that the book well represents the state of celestial spectroscopy at the present time. It would be strange if a few statements in the text should not already require modification, for the advance of this department of science is so rapid that facts accumulate even while an inventory is being taken. Thus we find that the parts relating to the spectrum of the Orion nebula and its relation to the spectra of the associated stars do not represent the results of the very latest investigations, while the statement on p. 251, that the D_3 line has never been observed as a dark line in stars whose spectra show only dark lines, must now be regarded as erroneous, since it has been shown that D_3 is dark in the spectrum of Rigel.

"Astronomical Spectroscopy" is admirably adapted to the requirements of the specialist, and large portions of it cannot fail to interest the amateur and general reader. It is well printed by Messrs. Ginn & Co., to whom Professor Frost acknowledges his obligations for undertaking the publication of a book from which pecuniary profit is hardly to be expected.

ASTRO-PHYSICAL NOTES.

All articles and correspondence relating to spectroscopy and other subjects properly included in ASTRO-PHYSICS, should be addressed to George E. Hale, Kenwood Observatory of the University of Chicago, Chicago, U. S. A. Authors of papers are requested to refer to last page for information in regard to illustrations, reprint copies, etc.

A Collection of Astronomical Photographs.*—With the exception of the excellent plates which Mr Ranyard has published in *Knowledge* and the few

* A Selection of Photographs of Stars, Star Clusters and Nebulæ, together with information concerning the instruments and the methods employed in the pursuit of celestial photography. By Isaac Roberts D. Sc., F. R. S. (London, The Universal Press, 326, High Holborn, W. C.)

the method employed in collimating the speculum of the telescope, one is led to think that a plane mirror, with its center cut out to contain the plate carrier, thus leaving a strip of mirror perhaps half an inch wide on each side of the plate, might perhaps serve well as a substitute, or at least as a useful aid, for the guiding telescope, which in any case seems out of place at the end of the declination axis, where the danger of unequal flexure is a maximum. But if the instrument meets the severe tests described in the following paragraphs it can hardly be considered to possess any serious defects. After some remarks on the purely photographic questions involved, the description of the plate of the Great Nebula in Andromeda introduces us to the principal portion of the volume. The fifty three plates, which in all cases are from enlargements of the original negatives, are reproduced in a fairly satisfactory way, but some of them—notably the remarkable photograph of the Great Nebula in Andromeda—have been much more successfully brought out in the pages of *Knowledge*. Anyone familiar with the photo-gravure process will appreciate, however, the immense difficulties attending the publication of a work of this kind. The only criticism we feel inclined to make is that many of the photographs, notably those of star clusters, have been enlarged so much that the grain of the plate becomes unpleasantly prominent. We do not share the rather common belief that great enlargement improves astronomical photographs. A certain amount of enlargement is often necessary to bring out on prints certain small details of structure, but it should always be remembered that the enlargement can show nothing which is not on the original negative, and it is usually best not to arrive at the point where the silver grain begins to be conspicuous.

But these matters are of minor importance, and we take great pleasure in recommending this unique collection of admirable photographs to the rapidly increasing number of astronomical photographers and to all interested in stellar astronomy.

Note on the Spectrum of the Orion Nebula.—In reference to Dr. Huggins' recent note on the Spectrum of the Great Nebula in Orion based upon my paper* on the same subject, I desire to offer some comment.

1. I think no one has questioned the *justice of the interpretation* which Dr. and Mrs. Huggins put upon their 1888 photograph; but it seems to me there is a reasonable doubt of the *justice of the photograph.*

We know the Trapezium stars have broad dark hydrogen lines. The 1888 photograph shows their spectra to be strictly continuous at the positions of the hydrogen lines.

The 1888 photograph differs from all subsequent photographs, and ascribes a strange character to the nebular spectrum; whereas the subsequent photographs apparently agree in proving that the Orion Nebula spectrum is substantially identical with all other well-known nebular spectra.

2. Dr. Huggins writes: "As Professor Campbell's remarks on the broadening of certain portions of the lines upon our plates (pp. 391 393) seem to show that he has not understood correctly the interpretation we put upon this appearance, I may say now that the view we took, and still hold is that this broadening is purely a photographic spreading on account of the greater brightness of the line at that place."

I am at a loss to know how I could have been expected so to interpret their original remarks. They have made it clear that the "abruptly different intensi-

* In Astronomy and Astro-Physics for May, 1894.

ties of different parts" of the line are due to the different brightness of different parts of the nebula (*Proc. Roy. Soc.*, vol. 48, p. 215), as indeed one would expect. By way of explanation of the *broadening* of the lines on their 1888 negative, they said in 1889, "On one side of the [Trapezium] star spectra, this line [λ 3724] is a little broader than on the other side; but as a similar appearance is presented by $H\gamma$ and the stronger lines of the group, it may arise from some optical or photographic cause." By way of comment on the above sentence, they said in 1890: "We now learn [from the 1890 photographs] that this difference between two parts of the lines indicates *probably a different condition of the nebula on the two sides of the star spectra.*"* I believe the natural interpretation of this sentence is that the broadening was caused, not by "some optical or photographic cause," as suspected in 1889, but on the contrary that it was *real* and due to "probably a different condition of the nebula" on the two sides of the Trapezium. I certainly accept, most gladly, Dr. Huggins' recent interpretation.

3. In regard to "the known variation in the visible region of the principal line to the line of hydrogen at F," I believe my interpretation of the four passages quoted (ASTRONOMY AND ASTRO-PHYSICS, pp. 386-7) is perfectly just, viz.: "some small differences of relative brilliancy" were suspected, but nevertheless were doubtful.

There can be no doubt that the relative intensities of the principal line and the $H\beta$ line vary enormously. We may say that the ratio of their intensities varies from about 4:1 to about 4:20 for different parts of the nebula.

4. The advantage in using a short camera for reducing the exposure time with stellar spectra has long been known, and my paper had nothing to do with that. I stated that the relatively short camera "applies effectively to the study of all large objects yielding bright-line spectra: comets, large nebulæ, auroræ boreales, etc.," a very different matter, involving very different principles. I have not seen these principles correctly stated before, and it was my purpose to call attention to their simplicity and the great importance of utilizing them.

Mt. Hamilton, 1894, August 16. W. W. CAMPBELL.

Dr. Huggins' 1888 Photograph of the Spectrum of the Great Nebula in Orion.—During a recent visit to the Tulse Hill Observatory I had the pleasure of examining, at Dr. Huggins' request, the remarkable photograph of the Orion nebula spectrum taken in 1888. The cut in *Proc. Roy. Soc.*, v. 46, p. 60 certainly represents very closely the appearance of the negative, which is in an excellent state of preservation. The pairs of lines on either side of the very strong line at 3724 were easily seen, and most of the lines in the group between 3825 and [illegible] were visible, though they were very faint. The lines at 3959, 3975, 3988 and 3998 were seen without difficulty, but the six lines between 4116 and 4167 were so faint that with the illumination used they were made out with great difficulty in the part of the spectrum due to the nebula. The increase in width and brightness of all the lines in the star spectrum was most striking, and could not have been overlooked by the most careless observer. It is probable that with suitable illumination and more time at my disposal I could have seen all the [illegible] with ease. I could not find the slightest trace of the missing hydrogen lines.

GEORGE E. HALE.

A New Triple Achromatic Object-Glass.—*To the Editors of Astronomy and Astro-Physics*: I shall be much obliged if you will afford me space in your widely

* The italics are mine.

read journal for a few remarks upon the new Cooke Photo-Visual Objective, of which you gave a short account in your issue for May (page 400). For that account raises two or three points which would naturally remain unsettled in the minds of your readers, in the absence of further information.

1. It is pointed out that in my original paper read before the Royal Astronomical Society, I did not state whether the lenses were to be cemented together or not, and you rightly concluded that cementing might seriously deteriorate such a lens if of moderate or large size. This is the case, and I came to the conclusion that it would be almost impossible to so perfectly cement a large lens as to show no signs of strain at the focus. Moreover, even supposing the 2d and 3d surfaces to be cemented together, yet the 4th and 5th surfaces certainly could not be cemented together, for it is *essential* to the good performance and perfect achromatism of this objective that the 4th and 5th surfaces should be separated by an interval. Your account then proceeds to say "and without it (cementing) gradual tarnishing of the interior surfaces would seem to be unavoidable; nevertheless Messrs. Cooke & Sons guarantee the permanence of objectives made on this plan."

Here Messrs. Cooke & Sons' guarantee seems to excite surprise. Now it is of course open to anybody to doubt the practical durability of the polish and transparency of the borosilicate flint glass used for this objective in spite of the best possible reasons to the contrary; yet there are many with whom the following considerations will have much weight.

Dr. Schott and Professor Abbe of Jena, as well as two other independent chemical experts, have been consulted as to the cause of a certain iridescent but perfectly transparent film which forms over a polished surface of this flint under certain circumstances. All four were unanimously of opinion that, looking to the composition of the glass (which contains boracic acid) the only impurities in atmospheric air which could tarnish it are the sulphur compounds, sulphuretted hydrogen, sulphurous acid and sulphuric acid. These impurities are well known to exist in the products of gas combustion, and it is a significant fact that a film of tarnish can be produced on this glass by a few days' open exposure in a room in which gas lights are burning, whereas if kept closed up from contact with much atmospheric impurities, no tarnish whatever ensues; pure air or even damp air have been proved to have no effect whatever upon it. Such a prolonged exposure to a damp air, as will cover a piece of ordinary hard crown glass with mould, will not touch the borosilicate flint in the least.

Now the method of mounting in its cell adopted for this new objective, renders the penetration of impure air, dampness or even dust, to the interior surface of the lenses practically impossible. To all intents and purposes the objective is hermetically sealed. I cannot enter here into an account of how this result is achieved.

2. Your account then refers to the double objective invented and patented by Professor Hastings some years ago, in which he made use of Schott's potassium silicate crown 0.13 and borosilicate flint 0.161. It is thus intimated that that flint is "nearly identical with that used by Mr. Taylor," and also that my objective "does not seem to differ materially from that proposed by Hastings."

I would like to point out that whereas the reciprocal value of the dispersion power or $\frac{\mu_D - 1}{\Delta\mu(\mathrm{C\ to\ F})}$ of the borosilicate flint used by Professor Hastings is 46.7, the reciprocal value of the dispersion power for the new borosilicate flint used in my objective varies between 50.2 and 50.6, a very considerable difference. More-

over Dr. Schott states that the former glass, used by Professor Hastings, is no longer manufactured, for it cannot be made good enough. It cannot be asserted that this constitutes no material difference.

The U. S. patent examiners kindly sent us a short while ago (*not* in an obstructive spirit) a copy of Professor Hastings' patent specifications, which I was much interested in reading. If I remember aright, he claimed that his combination reduced the secondary spectrum to a little less than half of its present amount. This is the real amount of the reduction, and not to 5 per cent as stated in your account.

I will here give the secondary longitudinal color aberrations of, 1st, the ordinary double combination of dense silicate flint and hard crown; 2nd, Professor Hastings' combination of potassium silicate crown and dense boro-silicate flint; and, 3rd, the Cooke photo-visual objective. All are calculated for an equal focal length of 360 inches and the secondary color aberrations are calculated from the figures given in Herrn Schott's catalogue and on the supposition in all three cases that the C and F rays are united to exactly the same focus.

	Ordinary double Objective. inches	Professor Hastings' Double O. G. inches	The Cooke Photo-Visual O. G. inches
A' (Red Potassium line)	about + .37	+ .319	− .03
C	0	0	0
D	− .12	0	+ .05
E	− .18	0	+ .03
F	0	0	0
G	about + .8	+ .376	+ .03
H_1	about + 1.4	?	+ .09

It scarcely needs to be pointed out that Professor Hastings' combination while achieving a possibly perfect concentration of all the rays between C and F nevertheless falls far short of bringing the blue photographic rays to the same focus, and therefore it could not be said to be equally available for photographic purposes. I would like to remark that the figures given in the 2d and 3rd columns follow from Messrs. Schott's figures, which are after all not accurate enough to enable the foci to be calculated, in the case of the deep curved Cooke O. G., within an error of about .05 inches in so great a focal length as 360 inches. The figures in column 3 indicate that the middle part of the spectrum should fall long. However, the best test of this matter is the careful trial of a real objective of moderate or large size.

I have proved in the case of a Cooke Photo-Visual O. G. of 5 inches aperture now almost perfected, that the central portion of the spectrum certainly does not fall beyond the focus for C and F. The most careful tests have proved conclusively that the residual secondary spectrum, and it is exceedingly minute and difficult to perceive, is of the same *tendency* as the secondary spectrum of an ordinary double objective, the brightest yellow green rays being refracted to the shortest focus, while the extreme red and extreme blue and violet rays focus very slightly long.

This I was very glad to find, because an extremely slight modification in the composition of the new melting of borosilicate flint which Messrs. Schott are now executing, or else in the composition of one or both of the other glasses, will enable me to get rid of all secondary spectrum absolutely.

In reply to your remarks upon the probable difficulty in figuring the negative lens with such a small central thickness, I will only point out that in the case of a

5-inch objective, I was anxious to see how thin it was possible to carry it, and in this case made it only .07 inch thick in the center. Nevertheless no difficulty whatever was experienced in working up its surfaces to a perfect spherical figure. I attribute the remarkable immunity from bending of this borosilicate flint to its extraordinary mechanical hardness, for rigidity and hardness go together.

H. DENNIS TAYLOR.

We are much obliged to Mr. Taylor for the information about his new objective, and must say that no fairer offer could be made than that of the Messrs. Cooke. With regard to the Hastings double objective mentioned in our May number, Mr. Taylor's recollection seems to be at fault. Although we cannot at present refer to the patent specifications, (which may possibly relate to some other form), the description of this objective in the *American Journal of Science*, vol. 37, shows that the secondary chromatic aberration is only six per cent of its amount in an objective of the usual construction, and not fifty per cent. The performance of the objective itself, which we have recently had the pleasure of examining, bears out the theoretical conclusions. The outstanding color around a bright star like Vega is hardly perceptible, and the images much resemble those formed by a reflector. We do not think however, that application to photography was contemplated in the construction of this telescope. Unfortunately the glass is hygroscopic, as stated in our former note.

The Progress of Astronomical Photography.—An address recently delivered before the section of Astronomy, Mathematics and Physics of the Australasian Association for the Advancement of Science, by the President, Mr. H. C. Russell, contains a valuable epitome of the progress of astronomical photography. Its positive statement that the corona has been photographed without an eclipse will be misleading to some, and a few other minor mistakes might be mentioned. In an article apparently aiming at some degree of completeness it is remarkable, as Miss Clerke has recently pointed out in *Knowledge*, that no mention is made of the work of Barnard, Max Wolf, von Gothard, and, we may add, Higgs. Dr. Gill's valuable investigations are barely referred to in an incidental way. But with these important exceptions the paper is a useful one, and can be recommended.

A Convenient Sensitometer.—In the *Zeitschrift fur Instrumentenkunde* for June 1894, Professor J. Scheiner, of Potsdam, describes a sensitometer of his own invention, which by its simplicity and suitability for every-day use recommends itself to those engaged in any branch of photographic work. Among the various uses to which a sensitometer can be put are mentioned the determination of the sensitiveness of plates, relation of the half tones to the strongest and weakest tones on different plates, connection between time of exposure and luminous intensity, influence of different developers on the strength of the picture, and the chemical intensity of various light sources. The apparatus consists of a circular disk, with an opening cut in it, through which the light falls upon the plate to be tested. The form of the opening is so chosen that a given distance on any part of the radius corresponds to the same intensity ratio. In front of the sensitive plate is a sheet of metal containing twenty equidistant rectangular openings corresponding in total length with the opening in the disk. Between these openings and the plate is a thin sheet of gelatine containing numbers from 1 to 20 corresponding to the rectangular openings. In order to test the sensitiveness of a plate a strip of the proper size (3 × 9 cm.) is cut and inserted in the carrier, and the disk is

set in rotation by a small hand wheel. Light from a special benzine lamp is then allowed to fall upon the whirling disk for one minute, and on development of the plate the highest number imprinted from the scale gives the sensitiveness of the plate on a scale of 100 by reference to a simple table. The benzine lamp is said to be very constant in brightness, so that reliable absolute determinations can be made.

A convenient form of the apparatus is supplied by Otto Toepfer, of Potsdam and each instrument is tested by Professor Scheiner before it is sent out

A sensitometer in all essential features identical with this was devised by Professor G. W. Hough, of the Dearborn Observatory, many years ago, and has since been employed in his photographic work. We have also used one built on Professor Hough's design at the Kenwood Observatory, and have found it very satisfactory

Mr. Brashear's New Optical Works.—Mr. Brashear is putting up new and large buildings for his optical shops at Allegheny, Pa., the old buildings having become too small for his increasing business The main building, of brick and steel, is 108 by 38 feet in plan, and three stories high, including a high basement The attached boiler room is 16 by 27 feet. In the basement will be done the heavier optical work requiring a uniform temperature, and the testing of surfaces. Other optical work will be done on the floor above, a separate room being devoted to each process, and the third floor will be used as a machine shop The building will be lighted by electricity. A more complete account of this new establishment will be given hereafter.

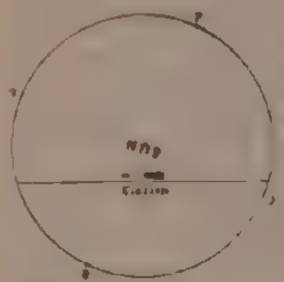
Another Great Telescope?—A paragraph has been going the rounds of the daily press, and has found its way into a number of scientific journals, to the effect that a great refractor of 50 inches aperture is to be made for an Observatory at Pittsburgh. We learn from good authority (Professor Keeler and Mr Brashear) that the report has little foundation. An effort is being made to provide a larger telescope for the Allegheny Observatory, and to remove the Observatory to a better site, and the city has reserved sufficient ground for the latter purpose in the new park, which is well to windward of all the manufacturing establishments. No effort will, however, be made to surpass existing instruments in size; this and all further details mentioned in the paragraph seem to have been invented by an ambitious reporter on a daily newspaper.

An Object-Glass Struck by Lightning.—During a thunder-storm on Mt. Elbert in Colorado last July, the Coast Survey station on the summit was struck by lightning, and among the pieces of apparatus which were damaged was a 2½ inch Brashear object-glass belonging to a portable transit instrument. The flint lens, which in this construction is on the outside, was cracked across. A close inspection of the outside surface shows that it is covered with many small and irregular pits, in some of which are imbedded minute pieces of metal apparently derived from the aluminum cell The crown lens was uninjured A new flint lens has been made for the objective by Brashear, and the old one will be kept by the Coast Survey as a curiosity.

Erratum in the August Number.—In the article by Professor Scheiner on the temperature of the stars, for "spectra which contain four lines," read "spectra which contain few lines" This misprint occurs in two places

CURRENT CELESTIAL PHENOMENA

PLANET NOTES FOR NOVEMBER.



Mercury will be at inferior conjunction Nov. 10 at $12^h\ 34^m$ P. M. central standard time. The declinations of Sun and Mercury differ by only 4′ 53″ so that the planet will be seen projected on the face of the Sun. The transit will last a little over five hours, beginning at $9^h\ 55^m$ A. M. and ending at $3^h\ 12^m$ P. M. central time. For more accurate predictions see the note on "The Transit of Mercury" on another page. The accompanying cut shows the apparent course which Mercury will take across the solar disc.

We hope that most of our readers will have the opportunity to witness this event. The best way for most to observe it will probably be by projecting the Sun's image on a white screen. Such a screen may be made of white cardboard and fastened a foot or more back of the eyepiece of the telescope by means of a wire frame. By proper focusing a very sharp image of the Sun, from six inches to a foot or more in diameter, may be obtained even with a very small telescope or spy-glass.

On the 11th at $10^h\ 21^m$ A. M. Mercury will pass by Venus, only 8′ south of the latter. On the 27th at $10^h\ 58^m$ A. M., Mercury will be at greatest elongation west from the Sun, 20° 10′. He will be at greatest brilliancy as morning planet, Nov. 26.

Venus will be at superior conjunction Nov. 30, at $9^h\ 17^m$ A. M., being then directly behind the Sun. She will not be in good position for observation during the month.

Mars has for some time been the most conspicuous object, save the Moon, in the evening sky. He far out ranks the first magnitude stars in brilliancy, appearing almost to have a disc visible to the naked eye. Having in October passed his point of nearest approach to the Earth, he is still comparatively near and in very favorable position for observation by amateurs. He will be in conjunction with the Moon, 3° south of the latter, Nov. 9 at $12^h\ 56^m$ A. M. On the 22d he will reach the end of the westward loop in his apparent path among the stars and will then begin to move eastward.

Jupiter lights up the eastern half of the sky while Mars does the western. The two planets are nearly equal in brilliancy but quite different in color, the silvery hue of Jupiter contrasting strongly with the ruddy light of Mars. Jupiter is in good position for observation after midnight. He will be in conjunction with the Moon Nov. 16 at $4^h\ 04^m$ A. M.

Saturn and *Uranus* will be behind the Sun during November.

Neptune may be observed all night, the best time being about midnight when the planet is near the meridian. He is in Taurus not far from the star *l*.

Planet Tables for November.

MERCURY

Date.	R. A.	Decl.	Rises	Transits	Sets.
	h m	° ′	h m	h m	h m
Nov. 5	15 26.4	− 20 22	7 46 A. M.	12 26.5 P. M.	5 07 P. M.
15	14 43.1	− 14 10	5 03 "	11 03.9 "	5 04 "
25	14 47.6	− 13 34	5 20 "	10 29.2 "	3 38 "

Date. 1894.	R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m
			VENUS.		
Nov. 5	14 20.6	− 12 54	6 09 A. M.	11 20.8 A. M.	4 32 P. M.
15	15 09.9	− 16 57	6 37 "	11 30.6 "	4 25 "
25	16 01.1	− 20 16	7 04 "	11 42.6 "	4 21 "
			MARS.		
Nov. 5	1 26.6	+ 7 52	3 51 P. M.	10 25.0 P. M.	4 59 A. M.
15	1 20.4	+ 7 54	3 05 "	9 39.6 "	4 14 "
25	1 19.5	+ 8 21	2 23 "	8 50.2 "	3 36 "
			JUPITER.		
Nov. 5	6 26.5	+ 23 00	7 46 P. M.	3 28.0 A. M.	11 11 A. M.
15	6 24.1	+ 23 02	7 04 "	2 46.4 "	10 29 "
25	6 19.9	+ 23 06	6 16 "	1 59.0 "	9 42 "
			SATURN.		
Nov. 5	13 54.3	− 9 19	5 28 A. M.	10 54.5 A. M.	4 21 P. M.
15	13 58.7	− 9 43	4 55 "	10 20.6 "	3 45 "
25	14 03.2	− 10 06	4 22 "	9 45.6 "	3 09 "
			URANUS.		
Nov. 5	14 52.6	− 16 11	7 07 A. M.	12 04.5 P. M.	5 02 P. M.
15	14 54.8	− 16 21	6 19 "	11 16.0 A. M.	4 13 "
25	14 57.4	− 16 32	5 44 "	10 39.6 "	3 36 "
			NEPTUNE.		
Nov. 5	4 56.4	+ 21 07	6 21 P. M.	1 54.3 A. M.	9 27 A. M.
15	4 55.4	+ 21 05	5 41 "	1 13.9 "	8 47 "
25	4 54.3	+ 21 03	5 01 "	12 33.5 "	8 06 "
			THE SUN.		
Nov. 5	14 43.6	− 15 51	6 44 A. M.	11 43.7 A. M.	4 44 P. M.
15	15 24.0	− 18 38	6 57 "	11 44.8 "	4 32 "
25	16 05.9	− 20 52	7 10 "	11 47.3 "	4 25 "

Phases and Aspects of the Moon.

		Central Time. d	h m
Apogee	Nov.	4	4 00 P. M.
First Quarter		5	9 16 A. M.
Full Moon		13	1 40 A. M.
Perigee		16	2 36 P. M.
Last Quarter		19	8 08 P. M.
New Moon		27	2 54 A. M.

Maxima and Minima of Variable Stars.

[From ephemerides by Dr. Loewy in the "Companion to the Observatory," and by Dr. Hartwig in the "Vierteljahrsschrift der Astronomische Gesellschaft."]

MAXIMA.

Nov. 2 T Herculis.
4 R Lyræ.
5 R Ophiuchi.
5 U Geminorum.
6 S Cassiopeæ.
8 U Capricorni.
9 T Ophiuchi.
11 S Piscium.
13 T Capricorni.
21 R Cancri.
21 U Arietis.

MAXIMA CONT.

23 R Capricorni.
26 S Orionis.

MINIMA.

Nov. 2 R Bootis.
3* R Sagittarii.
4 S Sagittarii.
8 V Tauri.
11 S Vulpeculæ.
12 R Ceti.
13 W Scorpii.
14 R Tauri.

MINIMA CONT.

15 L² Puppis.
18 R Leonis.
19 U Cygni.
20 S Tauri.
20 V Geminorum.
23 X Libræ.
26 S Coronæ.
27 S Ursæ Majoris.
27 U Monocerotis.
28 S Leonis.
29* V Bootis.
30 S Cancri.
30 R Scuti.

New Variable of the Algol Type.—Dr. Krueger announces D. M. 13.3115 to be an Algol variable, discovered by Dr. Hartwig. Epoch, September 10.35 Gr. M. T., period 2.00 days, minimum on September 16.37.

JOHN RICHTIE, JR.

September 15, 1894.

* The "Vierteljahrsschrift" gives this as a maximum.

Occultations Visible at Washington.

Date 1894		Star's Name.	Magnitude.	IMMERSION Washington M. T. h	m	Angle f'm N p't. °	EMERSION Washington M. T. h	m	Angle f't N p't. °	Duration. h	m
Nov.	3	ω Sagittarii	5	9	11	134	9	35	179	0	24
	7	b^{3} Aquarii	7	10	44	85	11	43	201	0	59
	7	b^{1} Aquarii	8	11	42	87	12	38	204	0	56

Minima of Variable Stars of the Algol Type.

(Given to the nearest hour in Central Standard Time.)

U CEPHEI.

Alternate Minima.

		h
Nov.	2	2 P. M.
	7	1 "
	12	1 "
	17	1 "
	22	12 M.
	27	12 "

ALGOL.

Alternate Minima.

Nov.	2	1 A. M.
	7	6 P. M.
	13	12 M.
	19	5 A. M.
	24	11 P. M.
	30	5 "

λ TAURI.

Alternate Minima.

Nov.	5	4 P. M.
	13	2 "

λ TAURI CONT.

Alternate Minima.

	h
21	11 A. M.
29	9 "

R. CANIS MAJORIS.

Every third minimum.

Nov.	3	10 A. M.
	6	8 P. M.
	10	6 A. M.
	13	3 P. M.
	17	1 A. M.
	20	11 A. M.
	23	9 P. M.
	27	7 A. M.
	30	4 P. M.

S. CANCRI.

Nov.	8	4 A. M.
	17	3 P. M.
	27	3 A. M.

S. ANTLIÆ.

Every tenth minimum.

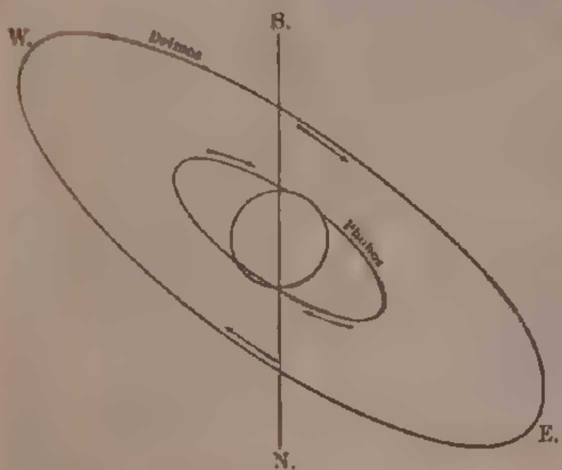
		h
Nov.	2	2 A. M.
	5	8 A. M.
	8	2 P. M.
	11	8 P. M.
	15	2 A. M.
	18	7 "
	21	1 P. M.
	24	7 P. M.
	28	1 A. M.

Y CYGNI.

Every fourth minimum.

Nov.	3	7 A. M.
	9	7 "
	15	7 "
	21	7 "
	27	7 "

The Satellites of Mars.



DEIMOS.

		h		
Nov.	1	4.5	A. M.	W.
	3	1.9	"	E.
	4	11.3	P. M.	W.
	6	8.7	"	E.
	8	6.2	"	W.
	10	3.6	"	E.
	12	1.0	"	W.
	14	10.4	A. M.	E.
	16	7.8	"	W.
	18	5.1	"	E.

PHOBOS.

Nov.	1	3.2	A. M.	W.
	2	6.0	"	E.
	3	8.8	"	W.
	4	11.6	"	E.
	5	2.4	P. M.	W.
	6	5.1	"	E.
	7	7.9	"	W.
	8	10.7	"	E.
	10	1.5	A. M.	W.
	11	4.3	"	E.
	12	7.1	"	W.

For Phobos the central time of every seventh eastern and western elongation is given, and for Deimos every third, the intermediate ones may be found by adding the periodic time of each satellite. Periodic time of Phobos 7h 39m.2. Periodic time of Deimos 1d 6h 17m.9

The Transit of Mercury Nov. 10, 1894.—The most important astronomical event predicted for this year is the transit of Mercury across the Sun's disc which will take place Nov. 10. This phenomenon occurs only about once in seven years, on the average, so that not many opportunities to observe it come in a life time. The amateur as well as the professional astronomer will be expected to make the most of this opportunity.

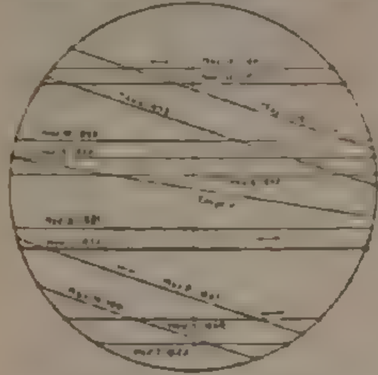


Diagram showing the Paths of Mercury across the Sun's Disc during the Transits of this Century.

This transit will be visible in Western Europe and Africa, the Atlantic Ocean, North and South America, and the Pacific Ocean. The Sun will be most favorably situated for observation in Central and South America, but the conditions will also be good in the United States. The planet will enter upon the disc of the Sun near the East point, 98° around the limb from the North point, at $9^h\ 55^m\ 18^s$ A. M., Central Standard time, as seen from Northfield, and will take a northwesterly course, leaving the disc at a point 50° west from the north point, at $3^h\ 12^m\ 15^s$ P. M.

The planet will appear as a round very *black* spot, distinguishable from ordinary sunspots by its color, its roundness and its motion. The observer should watch carefully to see if, as it enters upon and leaves the disc of the Sun the planet is encircled by a ring of light, and if, when fully on the disc, it is surrounded by a narrow dusky fringe. These if seen would be evidences of an extensive atmosphere upon the planet.

ELEMENTS OF THE TRANSIT.

Greenwich mean time of conjunction in R. A. Nov. 10, $6^h\ 51^m\ 16^s.3$.

Sun and Mercury's R. A.	$15^h\ 03^m\ 44^s.68$	Hourly motion $+10^s.12$ and $-12^s.46$	
Sun's declination	17° 18′ 58″.28 S.	Hourly motion	0′ 41″.88
Mercury's declination	17 14 05 .28 S.	Hourly motion	1 45 .28
Sun's equa. hor. par.	8 .94	True semidiameter	16 09 .83
Mercury's equa. hor. par.	13 .08	True semidiameter	4 .94

GREENWICH MEAN TIMES OF THE PHASES.

Ingress, exterior contact	November 10, 3^h	55^m	$31^s.2$
Ingress, interior contact	3	57	15.4
Least distance of centres 4′ 26″.8	6	33	48.5
Egress, interior contact	9	10	26.4
Egress, exterior contact	9	12	10.4

The Greenwich mean time of exterior contacts, for any point on the Earth's surface, may be computed from the following formulæ, in which ρ denotes the radius of the Earth at that place, φ' the geocentric north latitude and λ the longitude west from Greenwich.

$$\text{Ingress } T_1 = 3^h\ 55^m\ 31^s.2 + [0.7793]^s\ \rho \sin \varphi' - [1.6452]^s\ \rho \cos \varphi' \cos (329°\ 28'\ 15'' - \lambda)$$

$$\text{Egress } T_2 = 9^h\ 12^m\ 10^s.4 + [1.4333]^s\ \rho \sin \varphi' - [1.5339]^s\ \rho \cos \varphi' \cos (218°\ 51'\ 07'' - \lambda)$$

In the following table we give the Greenwich times of beginning and ending of the transit calculated for the several observatories from the American Nautical Almanac data.

Observatory.	Longitude.		Latitude.		Transit begins			Transit ends.		
	°	′	°	′	h	m	s	h	m	s
Harvard	71	07.7	+42	23	3	55	29 P. M.	9	12	07 P. M.
Washington	77	03.0	+38	54	3	55	24 "	9	12	06 "
Goodsell	93	09.0	+44	28	3	55	18 "	9	12	15 "
Chamberlin	104	51.9	+39	41	3	55	11 "	9	12	17 "
Lick	121	38.5	+37	20	3	55	04 "	9	12	23 "

It will be seen that the times vary only in the seconds throughout the whole United States. In order to get the Standard times we have only to subtract 5 hours for eastern, 6 hours for central, 7 hours for mountain and 8 hours for Pacific time.

Mr. Thos Lindsay, assistant secretary of the Astronomical and Physical Society of Toronto sends us the following times of the contacts at the beginning of the transit at Northfield.

Exterior contact $9^h\ 55^m\ 50^s.0$ A. M. } Central time
Interior contact $9^h\ 57^m\ 28^s.4$ " }

These he computed from the data given in the British Nautical Almanac, considering the transit as an occultation of the Sun by Mercury

The difference between his result and that given above is chiefly due to the difference in time of conjunction in right ascension as given in the two almanacs. The American gives the Greenwich time of conjunction as $6^h\ 54^m\ 16^s.3$ while the British almanac gives it as $6^h\ 54^m\ 47^s.6$.

Observations of the Partial Eclipse of the Moon, September 14th, 1894.—This eclipse was observed with the 12-inch and its finder and photographed with the 12-inch and the Willard lens.

The shadow seemed to be lighter in this eclipse than in others I have seen. In the 12-inch itself it was simply a pale, ashy, dusty shade, with scarcely any boundary line, though in the finder the outline of the shadow was quite marked. The limb of the Moon and details on its surface were seen while in the shadow—the limb being more conspicuous than at previous eclipses I have seen at a similar stage.

A sharp lookout was kept for the contact with the shadow At $7^h\ 38^m.7$ I decided that the limb was certainly in the shadow. Looking in the finder it was seen that contact was certainly past, as the shadow at this time had made quite an advance on the limb.

The following observations were obtained and will be quite accurate as they were made with the finder and the outline of the shadow was well marked.

Contact with the first portion of Sinus Iridum, $7^h\ 51^m\ 50^s$.

Bisecting Plato, $7^h\ 52^m\ 40^s$.

Bisecting Plato, $7^h\ 01^m\ 10^s$.

The last contact was observed at $9^h\ 25^m\ 7$, though a decided shade was present at that point of the limb for several minutes later. The actual contact was, I have no doubt, later than this.

Five exposures were made with the 12 inch with aperture reduced to 4 inches. These are fairly satisfactory considering the subject. Nine exposures were made with the Willard lens on three plates, a Carbutt transparency, an Eastman transparency and a Cramer "crown" plate (very rapid plate). These are satisfactory and show the phase far better than the 12-inch—just, indeed, as the naked eye shows the outline of the shadow better than the telescope does.

In this eclipse the shadow was a dark, cold gray (in finder) with no suggestion of warmth at any stage. I think it was lighter than usual.

The outline of the shadow at contacts was too indefinite to make even an approximate guess of contact times of any value—especially with such a large instrument as the 12-inch.

The recorded times are Standard Pacific—$8^h\ 0^m\ 0^s$ slow of Greenwich.

E. E. BARNARD

Mt. Hamilton, Sept 15, 1894.

COMET NOTES.

Correction to the Photograph of Gale's Comet in Astronomy and Astro-Physics for August.—In making the half tone from the glass contact positive I sent of the photograph of Gale's Comet on May 5th, the positive has unfortunately got reversed, as will be seen by comparison with that of May 3 in ASTRONOMY AND ASTRO-PHYSICS for June.

As this may cause confusion it will be well to correct it.

The one for May 3 is correctly printed—below is west, to the right is north. The engraver having used the wrong side of the plate in that of May 5, the north and south are interchanged. In this picture the west is at the bottom but north is to the left. This is to be regretted and the orientation should be corrected on the prints.

E. E. BARNARD

New Elements of Comet Tempel II, 1894, III (1873, II.)—In *Astronomische Nachrichten*, No. 3246, Mr. Schulhof gives new elements of this periodic comet corrected by means of the early observations made this year. He says that it is desirable that the comet be observed as long as possible by observers having the use of powerful telescopes.

ELEMENTS.

Epoch, 1894, June + 0, Paris M. T.

$$\begin{aligned} M &= 7^\circ\ 53'\ 09''.5 \\ \pi &= 306\ \ 15\ \ 00\ .3 \\ \Omega &= 121\ \ 10\ \ 05\ .5 \\ i &= 12\ \ 44\ \ 21\ .9 \\ \varphi &= 33\ \ 26\ \ 27\ \ 4 \\ \mu &= 679''.9391 \\ \log a &= 0.478358 \end{aligned} \quad \left.\vphantom{\begin{matrix}a\\b\\c\\d\end{matrix}}\right\} 1894.0$$

NEWS AND NOTES.

We believe our readers will pardon the delay of this number when they see its contents. The cheif cause of it was the fact that the copy for some plates came to hand late. We do not blame anyone for there was good reason for the delay although the time left to us was too short for prompt publication. Will contributors please bear in mind that *all* matter should be in hand before the 15th of the month preceding the one for which the publication is dated.

This is manifestly a Mars' number, and the articles and colored plates contributed to it show earnest work that already compares very well with that of the last favorable opposition. We will soon give more articles on the study of Mars now going on in this country, which we believe will still add to the interest of our readers from the news we have already received from prominent observers who have not yet reported their observations.

Reorganization of the U. S. Naval Observatory.—On Sept. 21, H. A. Herbert, Secretary of the Navy, wrote to Professor William Harkness, saying substantially that he had appointed him Astronomical Director of the Naval Observatory, to be in charge of, and responsible for the direction, scope, quantity and preparation for publication of all work purely astronomical to be performed at the Observatory. The Secretary admits that the criticisms of astronomers in regard to the lack of system in astronomical work at the Observatory has some foundation, but that all other charges are groundless.

The new regulations under which this appointment is made provide that the Observatory, under the control of the Secretary of the Navy, is subject to the direct supervision of the bureau of equipment, that a naval officer is assigned as superintendent and that the Observatory work is divided in two branches, astronomical and nautical. The first of these includes the department of astronomical observations, and the department of the nautical almanac. The second includes the departments of nautical instruments, of chronometers, and time service and of magnetism and meteorology. The superintendent, as commanding officer, is charged with the general superintendence and government of the Observatory.

Duties of the Astronomical Director.—All officers, assistant astronomers, computers and employes are subject to his orders and he reports the operations of the Observatory annually. The astronomical director has charge of and is responsible for the direction, scope, character, quantity and preparation for publication of all work purely astronomical which is performed at the Observatory. He has charge of the 26-inch and 12 inch equatorial telescopes, the 6-inch and 9-inch transit circles and in fact all the astronomical instruments. He is to personally inspect, both day and night the methods of observation and is to present to the department on the last days of June and December of each year reports upon the qualities of his subordinates, their aptitude, efficiency, zeal, punctuality, health, and deportment.

He is not to enter into any agreement with other Observatories for the performance of work which will occupy the time of any observer more than one month without the sanction of the department.

The director of the Nautical Almanac is held directly responsible for all that pertains to that publication.

Nautical Instruments and Time.—The head of the department of nautical instruments is to see that all the instruments issued except chronometers are thoroughly inspected, that a record shall be kept of each instrument, and is held responsible for the safe keeping of instruments.

The head of the chronometer and time service department is to make all the determinations of local time that are necessary, will transmit the daily time signals and care for all the time and chronometer work of the Observatory. The head of the magnetism and meteorology department is to keep up a continuous series of observations and report any unusual disturbances, their probable cause and relation to visible phenomena.

An annual report of the operations in the various departments for the preceding fiscal year should be submitted to the superintendent July 15.

The regulations for the supervision of the library, the buildings and grounds those governing the instrument maker, electrician, watchmen, fire organization and laborers are all placed under the control of the superintendent.

Nautical Almanac Error in Diagram of Mars' Satellite Orbits.—Professor William H. Pickering, Lowell Observatory, Flagstaff, Arizona, calls our attention to a slight error in the American Ephemeris regarding the orbits of the satellites of

Mars as found on page 456 of the Ephemeris for 1894. The arrows sho turned in the opposite direction and the orbit of Phobos should pass in fr the bottom, instead of in front of the top of the disc. This error was detec Mr. A E. Douglass of Lowell Observatory, September 14th, who noticed he took to be two faint stars near the planet. He soon saw that they were ing, but not being at elongation they did not agree in position with com places of the satellites. A little investigation soon showed where the erro He easily followed Phobos to within less than a radius of the planet A g at the diagram in the Ephemeris will show the mistake, for, since the sat revolve nearly in the plane of the planet's equator, and the south pole is t towards us, the southern side of the orbit of Phobos must clearly pass o further side of the planet's south pole Since the motion of the satellite is the arrows must certainly point in the opposite direction

We had not noticed this error We have had a new cut made showin orbits of the satellites correctly in these two particulars. It will be found where in this number

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The Chicago Academy of Sciences.—*Section of Astronomy and Mathem Sept. 4th.*—The regular monthly meeting was held in the Commerce Club, torium Building, Sept 4th, Professor G W Hough, president, in the chair

Professor Geo E Hale, who has just returned from an extensive trip ab read the first paper of the evening on "*Some European Observatories an Work they are doing*" Professor Hale gave an interesting and rathe account of his visit to the various Astro-Physical Observatories in Eng France, Germany, Austria and Italy. The speaker seemed much pl with his visit to Mr Higgs in Liverpool, Dr. Huggins and Mr Ranyard in don, Professor Vogel in Potsdam and Professor Tacchini in Rome He de the scientific and social events attending his visit to the different observat and spoke in terms of warm appreciation of the courtesies and hospit shown him by all the European astro-physicists

During his stay in Berlin, he had been engaged upon some researches Physical Institute on the effect of ultra violet light in discharging a sodium in vacuo. He was greatly interested in the work he found in progress Potsdam Observatory, and said that Professor Vogel had shown a deep in in the plans of the Yerkes Observatory, which were then being revised an fected From Berlin Professor Hale went to Vienna and after visiting th perial and von Kuffner Observatories continued his journey to Rome, several weeks were spent with Professor Tacchini at the Roman College Ob tory

The speaker then gave an account of his trip to Mt Ætna with Pro Riccò, undertaken with the hope of photographing the corona. The experi were unsuccessful owing to the smoke of the volcano, and the haze of the sphere, and he left the apparatus in charge of Professor Riccò, and return Paris and London.

The speaker seemed well pleased with his interesting journey, and said many new lines of research had been suggested by the work he found in pro abroad

In conclusion, Professor Hale thought that while the European countri generally in advance of America in most branches of pure science this wa true in Astro-Physics, as the great appliances of the Americans had given the lead The paper was illustrated by lantern projections and was high preciated by the members of the Academy.

Dr. See presented the second paper on "*The Locus of the Centre of Gr for a Homogeneous Ellipsoid of Revolution,*" an investigation to which been led by his lectures at the University of Chicago on the Attraction Figures of the Heavenly Bodies The work is supplementary to the inve tions of Herschel and Airy, and seemed to throw a very clear light upo theory of the attraction of a homogeneous planet. On motion of Professo it was voted to incorporate the section of Physics, so that this section Academy of Sciences will hereafter be known as the Section of Mathem Astronomy and Physics.

After some discussion the meeting adjourned

PLATE XXXI.

DENVER EQUATORIAL, 20 INCHES APERTURE.

MOUNTED BY G. N. SAEGMULLER, WASHINGTON, D. C.

ASTRONOMY AND ASTRO-PHYSICS, No. 129.

Astronomy and Astro-Physics.

VOL. XIII, No. 9. NOVEMBER, 1894. WHOLE No. 129.

General Astronomy.

THE 20-INCH EQUATORIAL OF THE CHAMBERLIN OBSERVATORY.*

H. A. HOWE, DIRECTOR.

The work of mounting the twenty-inch equatorial of the Chamberlin Observatory was begun in July, and the instrument is now in fair shape for use. The fact that the writer was able to get the instrument together without mistakes, and without the help of any skilled mechanics, speaks well for the care which was exercised in fitting and marking every piece in the shop. As this is the first large mounting of Mr. Saegmuller's construction, which has been set up in this country, astronomers will be interested to know about its peculiarities, together with the excellencies and the faults (if any) of its construction. First, however, for the object-glass:

THE OBJECTIVE.—The discs for this were obtained of Feil and were figured by Alvan G. Clark. They are well-nigh perfect specimens of optical glass: the crown lens is free from striæ, and the writer could find only three or four small ones in the flint lens. No polarization was shown by the ordinary test by reflected light, using a Nicol's prism. There is but one noteworthy bubble which is a millimeter and a half in diameter. The color-correction is better than the writer expected with so large a glass of the usual type, and the defining power is exquisite.

GENERAL DESCRIPTION OF THE MOUNTING.—The pier on which the instrument stands is built of a tough sandstone, being faced with dimension-stone, and filled with heavy rubble work. Its foundation is grout. The pier is 16 ft. square at the base, 12 ft. square at the top, and 25 ft. high, its base being 12 ft. below the surface. Into this pier are let three steel bolts, 9 ft. long and 3 inches in diameter. Their heads lock into horizontal foot-plates 2 ft. square, imbedded 7 ft. deep in the masonry. On top of the pier lie three similar plates, through which the bolts run, and to which they are held by very heavy nuts. On these bolts is sup-

* Communicated by the author.

ported the 7000 lb. casting (shown in cut), which formed the lowest section of the pillar, its top being nearly flush with the floor of the dome room. The adjustment of the instrument in latitude is made by lifting the entire column by means of the adjusting nuts on the north bolt. Upon this massive tripod stands a bell-shaped casting 5 ft in diameter at the base and about 5 ft. high, to which is fastened by bolts running through internal flanges a second casting, on which in turn stands the square clock case. The adjustment in azimuth is beneath the floor, the bell-shaped casting being rotated by three pairs of opposing adjusting screws. The clock box can be shifted in azimuth (without adjusting screws) and is held in place by four set screws inside the pillar. The headstock is of a peculiar form, and projects far to the south of the pillar, so that the centre of gravity of everything above the clock-case is well in toward the geometrical axis of the pillar. The weight of the entire instrument is 25,000 lbs.

ANTI-FRICTION DEVICES.—The polar axis is a fine piece of Midvale steel resting in phosphor bronze bearings. The friction at the upper bearing is relieved by a set of six hardened steel rollers, each a foot in diameter and a quarter of an inch thick, which stand vertically side by side on the same axis. This axis is supported in an anti-friction bearing composed of small hardened steel cylinders. The system of rollers is nearly under the center of gravity of the moving portion of the instrument, and is pressed upward by a powerful bar spring inside of the headstock. Any desired tension is put upon the spring by means of a worm-gear, and the polar axis may thus be lifted entirely off its upper bearing.

The comparatively slight tendency of the lower end of the polar axis to rise is counteracted by a friction roller placed above it. The end thrust of this axis is small and is taken by a ball bearing at the lower end of it.

The declination axis runs in plain bearings, but the end thrust is taken by a ball-bearing at each extremity of the axis. The ball-bearing at the small end of the axis is adjustable and firmly secured by a set-screw. A practical advantage of having plain bearings on the declination axis is that when the instrument is near the meridian, so that there is very little pressure on the ball-bearings, the friction is sufficient to keep the instrument from rotating when the micrometer is put on or taken off. Thus no manipulation of the counterpoises is necessary. The large screw on which the counterpoises for declination are strung is not a continuation of the axis, but of the sleeve.

Driving Clock.—The driving clock has a Young's double conical pendulum, the friction shoes of which are shod with vegetable fibre. The vertical spindle which carries the pendulum, carries also near its lower end a horizontal wheel, on the lower face of which are set two diametrically opposite armatures, which revolve over opposite pairs of helices, for electric control. The pendulum makes two revolutions in a sidereal second, and the helices are supposed to quicken or to retard its motion, as may be necessary. The clock train carries a chronograph which may be used either for regulating it, or for ordinary noting of time. The clock may be started, stopped, or wound from the floor, and runs so admirably that an electric control seems almost a superfluity. An electric motor for winding the clock is in contemplation. In winter heated air from a room below rises inside the pillar, and keeps the clock warm by day and by night.

Main Circles.—Each vernier of the hour circle is read from the floor by a reading telescope near the dial box on the south face of the pillar; the smallest reading is one second, but half a second may easily be estimated. The verniers of the declination circle are read from the eye-end by two telescopes, the smallest reading being 5 seconds of arc. The divisions on both circles are exceedingly satisfactory in point of sharpness and distinctness.

Setting Circles.—The observer, when on the floor, sets the instrument to any desired right ascension and declination by turning the hand-wheels on the south side of the pillar, and reading the two dials contained in the large cylindrical box, which is above them, on a level with the eye. Each dial hand moves at double the angular speed of the corresponding axis. The declination dial is figured from 0° to 90° each way, the smallest space being 1°. The right ascension dial has five-minute spaces, and is driven by an eight-day clock. Notwithstanding the large number of gears involved in driving this mechanism, the total back lash is so small that a star of known coördinates is brought near the center of the finder at once.

It is important to have another system of setting circles visible from the eye end when the observer is on the north side of the pillar. This system consists of a 4 ft. circle on the declination sleeve which is read by the naked eye of the observer at the eyepiece to the nearest quarter of a degree with entire ease, and a 3 ft. circle on the north side of the clock box, which is similarly read to the nearest minute of hour-angle.

Illumination.—As the entire building is lighted by an alternating current at a pressure of 50 volts, this current has been uti-

lized for the two-candle power lamps, which illuminate the verniers of the main declination circle, the large setting circles, the micrometer, the hand lamp, etc. The main hour circle is lit up by two lamps of 16-candle power each, which are so placed that they light up the dials south of the pier as well. As the voltage of the house current is too high for the small lamps, it is run through a special converter made by Mr. E. G. Richardson, of Denver, and presented by him to the Observatory. The converter carries a switch by which the voltage of the secondary current is made to suit the small lamps, which are arranged in parallel, and are thus as easy to control as the house lamps. The light is steady, and there are no batteries to require attention. This method of illumination is so eminently satisfactory that it is urgently recommended to all Observatories which have access to an alternating current.

The converter is inside of the pillar, about 4 feet above the floor, and is reached through a large opening on the north side of the pillar. The secondary wires run up from the converter through the clock case, thence out of a hole in the nose of the headstock, up through the polar axis (which is hollow) into the declination sleeve, emerging at the inner end of the sleeve, and being there attached to a series of concentric rings. Springs fastened to the telescope tube, and pressing against these rings, lead the current to all required points on the tube. All switches are placed just where they ought to be, and the writer expects great satisfaction from the completeness and easy manipulation of the electric lighting.

THE EYE-END.—To the end of the main sheet steel portion of the tube is attached a short cylindrical casting upon which rotates a spectroscope jacket, similar to that on the Lick telescope. In order to adapt the tube to photography, the entire eye end has been made to slide upwards a distance of about 3 feet, being guided by four steel rods, which run in eight guiding lugs within the casting which supports the spectroscope jacket. For visual work this sliding piece is pulled out as far as possible, and for photography it is thrust clear home, so that the photographic focus lies outside of it. In either position the sliding rods are held by clamp screws. At the lower end of the sliding piece is attached by a bayonet joint the tail-piece proper, consisting of the focussing tubes. The tail-piece has lateral adjusting screws, so that the sight line may be made perpendicular to the declination axis. There is but one finder, of five inches aperture.

MICROMETER.—This attachment varies in some particulars

from the ordinary American form. The verniers and the pinion for rotation in position angle are fixed, while the position circle revolves. Thus the observer can always find the pinion and the verniers, without loss of time. The circle which is 9 inches in diameter is divided to each tenth of a degree, and can be read by the verniers to hundredths if desired. Parallel to the movable micrometer wire is a system of wires, spaced at distances of 5 minutes of arc, for facilitating observations of comets or asteroids. There is but one fault to be found with the micrometer, namely that in certain positions the ends of the box are over the verniers, making them inconvenient to read. It is only just to the maker to state that he has promised to remedy this defect, together with any others which the observer may discover, after using it awhile.

ADAPTATION TO PHOTOGRAPHY.—The crown lens of the objective is reversible, the two lenses being then separated by several inches. To accomplish this the telescope is pointed to the nadir, and the lower end of the tube is fastened to the pillar by a simple device. A reversing carriage is then run under the objective, so that the crown cell, which weighs about 150 lbs. is safely taken off, turned over and put back. When one wishes to photograph, the 5-inch finder and the entire system of handles for clamping and executing slow motions in right ascension and declination are slid up the side of the tube so as to be out of the way of the photographer though still usable. The tail-piece is then removed, and the plate-holder attached in its place by a bayonet joint. The plate-holder is movable in both right ascension and declination by five screws, the back lash being controlled by powerful springs.

For "following" there is a photographic finder, the objective of which is 5 inches in aperture, and is mounted on the outside of the main telescope, close by the 20-inch glass. The eye-end of the photographic finder is a small micrometer, which is attached to the plate-holder by a sliding mechanism, which allows the micrometer to be moved quite a distance in right ascension or declination or both, till a star suitable for "following" is found, and placed at the intersection of the spider webs. It is hoped that the displacement of the image on the photographic plate by changes of refraction, differential flexure of the tube, etc., will be practically the same as the displacement of the star which is used for "following." If there is any twisting of the plate about the line of collimation as an axis, it may be possible to detect it by turning the position-circle of the micrometer so that one of the

spider-webs shall bisect two stars at opposite sides of the field of view. No mechanism has been provided for correcting such a twist, but should the twist be discovered, it will be easy to attach the plate-holder to the rotating spectroscope-jacket, the motion of which is controlled by a worm.

RIGIDITY.—As it is well known that Mr. Saegmuller strives to build his instruments as light as is consistent with proper strength, some astronomers have feared that a large telescope of his construction might lack rigidity. This mounting is not open to such a charge, and must be considered as reflecting great credit upon its maker.

THE OBSERVING CHAIR.—This is 13 ft. high, 6½ ft. wide and 9 ft. deep. The platform (4 ft. by 3) slides up and down on four heavy trunk rollers, and is supported by a three-quarter inch Manila rope which takes a turn and a half around a six-inch oak drum; it is so counterbalanced as to require only the pressure of one finger to raise it. If two or three heavy persons are to be on the platform at once, an extra turn of the rope around the drum gives security against sliding downward. The chair is mounted on four of Martin's truck castors, which are equipped with anti-friction wheels, so that they rotate about their vertical spindles easily. A ring of iron concentric with the top of the wall of the room, and 3 ft. less in diameter keeps the chair from running against the electric lights, etc., on the wall. The chair works very satisfactorily.

UNIVERSITY PARK, Colo.

THE PLANET MARS*

GIOVANNI SCHIAPARELLI

As our chart demonstrates, in its general topography Mars does not present any analogy with the Earth. A third of its surface is occupied by the great *Mare Australe*, which is strewn with many islands, and the continents are cut up by gulfs and ramifications of various forms. To the general water system belongs an entire series of small internal seas, of which the Hadriacum and the Tyrrhenum communicate with it by wide mouths, whilst the Cimmerium, the Sirenum and the Solis Lacus are connected with it only by means of narrow canals. We shall notice in the first four a parallel arrangement, which certainly is

* Continued from page 640

not accidental, as also not without reason is the corresponding position of the peninsulas of Ausonia, Hesperia, and Atlantis. The color of the seas of Mars is generally brown, mixed with grey, but not always of equal intensity in all places, nor is it the same in the same place at all times. From an absolute black it may descend to a light grey, or to an ash color. Such a diversity of colors may have its origin in various causes, and is not without analogy also upon the Earth, where it is noted that the seas of the warm zone are usually much darker than those nearer the pole. The water of the Baltic, for example, has a light, muddy color, that is not observed in the Mediterranean. And thus in the seas of Mars, we see the color become darker when the Sun approaches their zenith, and summer begins to rule in that region.

All of the remainder of the planet, as far as the north pole, is occupied by the mass of the continents, in which, save in a few areas of relatively small extent, an orange color predominates, which sometimes reaches a dark red tint, and in others descends to yellow and white. The variety in this coloring is in part of meteorological origin, in part it may depend on the diverse nature of the soil, but upon its real cause it is not as yet possible to frame any very well grounded hypothesis. Nevertheless, the cause of this predominance of the red and yellow tints upon the surface of ancient Pyrois is well known.* Some have thought to attribute this coloring to the atmosphere of Mars, through which the surface of the planet might be seen colored, as any terrestrial object becomes red, when seen through red glass. But many facts are opposed to this idea, among others, that the polar snows appear always of the purest white, although the rays of light derived from them traverse twice the atmosphere of Mars under great obliquity. We must then conclude that the arean continents appear red and yellow, because they are so in fact.

Besides these dark and light regions, which we have described as seas and continents, and of whose nature there is at present scarcely left any room for doubt, some others exist, truly of small extent, of an amphibious nature, which sometimes appear yellowish like the continents, and are sometimes clothed in brown, (even black in certain cases) and assume the appearance of seas, whilst in other cases their color is intermediate in tint, and leaves us in doubt to which class of regions they may belong. Thus all the islands scattered through the Mare Australe and the Mare

* Pyrois I take to be some terrestrial region, although I have not been able to find any translation of the name.—*Tr.*

Erythraeum belong to this category, so too the long peninsula called Deucalionis Regio and Pyrrhae Regio, and in the vicinity of the Mare Acidalium the regions designated by the names of Baltia and Nerigos. The most natural idea, and the one to which we should be led by analogy, is to suppose these regions to represent huge swamps, in which the variation in depth of the water produces the diversity of colors. Yellow would predominate in those parts where the depth of the liquid layer was reduced to little or nothing, and brown, more or less dark, in these places where the water was sufficiently deep to absorb more light, and to render the botton more or less invisible. That the water of the sea, or any other deep and transparent water, seen from above, appears more dark the greater the depth of the liquid stratum, and that the land in comparison with it appears bright under the solar illumination, is known and confirmed by certain physical reasons. The traveller in the Alps often has occasion to convince himself of it, seeing from the summits, the deep lakes with which the region is strewn, extending under his feet as black as ink, whilst in contrast with them even the blackest rocks illumined by the sunlight appeared brilliant.*

Not without reason then have we hitherto attributed to the dark spots of Mars the part of seas, and that of continents to the reddish areas which occupy nearly two-thirds of all the planet, and we shall find later, other reasons which confirm this method of reasoning. The continents form in the northern hemisphere a nearly continuous mass, the only important exception being the great lake called the Mare Acidalium, of which the extent may vary according to the time, and which is connected in some way with the inundations which we have said were produced by the melting of the snow surrounding the north pole. To the system of the Mare Acidalium undoubtedly belong the temporary lake called Lacus Hyperboreus and the Lacus Niliacus. This last is ordinarily separated from the Mare Acidalium by means of an isthmus or regular dam, of which the continuity was only seen to be broken once for a short time in 1888. Other smaller dark spots are found here and there in the continental area, which we may designate as lakes, but they are certainly not permanent lakes like ours, but are variable in appearance and size according to the seasons, to the point of wholly disappearing under certain

* This observation of the dark color which deep water exhibits when seen from above, is [illegible]

circumstances. Ismenius Lacus, Lunæ Lacus, Trivium Charontis and Propontis are the most conspicuous and durable ones. There are also smaller ones, such as Lacus Moeris and Fons Juventae which at their maximum size do not exceed 100 to 150 kilometers (60 to 90 miles) in diameter, and are among the most difficult objects upon the planet.

All the vast extent of the continents is furrowed upon every side by a network of numerous lines or fine stripes of a more or less pronounced dark color, whose aspect is very variable. These traverse the planet for long distances in regular lines, that do not at all resemble the winding courses of our streams. Some of the shorter ones do not reach 500 kilometers (300 miles), others on the other hand extend for many thousands, occupying a quarter or sometimes even a third of a circumference of the planet. Some of these are very easy to see, especially that one which is near the extreme left-hand limit of our map, and is designated by the name of Nilosyrtis. Others in turn are extremely difficult, and resemble the finest thread of spider's web drawn across the disc. They are subject also to great variations in their breadth, which may reach 200 or even 300 kilometers (120 to 180 miles) for the Nilosyrtis, whilst some are scarcely 30 kilometers (18 miles) broad.

These lines or stripes are the famous canals of Mars, of which so much has been said. As far as we have been able to observe them hitherto, they are certainly fixed configurations upon the planet. The Nilosyrtis has been seen in that place for nearly one hundred years, and some of the others for at least thirty years. Their length and arrangement are constant, or vary only between very narrow limits. Each of them always begins and ends between the same regions. But their appearance and their degree of visibility vary greatly, for all of them, from one opposition to another, and even from one week to another, and these variations do not take place simultaneously and according to the same laws for all but in most cases happen apparently capriciously, or at least according to laws not sufficiently simple for us to be able to unravel. Often one or more become indistinct, or even wholly invisible, whilst others in their vicinity increase to the point of becoming conspicuous even in telescopes of moderate power. The first of our maps shows all those that have been seen in a long series of observations. This does not at all correspond to the

furrows or depressions in the surface of the planet, destined for the passage of the liquid mass, and constituting for it a true hydrographic system, is demonstrated by the phenomena which are observed during the melting of the northern snows. We have already remarked that at the time of melting they appeared surrounded by a dark zone, forming a species of temporary sea. At that time the canals of the surrounding region become blacker and wider, increasing to the point of converting, at a certain time, all of the yellow region comprised between the edge of the snow and the parallel of 60° north latitude, into numerous islands of small extent. Such a state of things does not cease, until the snow, reduced to its minimum area, ceases to melt. Then the breadth of the canals diminishes, the temporary sea disappears, and the yellow region again returns to its former area. The different phases of these vast phenomena are renewed at each return of the seasons, and we have been able to observe them in all their particulars very easily during the oppositions of 1882, 1884 and 1886, when the planet presented its northern pole to terrestrial spectators. The most natural and the most simple interpretation is that to which we have referred, of a great inundation produced by the melting of the snows,—it is entirely logical, and is sustained by evident analogy with terrestrial phenomena. We conclude therefore that the canals are such in fact, and not only in name. The network formed by these was probably determined in its origin in the geological state of the planet, and has come to be slowly elaborated in the course of centuries. It is not necessary to suppose them the work of intelligent beings, and notwithstanding the almost geometrical appearance of all of their system, we are now inclined to believe them to be produced by the evolution of the planet, just as on the Earth we have the English Channel and the Chanel of Mozambique.

It would be a problem not less curious than complicated and difficult, to study the system of this immense stream of water, upon which perhaps depends principally the organic life upon the planet, if organic life is found there. The variations of their appearance demonstrated that this system is not constant. When they become displaced, or their outlines become doubtful and ill defined, it is fair to suppose that the water is getting low, or is even entirely dried up. Then in place of the canal there remains either nothing, or at most a stripe of yellowish color differing little from the surrounding background. Sometimes they take on a nebulous appearance, for which at present it is not possible to

assign a reason. At other times true enlargements are produced, expanding to 100, 200 or more kilometers (60 to 120 miles) in breadth, and this sometimes happens for canals very far from the north pole, according to laws which are unknown. This has occurred in Hydaspes in 1864, in Simois in 1879, in Acheron in 1884, and in Triton in 1888. The diligent and minute study of the transformations of each canal may lead later to a knowledge of the cause of these facts.

But the most surprising phenomenon pertaining to the canals of Mars is their gemination, which seems to be produced principally in the months which precede, and in those which follow the great northern inundation, at about the times of the equinoxes. In consequence of a rapid process, which certainly lasts at most a few days, or even perhaps only a few hours, and of which it has not yet been possible to determine the particulars with certainty, a given canal changes its appearance, and is found transformed through all its length, into two lines or uniform stripes, more or less parallel to one another, and which run straight and equal with the exact geometrical precision of the two rails of a railroad. But this exact course is the only point of resemblance with the rails, because in dimensions there is no comparison possible, as it is easy to imagine. The two lines follow very nearly the direction of the original canal, and end in the place where it ended. One of these is often superposed as exactly as possible upon the former line, the other being drawn anew, but in this case the original line loses all the small irregularities and curvature that it may have originally possessed. But it also happens that both the lines may occupy opposite sides of the former canal, and be located upon entirely new ground. The distance between the two lines differs in different geminations, and varies from 600 kilometers (360 miles) and more, down to the smallest limit at which two lines may appear separated in large visual telescopes—less than an interval of 50 kilometers (30 miles). The breadth of the stripes themselves may range from the limit of visibility, which we may suppose to be 30 kilometers (18 miles), up to more than 100 kilometers (60 miles). The color of the two lines varies from black to a light red, which can hardly be distinguished from the general yellow background of the continental surface. The space between is for the most part yellow, but in many cases appears whitish. The gemination is not necessarily confined only to the canals, but tends to be produced also in the lakes. Often one of these is seen transformed into two short, broad, dark lines parallel to one another, and traversed by a yellow line. In these cases

the gemination is naturally short, and does not exceed the limits of the original lake.

The gemination is not shown by all at the same time, but when the season is at hand, it begins to be produced here and there, in an isolated irregular manner, or at least without any easily recognizable order. In many canals, (such as the Nilosyrtis for example), the gemination is lacking entirely, or is scarcely visible. After having lasted for some months, the markings fade out gradually and disappear, until another season equally favorable for their formation. Thus it happens that in certain other seasons, (especially near the southern solstice of the planet), that few are seen, or even none at all. In different oppositions the gemination of the same canal may present different appearances, as to width, intensity and arrangement of the two stripes, also in some cases the direction of the lines may vary, although by the smallest quantity, but still deviating by a small amount from the canal with which they are directly associated. From this important fact it is immediately understood that the gemination can not be a fixed formation upon the surface of Mars, and of a geographical character like the canals. The second of our maps will give an approximate idea of the appearance which these singular formations present. It contains all the geminations observed since 1882 up to the present time. In examining it, it is necessary to bear in mind that not all of these appearances were simultaneous, and consequently that the map does not represent the condition of Mars at any given period, it is only a sort of topographical register of the observations made at different times of this phenomenon.*

The observation of the geminations is one of the greatest difficulty, and can only be made by an eye well practiced in such work, added to a telescope of accurate construction, and of great power. This explains why it is that it was not seen before 1882. In the ten years that have transpired since that time, it has been seen and described at eight or ten observatories. Nevertheless, some still deny that these phenomena are real, and tax with illusion (or even imposture) those who declare that they have observed it.

Their singular aspect, and their being drawn with absolute geometrical precision, as if they were the work of rule or compass, has led some to see in them the work of intelligent beings, inhabitants of the planet. I am very careful not to combat this

* This map may be found also in "La Planète Mars" by Flammarion, p. 440. —*Tr.*

supposition, which includes nothing impossible. (To mi guarderò bene dal combattere questa supposizione, la quale nulla include d'impossibile). But it will be noticed that in any case the gemination cannot be a work of permanent character, it being certain that in a given instance it may change its appearance and dimensions from one season to another. If we should assume such a work, a certain variability would not be excluded from it for example, extensive agricultural labor and irrigation upon a large scale. Let us add further that the intervention of intelligent beings might explain the geometrical appearance of the gemination, but it is not at all necessary for such a purpose. The geometry of nature is manifested in many other facts, from which are excluded the idea of any artificial labor whatever. The perfect spheroids of the heavenly bodies and the ring of Saturn were not constructed in a turning lathe, and not with compasses has Iris described within the clouds her beautiful and regular arch. And what shall we say of the infinite variety of those exquisite and regular polyhedrons in which the world of crystals is so rich! In the organic world, also, is not that geometry most wonderful which presides over the distribution of the foliage upon certain plants, which orders the nearly symmetrical, starlike figures of the flowers of the field, as well as of the animals of the sea, and which produces in the shell such an exquisite conical spiral, that excels the most beautiful master-pieces of gothic architecture? In all these objects the geometrical form is the simple and necessary consequence of the principles and laws which govern the physical and physiological world. That these principles and these laws are but an indication of a higher intelligent power, we may admit, but this has nothing to do with the present argument.

Having regard then to the principle that in the explanation of natural phenomena it is universally agreed to begin with the simplest suppositions, the first hypotheses on the nature and cause of the geminations have for the most part put in operation only the laws of inorganic nature. Thus, the gemination is supposed to be due either to the effects of light in the atmosphere of Mars, or to optical illusions produced by vapors in various manners, or to glacial phenomena of a perpetual winter, to which it is known all the planets will be condemned, or to double cracks in its surface, or to single cracks of which the images are doubled by the effect of smoke issuing in long lines and blown laterally by the wind. The examination of these ingenious suppositions leads us to conclude that none of them seem to correspond entirely with

the observed facts, either in whole or in part. Some of these hypotheses would not have been proposed, had their authors been able to examine the geminations with their own eyes. Since some of these may ask me directly,—Can you suggest anything better? I must reply candidly, No.

It would be far more easy if we were willing to introduce the forces pertaining to organic nature. Here the field of plausible supposition is immense, being capable of making an infinite number of combinations capable of satisfying the appearances even with the smallest and simplest means. Changes of vegetation over a vast area, and the production of animals, also very small, but in enormous multitudes, may well be rendered visible at such a distance. An observer placed in the Moon would be able to see such an appearance at the times in which agricultural operations are carried out upon one vast plain,—the seed time and the gathering of the harvest. In such a manner also would the flowers of the plants of the great steppes of Europe and Asia be rendered visible at the distance of Mars,—by a variety of coloring. A similar system of operations produced in that planet may thus certainly be rendered visible to us. But how difficult for the Lunarians and the Areans to be able to imagine the true causes of such changes of appearance, without having first at least some superficial knowledge of terrestrial nature! So also for us, who know so little of the physical state of Mars, and nothing of its organic world, the great liberty of possible supposition renders arbitrary all explanations of this sort, and constitutes the gravest obstacle to the acquisition of well founded notions. All that we may hope is that with time the uncertainty of the problem will gradually diminish, demonstrating, if not what the geminations are, at least what they can not be. We may also confide a little in what Galileo called "the courtesy of Nature," thanks to which, sometime from an unexpected source, a ray of light will illuminate an investigation at first believed inaccessible to our speculations, and of which we have a beautiful example in celestial chemistry. Let us therefore hope and study.

A SIMPLE METHOD OF MOUNTING AN EQUATORIAL AXIS ON BALL BEARINGS.

F. L. O. WADSWORTH

The usual method of mounting an equatorial axis on ball bearings is to use a single, or if the instrument is very heavy, a double

row of five steel balls or rolls at each end of the axis to receive the component of pressure perpendicular to it with a third row at the lower end to take the end thrust. The components of pressure on these two sets of bearings respectively are $w \cos \varphi$ and $w \sin \varphi$; where w is the total weight of rotating parts and φ the latitude of the place. The total pressure acting to produce friction is therefore $Pw(\sin \varphi + \cos \varphi)$ which is greater than the actual weight to be supported in the ratio,

$$\frac{\sin \varphi + \cos \varphi}{1}$$

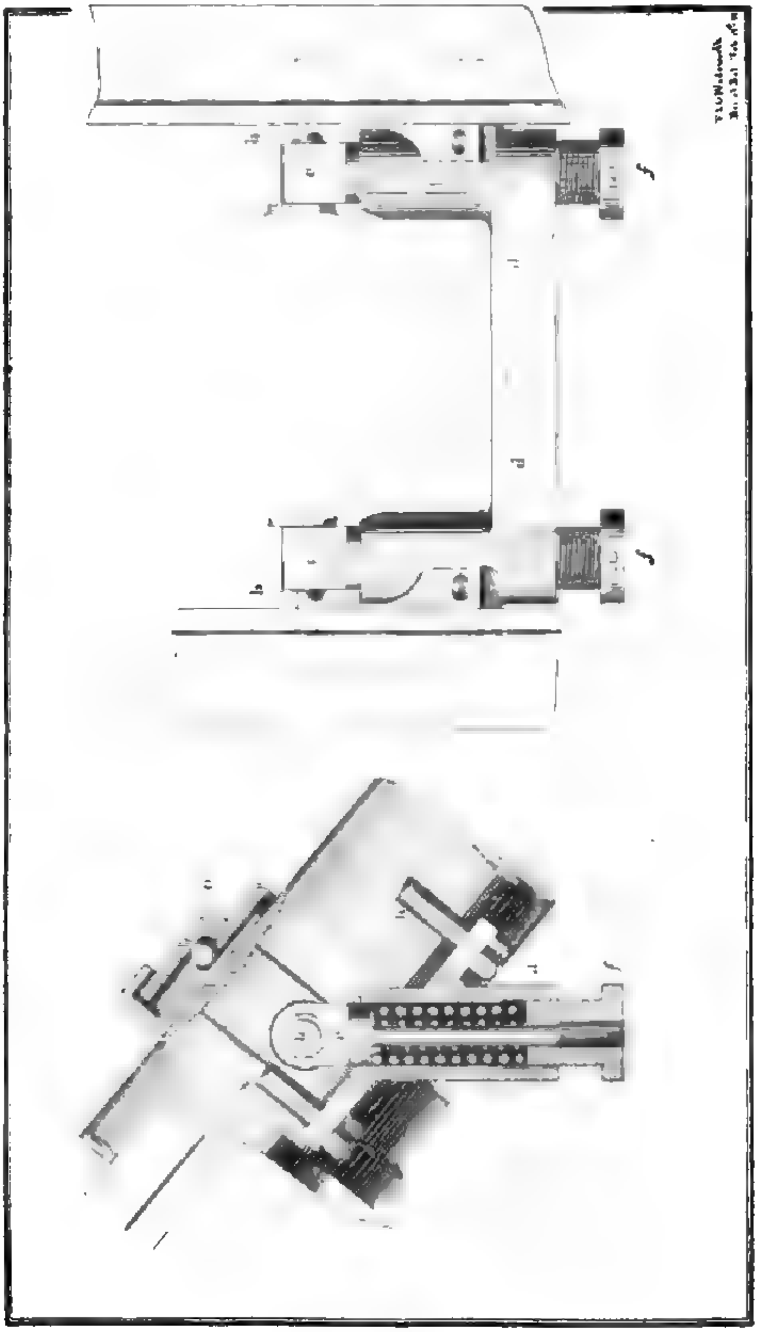
It is a maximum for $\varphi = 45°$ when $\sin \varphi + \cos \varphi = \sqrt{2}$ or $P = 1.414w$.

This method of support is illogical in another respect, for when the latitude is greater than 30° this third row of balls has to support more than ½ the weight of the axis, and the superficial pressure is therefore from twice to three times as great as on the balls of the lateral bearings, in which the pressure is distributed over two or perhaps over four rows. It is true, that wear in the end thrust bearing is of less importance than in the lateral bearings, because the former will not alter the adjustment of the axis while the latter may; but good mechanical design would aim at making the conditions of wear as nearly uniform as possible in all directions.

To secure this result and to avoid the other disadvantages, just pointed out, of the ordinary method of mounting we may use, instead of the customary three rows of balls, a single row, so placed that the plane of the bearing intersects the axis of the equatorial in the center of gravity of the latter. If this condition is fulfilled and the ball races properly designed to resist a vertical pressure, (as in Fig. 1, Plate I), the one row will support the axis in equilibrium at any angle, while allowing perfect freedom of rotation. In practice the outer race or ball cap is supported on a pair of spiral springs as shown in Plate II, which is a scale drawing of a ball bearing, recently designed on the above principle for the support of the equatorial axis of a 20-in. Foucault Siderostat. The axis is hollow, and as no telescope is attached to it, it is comparatively light, and the steel balls are therefore only ½ inch in diameter. The outer collar *a* is provided with two diametrically opposite trunnions *b, b*, which rest in the forked ends of two vertical rods *c, c*. The whole weight is carried by the two spiral steel springs whose tension can be adjusted by means of the screws *t, t*.

The casting *d, d*, which carries the screws and springs is bolted

A Simple Method of Mounting an Equatorial Axis on Ball Bearings.



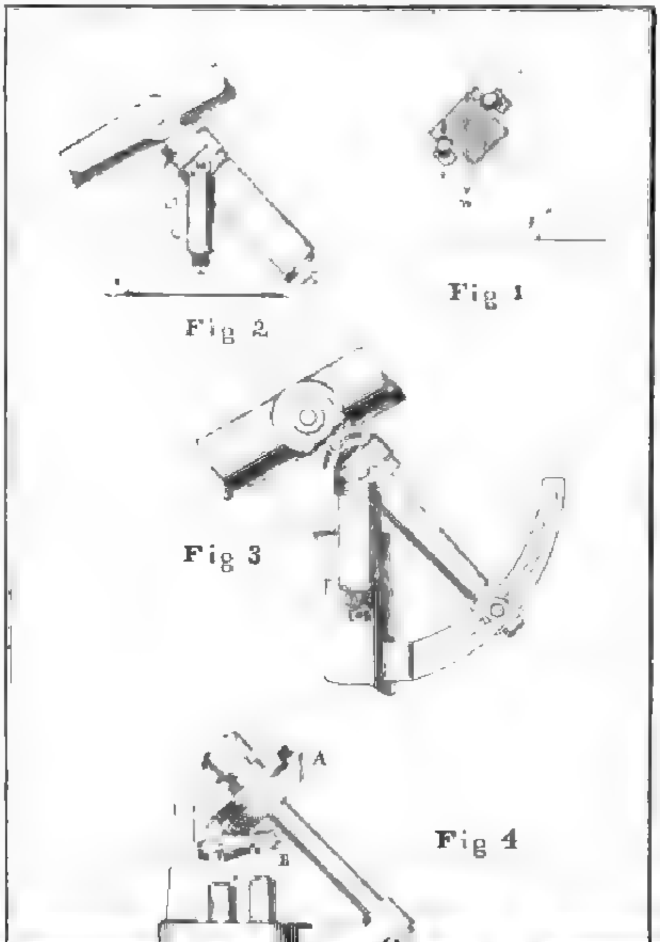
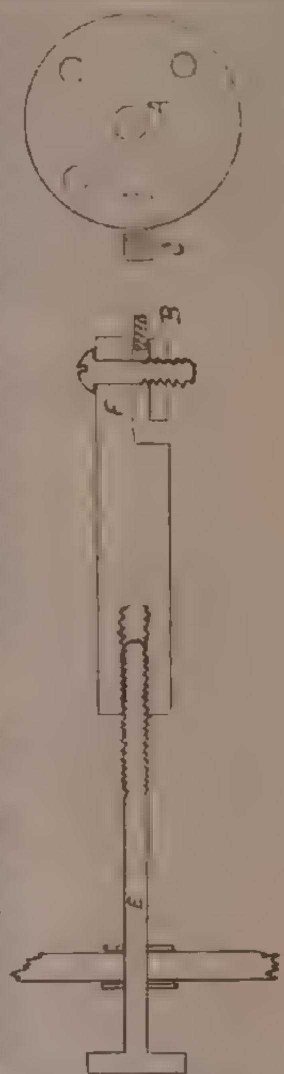


Fig 2
Fig 1
Fig 3
A
B
Fig 4



Another advantage of this method of mounting is that it may be applied, at small expense, to old instruments whose axes are already mounted in plain bearings. If the support for the axis is of the usual solid form, it will be necessary to cut away a small portion of it just below the center of gravity of the system (as in Fig. 2); but no returning of the axis itself is necessary.

For portable instruments, the form of support shown in Fig. 3 may be adopted which allows the instrument to be readily adjusted for any latitude. As the pressure on the end boxes is very slight the dotted sectors to which they are bolted need only be heavy enough to steady the axis and prevent vibration, all the weight as before being carried by the central row of balls which is placed directly above the central pillar.

* * * * * * * *

A second method of support which has more recently suggested itself is shown in Fig. 4.

Here the axis does not rest immediately upon balls, but has turned upon it a conical wheel A, whose face makes with the axis an angle, φ, equal to the latitude of the place. This wheel is in such a position on the axis that when the latter is properly mounted, the perpendicular through the center of gravity of the system, passes through the center of the lower face.

It rolls upon a second smaller conical wheel, B, whose axis is mounted on balls, and supported on spiral springs as before. The ball races are designed to take the slight end thrust, due to the resolved component of the weight, along the axis of B, which component will be equal to

$$w \frac{b}{a} \sin \varphi = \mathrm{P}$$

Hence if $\varphi = 45°$ and $b = \frac{1}{3}a$, $\mathrm{P} = \frac{1}{4}w$. The end thrust is there-

$16^h\ 34^m$ the 18-inch Brashear telescope showed that the 1st and 3d satellites were very near together, and that both of them presented elongated discs, the elongations being nearly at right angles to one another. The 2d and 4th satellites were also slightly elongated, but in different directions. The 12-inch Clark telescope was then turned upon the planet, and the observation repeated without difficulty. I next went to the 6-inch Clark, and was surprised to find that with a power of 400, not only the elongations of the 1st and 3d were easily seen, but even that of the 4th could be detected. I especially noted that the elongations had precisely the same position angles relatively to each other as in the larger instruments. The seeing was very good at the time,—8 on a scale of 10.

Since the elongations occurred in different directions for the different satellites, the appearance clearly could not be due to atmospheric conditions, or to the eye. Since identically the same appearance was seen in three different telescopes, it could not be instrumental; and finally, since the elongation has been seen at different times by at least half a dozen different persons, it cannot be a personal idiosyncrasy.

Besides furnishing another proof of the genuineness of the phenomenon, I think this observation has two other important bearings. First, as showing the relative importance of atmosphere *versus* aperture, for delicate visual observations of this sort. In this same category would be included also studies of planetary surface detail, as distinguished from the examination of very faint objects. In other words, if an observer wishes to study very faint stars he must have a large telescope. If he wishes to study the neighboring planets and brighter satellites he may use a small telescope, but he must have a very good atmosphere.

Secondly, it is hoped that this observation may encourage possessors of small telescopes to try them on the satellites, and it is possible that they may succeed in verifying what larger telescopes less favorably located have heretofore failed to detect. The test of the adequacy of the instrument and atmosphere is a simple one: it is merely, will the disc of the satellite satisfactorily bear a magnification of 400 diameters? If so, then under the most favorable conditions the ellipticity of the discs of the three larger ones should be capable of detection.

In order to aid those who may feel inclined to pursue this investigation, I have prepared ephemerides for the 1st and 3d satellites. They will be found at the end of this article. Until others have succeeded in perceiving the varying elongations of these

bodies, it is evidently useless to predict the shapes for the 2d and 4th, which are much more difficult objects to study.

Returning now to this year's work, upon September 21, an opportunity was offered to repeat my former observation upon the occultation of the 3d satellite behind the dark limb of Jupiter. A very satisfactory series of micrometer measurements were obtained for the determination of the extent and density of Jupiter's atmosphere. The flattening of the satellite due to refraction, as it approached the dark limb of the planet was very evident, and was carefully measured upon the scale of ellipticities. As the satellite set behind the planet, the dark space separating the termination from the dark limb of Jupiter was very marked, and measured about 0″.3 as determined by comparison with the threads of the micrometer. The cusps of the setting satellite seemed well defined and pointed, but the seeing was unfortunately too poor at the time to permit me to see the illumination of the atmosphere of Jupiter by the satellite after it had set as was done in 1892. (ASTRONOMY AND ASTRO-PHYSICS, 1893, p. 395.)

Surface detail has been clearly seen upon the 1st, 3d, and 4th satellites. Upon the two latter its nature seems to be the same as previously designated. Upon the former, in addition to the dark line seen in 1892, the bright equatorial region so-called, has been clearly recognized upon several occasions, even when the satellite was not in transit. In fact in one or two instances it was so conspicuous and easily visible, that it appeared at first difficult to understand why it had not been detected earlier. Further investigation, however, showed that its form is not that of a belt, as hitherto supposed, but rather that of an elongated bright mass, or possibly two masses, arranged in a plane nearly parallel to the orbit. In certain positions of the satellite these markings are, therefore, very conspicuous, while in others they are invisible. Under the latter conditions the dark line shown in my drawings of 1892 is to be seen. This explains then why I missed the bright regions in my earlier studies of this body, as it was examined for detail upon only a few occasions, and upon those occasions I apparently sketched the satellite's other side! It is thought possible that an independent determination of the period and direction of rotation of this satellite may be obtained by means of these markings, and I hope shortly to undertake this investigation.

In the paper above referred to (ASTRONOMY AND ASTRO-PHYSICS, 1893, p. 392) describing my observations in 1892, two periods were given for the 1st satellite, differing from one another by fifteen seconds, one based on the supposition that the direction of

rotation of the satellite upon its axis was direct, and the other considering the direction of rotation to be retrograde. It was stated at that time that observations made at the next opposition would decide between these two periods, as the difference in the time when the circular phase was presented by the satellite would then have amounted to over three hours. At that opposition, however, I was not so situated that I could make the observations myself, and unfortunately no one else succeded in making them. Now it happens that at the end of two years 1321 retrograde revolutions very nearly equal 1320.5 direct revolutions, and by a curious coincidence, the position of the Earth is such at the present time, that the correction which this factor introduces into the computation almost exactly makes up the discrepancy between the two results, so that the round phase of the satellite, upon the theory of direct rotation, would occur upon September 30 at 17^h 38^m M. M. T., and upon the theory of retrogade rotation upon September 30, at 17^h 40^m M. M. T. It is therefore quite impossible at present to distinguish between the two periods, by this method. It will be noted that this is the first occasion since 1892 when such an unfortunate coincidence could have occurred.

We will now see how this year's observations of the circular phase of the satellite bears out these predictions. In the following table, both the direct and retrograde theories are presented. The figures in the first column give the observations of the circular phase in 105th meridian time; those in the second the theoretical times as deduced from these observations, assuming that the rotation of the satellite is direct; those in the third the difference between the two; while those in the fourth and fifth columns give the theoretical times and differences assuming a retrograde rotation:

SATELLITE I.

Observed.				Direct Rotation.			Retrograde Rotation.		
	d	h	m	h	m	m	h	m	m
Sept	9	18	12	18	28	− 16	18	36	+ 24
	15	18	14	18	06	8	18	11	− 3
	17	16	12	15	47	25	15	51	− 21
	20	15	40	15	37	− 3	15	37	− 3
	21	17	46	17	44	2	17	43	3
	23	15	21	15	25	+ 4	15	23	+ 2
	24	17	29	17	32	+ 3	17	29	0
	30	16	57	17	10	+ 13	17	04	+ 7
Average deviation						± 9^m			± 8^m

The theory of retrograde rotation best satisfied the observations of 1892. The observations of 1894 indicate a slight advantage in its favor. This advantage would be much more marked, were it not for the first observation, which was made by daylight. The present investigation shows that the circular phase really occurred that morning after the observations had ceased. In a rigorous discussion, it would therefore probably be best that this observation should be discarded, as incomplete. This would reduce the average deviations of an observation on the hypothesis of retrograde rotation to 5^m, and would make the most probable time of the last observation, September 30^d 17^h 07^m.

We have already seen that the theoretical time of the circular phase, based upon the observations made in 1892, was September 30^d 17^h 40^m. It should be stated, moreover, that I purposely delayed making this latter computation until all the observations save the last one had been secured, in order that my mind should be entirely unbiased in the matter. On completing the computations, it was satisfactory to note that the accuracy of the observations made in 1892 was sufficient to enable me to compute the time of the circular phase for two years in advance with an error of only 33^m.

The recent observations show that a correction of $1^s.5$ should be applied to the period formerly published. This period was 13^h 03^m $10^s.8$ upon the hypothesis of retrograde rotation. The corrected period therefore now becomes 13^h 03^m $09^s.3$, and is probably correct within $0^s.2$.

The change in shape of the satellite at the time that the circular phase is assumed is quite marked, and much more rapid than at any other period. The name "circular phase," although apparently quite applicable in 1892, does not appear to be so at the present time, since the observations show that the ellipse, although it becomes much shortened, never quite reaches the circular shape. Whether it will do so later remains to be seen. The following series of figures illustrate the nature of a night's observations, and indicate the length of the major axis of the elliptical disc in terms of the minor one:

SATELLITE I.

Date.				Ell.	Date				Ell	Date				Ell
	d	h	m			d	h	m			d	h	m	
Sept	23	15	06	115	Sept	24	16	25	118	Sept	24	17	29	108
"		"	13	"	"		"	34	114	"		"	37	[illegible]
"		"	21	112	"		"	39	"	"		"	39	[illegible]
"		"	27	114	"		"	43	112	"		"	48	[illegible]
"		"	34	118	"		"	51	"	"		"	52	110
"		"	36	116	"		"	57	"	"		"	58	[illegible]
"		"	40	118	"		17	04	110	"		[illegible]	04	[illegible]
"		"	46	120	"		"	14	109	"		"	11	114
"		"	51	119	"		"	19	108	"		"	29	[illegible]
"		16	02	"	"		"	24	"	"		"	33	116

Theory indicates that the circular phase occured upon September 23, at $15^h\ 26^m$, and upon September 24, at $17^h\ 32^m$.

In the computation of an ephemeris for the 1st satellite we must make use of the following data:—

Period, $13^h\ 03^m\ 09.3^s$
Rotation, retrograde.
Epoch, 1894, Sept. $30^d\ 23^h\ 50^m$.

The period indicates the mean solar interval which elapses from one circular phase to the next but one, as seen from the Sun. The following approximation is a very convenient one, 57 periods equal 31 days. This involves an error in the period of only $0^s.17$, which may be neglected for ordinary purposes. We will continue to assume the rotation retrograde, as that assumption continues to accord best with the observations. The epoch represents the Greenwich Mean Time at which the satellite would have appeared circular as seen from the center of the Sun, had the velocity of light been infinite. To compute an ephemeris, we must allow for the time required by light to pass between the satellite and the Earth, and we must also make a correction for the angle between the Sun and the Earth as seen from the satellite. The first correction is always positive, the second is negative before opposition, and positive afterwards.

Since the rotation is retrograde, the sidereal period of the satellite is longer than the solar one. Its length is $13^h\ 03^m\ 15^s.2$. The ephemeris is more quickly computed from its solar period, since the angular distance of the Sun and Earth as seen from Jupiter is given for every other day by Marth, but the sidereal period may be employed if we compute the longitude of Jupiter as seen from the Earth. It is of interest to note that this satellite is one of the

few bodies in our system whose period is now known with considerable accuracy. It ranks in order next after Mars.

The following ephemeris gives the Greenwich Mean Times at which the satellite will present the nearest approach to a circular phase as seen from the Earth. The position angle in general varies from 0° to + 12° with regard to Jupiter's equator, but occasionally exceeds these limits, especially when the disc is near its minimum elongation. This position angle is readily measured, but is subject to rapid fluctuations, whose nature I am investigating at the present time, and hope before long to be able to predict.

EPHEMERIS FOR SATELLITE I.

Nov.	1	06	39	Nov.	13	12	22	Nov.	25	18	07	Dec.	7	23	52
		13	10			18	54		26	00	38		8	06	24
		19	42		14	01	25			07	10			[illegible]	55
	2	02	13			07	57			13	42			19	27
		08	45			14	28			20	14		9	01	58
		15	16			[illegible]	00		27	02	45			08	30
		21	48		15	03	32			09	17			15	02
	3	04	19			10	04			15	48			21	37
		10	51			16	35			22	20		10	04	05
		17	23			23	07		28	04	52			10	37
		23	55		16	05	38			11	24			17	08
	4	06	26			12	10			17	55			23	40
		12	58			18	42		29	00	27		11	06	[illegible]
		19	29		17	01	14			06	58			12	44
		02	01			07	45			13	30			19	15
	5	08	33			14	17			20	02		12	01	47
		15	05			20	48		30	02	34			08	18
		21	36		18	03	20			09	05			14	50
	6	04	08			09	52		30	15	37			[illegible]	22
		10	39			16	24			22	08		13	03	54
		17	11			22	55	Dec.	1	04	40			10	25
		23	42		19	05	27			11	12			16	57
	7	06	14			11	58			17	44			23	28
		12	46			18	30		2	00	15		14	06	00
		19	18		20	01	02			06	47			12	32
	8	01	49			07	34			13	18			19	04
		08	21			14	05		2	19	50		15	01	35
		14	52			20	37		3	02	21			08	07
		21	24		21	03	08			8	53			14	39
	9	03	55			09	40			15	25			21	11
		10	27			16	12			21	57		16	03	42
		16	59			22	44		4	04	28		16	10	41
		23	31		22	05	15			11	00			16	46
	10	06	02			11	47			17	32			23	18
		12	34			18	18		5	00	04		17	05	49
		19	05		23	00	50			06	35			12	21
		01	37			07	22			13	07			18	53
	11	08	09			13	54			19	38		18	01	25
		14	41			20	25		6	02	10			07	56
		21	12		24	02	57			8	42		18	14	28
	12	03	44			09	28			15	14			20	59
		10	15			16	00			21	45		19	03	31
		16	47			22	32		7	04	17			10	03
		23	18		25	05	04			10	48			16	35
	13	05	50			11	35			17	20			23	06

Dec.	20	05	38	Dec.	23	05	27	Dec.	26	05	17	Dec.	29	05	06
		12	10			11	59			11	48			11	38
		18	42			18	31			18	20			18	09
	21	01	13		24	01	03		27	00	52		30	00	41
		07	45		24	07	34			07	24			07	13
		14	17			14	06			13	55			13	45
		20	49			20	38			20	27			20	16
	22	03	20		25	03	10		28	02	59		31	02	48
		09	52			09	41			09	31			09	20
		16	24			16	13			16	02			15	52
		22	56			22	45			22	34			22	32

The periodical changes of the 3d satellite are not as yet sufficiently well understood to enable me to give its ephemeries with the same certainty as that of the 1st. It is likely however to be elongated upon or about the following dates:—November 1, 5, 8, 9, 13, 16, 17, 21, 24, 25, and 29. Upon November 5, 13, 21, and 29, itsposition angle will differ materially from the position it will have upon the other dates, possibly at times by as much as 60°.

On account of the comparatively slow motion of this satellite, it will be some weeks before I can connect our present series of observations with those made in 1892. When that is done, however, its motions will be pretty well understood, and it will then be possible to furnish an accurate ephemeris for many months in advance.

LOWELL OBSERVATORY, October 6, 1894.
Flagstaff, Arizona.

THE GREAT RED SPOT AND OTHER MARKINGS ON JUPITER

E. E. BARNARD

THE GREAT RED SPOT.

The surface of Jupiter is very strongly marked this opposition by two broad reddish belts, one on each side of the equator, and a broad white belt between them at the equator.

The Great Red Spot is fairly distinct in outline, though very pale—a feeble red.

The great bay in the south equatorial belt north of the Red Spot is still persistent and well marked.

Following are observations of the central transit of the Great Red Spot and the deduced longitudes (referred to Marth's System II)

			h	m			°
1894	Sept.	23	15	46.5	Standard Pacific Time	Longitude	360.9
	Sept.	30	16	32.8			360.7
	Oct.	7	17	[illegible]			361.3
	Oct.	14	18	[illegible]			359.4

From these observations, it is apparent that the spot is very closely following the motion assigned to it by Mr. Marth.

The following end of the spot is quite dark. There are white regions on its surface. The belt south of it seems to be in contact with the spot, if it does not actually overlap it slightly.

Small Black and White Spots in the Northern Hemisphere.

In the northern equatorial red belt is a number of very small black red spots. They are very slightly elongated in an equatorial direction. There are also in this belt some small well defined white spots—similar in size and form to the dark ones. Indeed in certain regions of the belt, these black and white spots alternate with decided regularity.

Immediately opposite the Great Red Spot and near each other lie a couple of these objects—a black and a white spot. These are strikingly well defined and easy of observation.

I have selected these for a series of measures to determine their relative motion.

The measures show that the white spot (which follows) is slowly gaining on the dark one; the distance is diminishing about 0″.05 daily. This will bring them together near the middle of January, 1895.

The conjunction of these spots would be a very interesting and important phenomenon, if they were exactly in the same parallel, since an occultation of one or the other must then occur and the result would show us which is uppermost in the Jovian atmosphere. Unfortunately, however, there will be no occultation as the south edge of the white spot is exactly on the same parallel with the north edge of the dark one. They will perhaps graze in passing; it will therefore be interesting to watch the conjunction if they remain sufficiently permanent.

Following are the measures of the distance between their centers when one or the other was in transit.

		d	h	m			″
1894	Sept.	23	15	48	Standard Pacific Time.	Distance =	5.82
	Sept.	30			Standard Pacific Time.		5.78
	Oct.	7	17	5	Standard Pacific Time.		5.32

Reduced to distance 5.20 these become

	″
Sept. 23	5.67
Sept. 30	5.52
Oct. 7	4.97

The small black spot seems to have approximately the same rotation period as that of the Great Red Spot.

TRANSIT OF THE SMALL WHITE SPOT.

	d	h	m		
Sept	23	15	55	Standard Pacific Time.	Longitude = 366°.0

TRANSIT OF THE SMALL BLACK SPOT.

	d	h	m		
Oct.	7	17	0.0	Standard Pacific Time.	Longitude 349°.3

Estimated transits of this object on

Sept. 23 gave longitude 349°.2
Sept. 30 gave longitude 351.0

This and the other black spots, are very similar in appearance and location to the small black spots of 1890 and 1891. See my papers on these objects in *Monthly Notices* R. A. S., vol LI and vol. LII. The previous spots, however, decreased their longitudes about a quarter of a degree daily

The customary white spots in the southern hemisphere are as abundant as ever.

These observations have been made while examining the planet with the 36-in. to get early observations of the fifth satellite

On Sept. 10^d 17^h $1^m.6$ with the 12-inch, another of the small black spots was in transit, and its longitude was 108°.8

The great white equatorial belt has been singularly free of markings of any kind. There are now, however, a few dusky markings appearing in it.

MT. HAMILTON, Oct. 18, 1894.

THE POLAR CAP OF MARS.*

A. E. DOUGLASS

The recent disappearance of the Martian snow cap renders more interesting its position and size as observed during the week preceding this occurrence. On October 4 the following side (the side toward the Sun) of the cap was noticed to be much brighter than other portions, presenting an appearance similar to that of June last but on a much smaller scale. On October 5, at nearly 19^h, G. M. T., a very narrow dark line was observed dividing the cap into two slightly unequal parts, the following part being the larger and showing a very bright north-following

* Communicated by the author.

edge. Three hours later a determination of its size and position was made. October 7, at about 19^h, G. M. T., the rift or dividing line in the cap was noticed to have a direction *s.-f.* and *n.-f.* and it was estimated that at $19^h\ 42^m$ it would have a direction perpendicular to the nearest part of the limb.

On October 8 the position and size of the snow cap was again taken, but the rift was not observed though it was in a favorable position. At this observation the cap appeared about three times as large in area as on the 5th and 12th of the month. A lower power eyepiece (630 diameters instead of 840) was used in this case alone, which might explain some of the increase in size by irradiation; but the whole change cannot be thus disposed of. On August 19 a comparison had been made between powers 630 and 420 between which the difference in irradiation should be more noticeable, but no disagreement in the size of the snow cap was found. The snow cap seems actually to have been of larger size on October 8.

No further observations were made upon this object until October 12 at $20^d\ 26^m$, G. M. T. when its size and position were obtained but no rift was seen. The seeing, 3 or 4 on a scale of 10, was not sufficiently good to show it had there been one. On October 13 no polar cap was visible nor has any sign of one been seen since up to the present time (October 17). Since this disappearance no part of the region occupied by the snow cap has appeared as bright as regions close to the northern limb. The region seems different in no respect from the preceding and following limbs of the planet.

The results of the observations are here presented.

Position of Snow Cap		d	h		Computed Distance from Pole
	Central Oct.	12	20.4	G. M. T.	
Turned 50° toward *p*	"	8	21.4	"	4°.3 wt. 2
" 83 "	" "	5	21.9	"	5 .0 " 3
	Mean distances from south pole				4 .7
	Longitude				59

Size of snow cap:

Oct. 12.9 E. and W. 0″.72 = 140 miles in longitude.
N. and S. 0″.36 = 175 miles in latitude.
(1″ = 36.84 miles) area 19,500 square miles.

Oct. 8.9 E. and W. 0″.99 = 193 miles.
N. and S. 0″.72 = 380 miles.
Area 64,500 square miles.
By computation, width in longitude 146 miles.
By computation, width in latitude 128 miles.

Oct. 5.9 E. and W. 0″.81 = 157 miles in latitude.
N. and S. 0″.36 = 220 miles in longitude.
Area 27,900 square miles.

From nearness to the limb E. and W. measures should have about three times the weight of N. and S. measures.

Rift in snow cap.

Oct. 5.8 rift pointed at (equatorial) long. 101.6
Oct. 7.8 rift pointed at (equatorial) long. 92.4

Mean direction of rift toward longitude 97.0 at the equator.

Since Flammarion gives no instance of the complete disappearance of the snow at either pole we may consider the present case to be the first recorded. The smallest minimum given in "La Planète Mars" was observed by Schiaparelli in 1879 at the south pole. The measured diameter of the cap was 3°.8 or 140 miles or 1.6 times as large as on October 12. Moreover Schiaparelli's minimum occurred 75 days after summer solstice, but he was inclined to attribute nearly half of this size to irradiation and thought 2° or 74 miles an equally probable figure.

Schiaparelli's minimum occurred 75 days after the summer solstice and for about 55 days longer the cap did not reach 10° in diameter. In the present opposition October 12 was but 42 days after the summer solstice and 130 days after the solstice will bring us to January 8, 1895. Therefore, while it is impossible to say whether or not we shall have occasional reappearances of the polar cap, it seems unlikely that it will attain any great size for some months to come.

LOWELL OBSERVATORY, Flagstaff, A. T., Oct. 17, 1894.

MARS

PERCIVAL LOWELL

On Sept. 24th an interesting observation was made here by Mr. Douglass of what appears to have been the formation and subsequent dissipation within twenty four hours of a cloud over the western half of Elysium, that part of it which lies between Galaxias and Hyblaeus. Three accompanying drawings of his show what occurred. On Sept. 22d and 23d the area referred to was of about the same brilliancy as the eastern half of the region, but on Sept. 24th he observed the western half much brighter than the eastern, almost as brilliant as the polar cap; on Sept. 25th the western half had faded again and become darker than the other. Their appearances suggest clouds, forming presumably over high ground, since neither Galaxias nor Hyblaeus were in any way obscured. On the contrary he found the canals perceptibly darker.

Solis Lacus has shown a longitudinal division. The division detected by Mr. Douglass begins in the Nectar, a light line running along the middle of it, which is continued, much fainter, through the Lake of the Sun. The best seeing is necessary to see this. Under poorer seeing Solis Lacus has appeared to him trisected horizontally, an effect caused chiefly by the dark patches that show in his drawing.

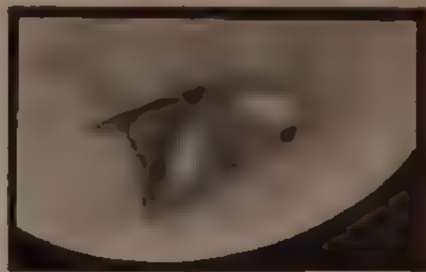
Mr. Douglass finds a small rift in the minute snow-cap which is further interesting as being possibly about the last one to occur before the weather turns cold and the cap begins to increase again.

LOWELL OBSERVATORY, Oct. 12, 1894.

MARS.

PLATE XXXII.

N

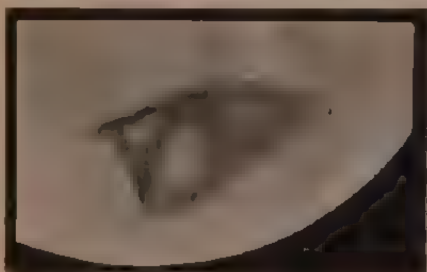


N

Trivium Charontis Region.

1894, Sept. 22, 19h 20m, G. M. T.
Same as Sept. 23, 20h 43m. Seeing 3 to 5.

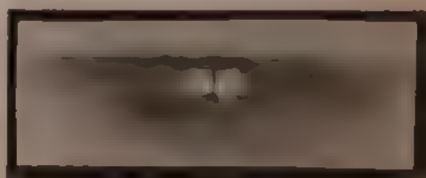
1894, Sept. 24, 20h 15m, G. M. T.
Power 630. Seeing 6.



N

Trivium Charontis Region.

1894, Sept. 28, 21h 54m, G. M. T. Power 630.
Seeing 4. Scale same as two above, 4mm = 1''



S. Snow.

Oct. 5, 11h 59m, G. M. T. Power 860.
Seeing 7. Scale 30mm = 1''.

Solis Lacus Region.

1894, Oct. 8, 13h 13m to 14h 16m
Power 431. Seeing 4 to 9. Scale 4mm. = 1''.

Drawings by A. E. D

Astro-Physics.

ON A NEW METHOD FOR MAPPING THE SPECTRA OF METALS.

HENRY CREW AND ROBERT TATNALL

The difference in physical character between the various lines in the spectrum of an element has recently assumed such importance that a table of wave-lengths is now to some extent incomplete unless accompanied by a photographic map. This is especially true for one who is seeking new relations among the wave-lengths.

Thus, in the case of Cadmium, the triplets overlap: but, "owing to the physical similarity of the lines forming any one triplet it is a matter of perfect ease to select them."*

Indeed, in many cases where series have been discovered one might decide to what series a given line belongs quite as well by its appearance as by its wave-length. Rydberg has happily suggested for these series names which describe the appearance of their respective lines.

So far as we are aware, all photographs of metallic spectra which have hitherto been made are with two exceptions, either of spark spectra or spectra of substances vaporized in the carbon arc. The two exceptions to which we refer are, *first*, the well-known spectrum of iron by Kayser and Runge in which the arc employed is that between iron-rods about one centimeter in diameter, and *secondly* a copper arc with which these same gentlemen have attempted to vaporize strontium and thus obtain the triplet† at λ 3800 free from the cyanogen band. They say, however, that the arc worked so badly as to give only one line out of the three.

The well known difficulty with the spark spectrum is that it is almost as characteristic of the slight differences in physical condition under which it is obtained as of the chemical element from which it is obtained. Not only so, but owing to its streaks, as it were, of high temperature ("luminescence"?) there is obtained at the same time with the spectrum of the metal also the spectra of the gases in which the discharge takes place.

In the case of the carbon arc, nature has fortunately grouped its many thousand lines into bands, leaving here and there comparatively clear spaces in which the lines due to substances delib-

* Ames: *Phil. Mag.* July, 1890, p. 45.

† Kayser and Runge: *Wied. Ann. Bd.* 52, p. 115 (1894).

erately introduced into the arc can be studied with a high degree of accuracy as exemplified in the work of Rowland and of Kayser and Runge.

Fortunately, also, in the case of some metals, especially the easily volatile ones, the metallic vapor acts *as if* it shunted off the current from the carbon vapor: and the metal comes out strong in comparison with the carbon.

At the same time, the carbon and cyanogen bands extend practically through the whole spectrum from λ 3500 into the infra red. Not only so, but many of these carbon lines have, as a rule, intensities quite comparable to those of the metallic lines. One ingenious effort has been made by Kayser and Runge (*l. c.*) to rid themselves of the cyanogen bands by working the carbon arc in a current of carbon dioxide. This is partially successful; but, at best, it only *diminishes* the intensity of the band. Messrs. Lewis and Ferry,* speaking of the infra-red metals, say, "It seems as though little more could be done in the discovery of new metallic lines unless the carbon lines are first carefully mapped, or some means is devised for raising the substances investigated to sufficiently high temperature without placing them directly in the [carbon] arc."

We have, therefore, devised and used during the past year the following method for obtaining the arc spectrum of the metallic elements free from carbon, free from air lines, and free from any continuous spectrum.

The idea is simply that of an arc in which one pole rapidly rotates or vibrates, and thus prevents welding and destroys the coating of oxide which in many cases interrupts the current between ordinary metallic poles.

To accomplish this a brass disc is fitted by means of a collar and set screw to the shaft (or counter-shaft) of a small high speed electric motor. Parallel to this brass disc, and upon it as a base, is screwed a similar disc. These discs are used as jaws in which to clamp small pieces of metal to be vaporized. One pole of the electric circuit which includes the arc is connected, by brushes, to the counter-shaft shown in section at A (Fig. 1). The other pole of the arc circuit is connected to another clamp F, which by means of the screw E can be made to approach or recede from the rotating disc. The clamp is also fitted with parallel jaws to receive a small piece of metal, B, to be vaporized. This metal, B, is moved always parallel to itself, and the arc between B and C is maintained always at the same point. Both the rotating and

* *Johns Hopkins University Circular*, May 1894.

the sliding jaws are mounted on the same base with the motor: and the whole is so light as to be easily carried in one hand.

The disc is set in rapid rotation and the metal at B is slowly fed in, by the screw at E, until the arc strikes. The incandescent vapor is then carried out by the disc into the form of an open fan, and is projected upon the slit of the spectroscope by the "image" lens.

In the case of those elements which are easily obtainable in the form of a regulus an entire disc may be made of the metal. With the rarer metals one needs to use only a small piece in the clamp, but the time of exposure is correspondingly lengthened. The disc once started, no attention is required except the feeding in of the metal B. Nearly all the wear is on this piece and very little on the disk, so that the latter will last for a comparatively long time while the former has to be renewed with a frequency depending upon the amount of current employed. We have generally used a hundred-volt circuit and an alternating current of from two to ten amperes. Higher voltages sustain a longer arc and protect the metal from mechanical wear.

For the purpose of a comparison spectrum, is used a second counter-shaft placed parallel to and in the same horizontal plane with the first. This shaft carries an iron disc, about an inch in diameter, against which is fed a piece of iron tubing. The spectrum of any one metal having been photographed, the whole instrument is translated laterally and the current switched on to the iron disc.

While not so convenient as the Sun in many ways, the iron spectrum has an abundance of sharp lines evenly distributed; it permits one to work in all kinds of weather and at night.

The plates whose measures follow will illustrate the method. They were taken with a Rowland concave grating of ten feet radius and ruled with fifty thousand lines. The portion of the plate measured, in each case, covers a part of the spectrum where the carbon bands are strong. Knowing of no adequate method of reproduction, except silver printing, which is too expensive, we have selected three typical plates and simply measured on a dividing engine all the lines visible including "ghosts" and recognized impurities. The tables explain themselves. They include all the lines certainly visible through the reading microscope of the dividing engine; but a still lower power microscope shows a number of weaker lines between those measured.

The wave-lengths were determined not with the highest accuracy possible; but well within a tenth of an Angström unit, which

is usually ample for purposes of identification. The method was interpolation between two of Rowland's standard iron lines, except in the case of copper where, for convenience, the interpolation is between two of Kayser and Runge's copper lines.

Plate No. 178. Tin

Element	Plate 178.	Kayser and Runge.	Remarks
Tin	4893.66	3262.44	Third order line, not completely absorbed by glass
Tin	4762.72	3175.12	Third order line, not completely absorbed by glass.
(Ghost)	4531.20		Second order ghost of Sn 4524.92.
(Ghost)	4528.04		First order ghost of Sn 4524.92.
Tin	4524.91	4524.02	Intensity 2.
(Ghost)	4521.77		First order ghost of Sn 4524.92.
(Ghost)	4518.63		Second order ghost of Sn 4524.92
	4511.43		Intensity 6, sharp. Not recognized.

Plate No. 177. Copper.

Element	Plate 177.	Kayser and Runge	Intensity.	Remarks
Copper	4003.18	4003.18	5	
	3998.08		6	Hazy
	3979.97		6	Hazy
	3976.14		6	Extremely wide and hazy.
Calcium	3968.55		4	Fraunhofer's H.
	3964.27		6	Wide and hazy.
	3961.64		6	Very weak.
	3951.63		6	Very weak.
	3947.00		6	Hazy.
Calcium	3933.76		4	Fraunhofer's K.
	3933.11		6	Wide and hazy.
Copper	3925.36	3925.40	5	
Copper	3821.32	3821.38	5	
Copper	3899.42	3899.43	6	
	3888.73		6	Very weak and hazy: vacuum line at λ 3888.83
	3883.39		6	No resemblance to head of C band at 3883.47.
	3881.75		6	Hazy
(Ghost)	3865.97			Second order: belongs to Cu 3860.64.
Copper	3861.90	3861.88	[illegible]	
Copper	3860.57	3860.64	2	
Iron	3860.03		6	Fe 3860.03 (K. & R.)
(Ghost)	3857.88		6	First order: belongs to Cu 3860.64.
	3844.57		[illegible]	Wide and hazy
	3837.48		6	Wide and hazy.
Iron	3825.99		6	Sharp trace of Fe 3826.04 (K. & R.).
Copper	3825.17	3825.13	6	
Copper	3820.97	3821.01	4	
	3820.52		6	Sharp: unlike Hg. 3820.6 (K. & R.).
	3817.57		6	Hazy
	3813.60		5	Probably not copper.
Copper	3812.08	3812.08	6	Wide and hazy.
Copper	3805.29	3805.33	3	

Element	Plate 177	Kayser and Runge.	Intensity.	Remarks.
	3803.64		6	Wide and hazy.
	3800 57		4	Fairly sharp.
	3799 99		5	Rather sharp.
	3797 34		6	Hazy.
	3785.74		6	Wide and hazy.
	[illegible]		6	Wide and hazy.
Copper	3771 96	3771.96	4	
	3764.98		6	Wide and hazy.
Copper	3759 56	3759 53	4	
Iron	3758 36		6	Fe 3758 38 (K. & R.).
Iron	3749 61		6	Fe 3749.61 (K. & R).
	3745 53		5	Hazy.
	3743.53		6	
Copper	3741 36	3841.32	3	
Iron	3737 27		6	Fe 3737 27 (K & R).
Iron	3734 96		6	Sharp trace of Fe 3735.00 (K. & R.)
Copper	[illegible]	3734.27	3	
	3721.79		6	Hazy.
	3720 89		5	Very sharp.
	3720.09		6	Sharp.
Copper	3712 06	3712 05	4	Hazy.
	3707.31		6	Exceedingly weak and hazy.
Iron	3701.20		6	Trace of Fe 3701.20 (K. & R.).
Copper	3700 61	3700.63	3	
	3699.17		6	Hazy.
	3695.48		6	Hazy.
Copper	3688.38	3688 60	[illegible]	Exceedingly wide and hazy.
	3686 67		5	Rather sharp.
	3685.04		6	Rather sharp.
Copper	3684.77	3684.75	[illegible]	
Lead	3683 60		6	Faint trace of Pb. 3683 60 (K. & R).
Copper	3676 96	3676 97	5	
Copper	3672 04	3672 00	5	
Copper	3665 83	3665 85	4	
	3664 21		6	Hazy.
Copper	3659 44	[illegible]	5	
Copper	3656 86	3656 90	6	
Copper	3655.99	3655 99	4	
Copper	3654 47	3654.60	6	
Copper	3652 48	3652 56	6	
	3650.97		6	Hazy.
Copper	3648 52	3648 52	5	
Copper	3645.31	3645.32	4	
	3643 80*		6	
Copper	3641 80	3641 79	5	
Copper	3636 01	3636 01	4	
	3632 67		[illegible]	Shaded towards violet.
	3629 90		6	
Copper	3627 40	3627 39	4	
Copper	3624.36	3624.35	5	
Copper	3621 32	3621 33	3	
Copper	3620 47	3620 47	5	
	3619.52		6	Certainly not copper.
(Ghost)	3618 88		6	First order belongs to Cu 3621 33
(Ghost)	3616.37		6	Second order belongs to Cu. 3621.33.

* The iron line at 3643.80 (K. & R.) is much weaker than some of its neighbors which do *not* show as impurity lines. Hence this line is probably not iron.

Element	Plate 117.	Kayser and Runge	Intensity.	Remarks
Copper	3614.31	3614.31	6	
Copper	3613.85	3613.86	4	
	3610.88		5	Hazy. Strong Cd. line at λ 3610.66
	3609.43		5	Very sharp.
(Ghost)	3607.22		[illegible]	Second order: belongs to 3602.11.
(Ghost)	3604.64		6	First order: belongs to 3602.11
(Ghost)	3604.30		6	Second order: belongs to 3599.20
Copper	3602.10	3602.11	[illegible]	
Copper	3599.20	3599.20	3	

Plate No. 169. Zinc.

Element	Plate 169	Kayser and Runge	Remarks.
(Ghost)	4828.26		Fifth order
(Ghost)	4824.21		Fourth order.
(Ghost)	4821.19		Third order.
(Ghost)	4817.41		Second order.
(Ghost)	4814.18		First order.
Zinc	4810.79	4810.71	Intensity 1.
(Ghost)	4807.36		First order.
(Ghost)	4804.11		Second order.
(Ghost)	4800.84		Third order.
(Ghost)	4797.30		Fourth order.
(Ghost)	4793.99		Fifth order.
(Ghost)	4738.70		Fifth order
(Ghost)	4735.52		Fourth order.
(Ghost)	4732.26		Third order.
(Ghost)	4728.89		Second order.
(Ghost)	4725.70		First order.
Zinc	4722.34	4722.26	Intensity 1.
(Ghost)	4719.03		First order.
(Ghost)	4715.79		Second order.
(Ghost)	4712.67		Third order.
(Ghost)	4709.36		Fourth order
(Ghost)	[illegible]		Fifth order
(Ghost)	4693.27		Fourth order
(Ghost)	4686.85		Second order
(Ghost)	4683.50		First order.
Zinc	4680.38	4680.38	Intensity 1.
(Ghost)	4677.04		First order
(Ghost)	4673.97		Second order.
(Ghost)	4670.66		Third order.
(Ghost)	4667.72		Fourth order.
(Ghost)	4664.25		Fifth order.
Zinc	4630.06	4630.06	Intensity 1
Zinc	(4613.96)	3075.99	Intensity 6 third order line
Zinc	(4608.29)	3072.19	Intensity 6 third order line.
Zinc	(4553.83)	3035.93	Intensity 6 third order line.

Out of 98 lines measured on the copper plate it will be noticed that we are unable to identify 41. They are not to be found among Kayser and Runge's values for Ag., Au., Sn., Pb., As., Sb., Mg., Ca., Zn., Sr., Cd., Ba., Hg., Li., Na., K., Rb., Cs., or Fe

It is probable that these 41 lines belong to impurities whose wave-lengths have not yet been determined (or, at least, not published) with an accurrcy sufficient for identification. It is not impossible, however, that some of these are *new* copper lines. We have found very little difference between "commercial" copper and that which is sold by chemical supply houses under the label "chemically pure."

From the tables it will be seen that the plates are practically clear except for the impurity lines, which are very weak, many of them not showing on a silver print. In any case, a table of impurity lines and ghosts might accompany each map. A few years hence, when the spectra of the metals are more completely measured, such a table will be easily made.

NORTHWESTERN UNIVERSITY,
Evanston, Illinois, July, 1894.

THE INFRA-RED SPECTRA OF METALS *

E. P. LEWIS AND B. S. FERRY.

In order to extend the range of standards of wave-lengths, we have, under Professor Rowland's direction, been engaged for several months in a bolometric investigation of the infra-red spectra of metals. The approximate wave-lengths of a few lines in this region have been found by Becquerel, Abney, and others. The most recent and reliable results are those obtained by Snow, who, with a fluorspar prism and bolometer, investigated the spectra of the alkaline metals. He measured a number of wave-lengths by an ingenious interference method, with probably the highest degree of precision attainable by the use of a prism. In order to obtain any high degree of accuracy, however, it is necessary to use a concave grating, although the difficulties are very much increased by the greater dispersion and the division of the energy among a number of spectra. On account of these difficulties very little in the way of definite results has as yet been accomplished, because it will be necessary to compare a large number of observations in order to eliminate the many accidental effects which unavoidably occur. We have not as yet succeeded in making a large number of reliable observations, because it has been impossible to take any trustworthy readings during variable weather, there being no means of keeping the room at a constant temperature.

* Johns Hopkins University Circular, No. 112.

Apparatus and Methods of Observation.

The apparatus consists of a six inch concave diffraction grating of 21 ft. 6 in. radius with 14000 lines per inch, mounted on a spectrometer of the usual Rowland pattern,* a bolometer and a galvanometer. The spectrometer is so arranged with heliostat and arc-lamp that by the simple movement of a cord either the solar or arc spectrum can be observed as desired. A salt of the metal whose spectrum is to be investigated is placed in holes drilled in the carbons.

The galvanometer is one designed by Professor Rowland especially for this experiment, and contains several new points that make it much easier to adjust and use than the ordinary forms. The two sides containing the coils are hinged at the bottom, and when closed are fastened to the frame by thumb-screws, enabling one to examine the system with minimum difficulty. The four coils are in brass cups that can be moved to different distances from the needles, or can be entirely removed and others substituted in a few seconds. In these experiments, the coils are wound to about 25 ohms resistance each, and coupled in multiple so that the resistance of the four coils is about 6.25 ohms. For the needles, various kinds of steel were tested, and the selection made by the following device: after the little steel bars were shaped, hardened and magnetized, a pair pointing in the same direction was fastened to a minute glass tube and the period of oscillation observed. The pair giving the most rapid period is the strongest. Then one of the needles was reversed and the period of oscillation again observed. The pair giving the slowest period is the most nearly astatic. The pair of needles best fulfilling these two conditions was selected. The entire system weighs 0.125 grams of which the needles weigh 0.085 grams. Both quartz and silk suspensions have been used, but a silk one has been finally adopted on account of its smaller torsion. The figure of merit of the galvanometer as actually used with a period of ten seconds per single swing is of the order 10^{-9}; but, if desirable, this could be greatly increased. In bolometric work, however, the effect of extraneous disturbances gives a practical limit to the sensitiveness desirable, which, with the apparatus employed under the conditions of this experiment, is about that given above.

Great difficulty was experienced in securing suitable platinum strips for the bolometer. An attempt was made to make strips by electro-deposition on silver, but deposits of a suitable thickness

* For description of this spectrometer see a paper by Dr. J. S. Ames in Astronomy and Astro-Physics, January 1892.

were found too fragile for use. Those finally used were obtained by hammering No. 40 wire flat between two pieces of polished steel, the wire being stretched by a bow of weak spiral spring during the process. The diameter of wire was about 0.042 mm., and the strips produced were about 0.4 mm. in width and 0.004 mm. thick. The platinum was evidently quite impure, its temperature coefficient being only 0.0027. The resistance of the wire was about 2 ohms per inch at 20° C.

The bolometer was arranged differentially according to Professor Rowland's suggestion, so that the effects due to variations in the arc might be to a great extent eliminated. Two parallel strips of platinum about 2.5 cm. long were placed about 4 mm. apart in a vulcanite box containing two openings—one in front to admit the radiation and another in the rear so that the strips could be seen by the observer. The other arms of the bridge were of German silver. Originally each was of fine wire, of about 36 ohms resistance, wound on a brass cylinder screwed in the top of the vulcanite box, but it was found that the current used—about 0.3 amperes—heated it considerably, and the heat, being conducted downward by the brass cylinder, produced considerable disturbance. Arms of larger wire with a resistance of about 4 ohms, wound upon a vulcanite cylinder, were finally substituted with more satisfactory results. It was, of course, impossible to secure a perfect balance on the arms of the bridge, so that it was necessary to put a resistance box as a shunt to an arm. The bolometer was mounted in a brass tube which was substituted for the eye-piece on the spectrometer arm.

The platinum strips being both exposed to the radiation, a double set of readings is obtained on each hot line as the bolometer is moved through the spectrum. There is little danger of confusion arising from the use of two strips, unless a number of strong lines are grouped together, while the diffused radiation from the arc should affect both equally. At first, great trouble and loss of time was experienced from rapid drift of the needle. It seems that nearly all who have used the bolometer have met with this difficulty. Professor Langley, with all the precautions and skillful devices suggested by his long experience, has not succeeded in entirely doing away with it. Snow attributed it to lack of homogeneity in the strips due to smoking over a flame. Our experience indicates that lack of homogeneity is the correct explanation. After breaking the original strips, several pairs of new ones were tried, all giving great trouble. Finally, a pair was used which produced very little drift. Nothing was changed in the

bolometer or its connections except the strips, so it seems evident that the processes of annealing, hammering, soldering and smoking may produce considerable differences in the temperature coefficient of strips from the same piece of wire and to all appearances similar. Thus the selection of suitable strips seems largely a matter of chance.

It was thought best to use rather wide strips on account of the increased sensitiveness due to greater surface exposed to radiation and to the larger current which could be employed. The arc subtended by the strip, however, is less than 12″. This is very near the limits of accurate measurement at present attainable. The strips used by Snow, though very narrow, subtended an angle of about 69″, while of course the dispersion of his prism was much less than that of the grating used in these experiments. The calculated sensitiveness of the bolometer is about .000001 degree.

In order to decrease the effect of air currents about the wrappings of the bolometer, it was decided to try the effect of having all four arms made of thin strips as nearly identical as possible, and having them all close together within the same enclosure. For the purpose of increasing the galvanometer deflections two changes were made in the original arrangement; first, each strip was doubled back upon itself so as to have twice the area and twice the resistance acted upon by the light; and second, all the strips were made effective by placing the ones corresponding to opposite arms of the bridge one back of the other in such a manner that two compound strips were formed each of four times the width of a single one. The same differential arrangement was employed as in the instrument first constructed. The case was made of metal and the openings closed with glass slides so as to be draught tight. A current of 0.3 amperes was used in the bolometer, and for the purpose of keeping the temperature of the instrument constant, this current was kept in the instrument continuously.

The method of observation consists in slowly moving the bolometer through the metallic spectrum produced by the volatilization of a metal in the arc, and noting all galvanometer deflections. The motion of the carriage supporting the bolometer is produced by a very accurate screw made by Professor Rowland's process, enabling the position of the bolometer strips to be read down to .01 of an Angström unit. Similar carbons are then substituted, in which none of the metal has been introduced, and the galvanometer deflections again noted. The deflections observed with the

metal that are not observed when the clean carbons are employed, are due to the bright line spectrum of the metal. Of course, in both sets of observations, there will be many deflections due to extraneous magnetic and mechanical disturbances, but these can usually be distinguished from the deflections produced by a bright line by the fact that with the differential arrangement here employed, a bright line gives four distinct galvanometer deflections of definite directions and separated from each other by definite distances. The only uncertainty occurs when the bright lines are close together, as in the case of a carbon band. To overcome this uncertainty, the region is traversed several times by different bolometers in which the strips are separated from each other by different incommensurable known distances.

As a test of whether the line thus discovered really belongs to the metal to which it is supposed to be due, one strip of the bolometer is placed at the point where the line is supposed to be located and the arc lamp started burning clean carbon. An end of a bored carbon filled with a salt of the metal is then placed in the highly heated gaseous envelope of the poles and the galvanometer observed. If a deflection occurs in the proper direction, the bolometer is moved till the strip is replaced by the other one, and the charged carbon again inserted in the arc. There ought now to occur a deflection of the galvanometer in the opposite direction. By repeating these observations many times, consistent results have already been obtained which locate several infra-red metallic lines. The wave-length of these lines was determined by comparison with the overlapping solar spectrum of the second order.

The region thus far investigated is in the first spectrum and extends only to about 12000 Angström units, so that there is little trouble due to overlapping spectra, which can be cut out by various absorbing media.

The principle difficulty experienced has been with the drifting of the galvanometer needle. After preparing bolometer strips which give a minimum of drift, the precautions which have been found most effective in decreasing it are: thermal insulation of the bolometer and connections in the leads from air draughts, stationary temperature of the strips themselves by the continuous passage of a constant current through them, uniform temperature of the room in which the experiments are performed, frequent astaticization of the galvanometer needles, shielding of the galvanometer connections from radiation by the use of a metallic screen, rheostat for the bolometer shunt having coils of low temperature coefficient.

We at first attempted the investigation of calcium, but the co-existent carbon lines made the attempt hopeless.

It is well known that when sodium and potassium salts are burned in the arc they cool it to such an extent that the carbon lines are almost eliminated in the visible spectrum. For this reason we used sodium, so that complications due to the carbon lines might be avoided, but to our surprise found that scarcely any effect of the kind was noticable in the infra-red. We then decided to confine ourselves in the beginning to the more accurate measurement of lines whose positions are already approximately known. The following infra-red wave-lengths for sodium have been found previously:

Abney.	Kayser and Runge	Snow
8199	8200.3	8180.
8187	8186.3	

We have found decided indications of lines approximately coincident with the above, but have not yet succeeded in fixing the position of the maxima closely enough for more accurate measurement. Snow found an intense line at 11430, and Kayser and Runge have, by calculation from empirical formula, predicted two lines of wave-lengths 11504.8 and 11481.8. We have found decided evidence of two sodium lines of approximate wave-lengths 11488 and 11468.

We intend to go on with the more accurate measurement of lines whose position is already approximately known, and to make at least a preliminary survey of the infra-red carbon spectrum. It seems as though little more could be done in the discovery of new metallic lines unless the carbon lines are first carefully mapped, or some means is devised for raising the substances investigated to sufficiently high temperature without placing them directly in the arc.

THE SPECTRUM OF MARS.*

W. W. CAMPBELL.

The spectrum of Mars has been observed by several eminent astronomers—Rutherfurd, Secchi, Janssen, Huggins, Vogel, Maunder. These observers had especially in mind the solution of two questions: (*a*) Is there spectroscopic evidence of an *atmosphere* on Mars? and (*b*) Is there spectroscopic evidence of *aqueous vapor* in the atmosphere of Mars?

* *Publications A. S. P.*, No. 37.

The observations made in 1862 in America by Rutherfurd* did not give an affirmative answer to these questions. The meagre results obtained seem to show that his instrumental equipment was not adapted to the solution of this problem.

The details of Janssen's observations have not been published, so far as I know, but his conclusions are expressed in a letter† read to the French Academy in 1867: "I cannot bring this letter to a close without telling you that I have ascended Mt. Etna for the purpose of making some spectroscopic observations which require a great altitude in order to annul the greater part of the influence of our atmosphere. From these observations and from those which I have made at the Observatories of Paris, Marseilles, and Palermo, I believe I can announce to you the presence of aqueous vapor in the atmospheres of Mars and Saturn."

In 1867 Huggins‡ in England observed lines in the spectrum of Mars "apparently coincident with groups of lines which make their appearance when the Sun's light traverses the lower strata of the [Earth's] atmosphere, and which are therefore supposed to be produced by the absorption of gases or vapors existing in our atmosphere. The lines in the spectrum of Mars probably indicate the existence of similar matter in the planet's atmosphere."

We have not the dates and details of Secchi's observations, but in 1872 he wrote§ that he considered his observations "proved the existence of a Martian atmosphere analogous to our own." However, there appears to be a reasonable doubt as to the sufficiency of his observations.

The most extensive observations on this subject are those made by Vogel in Germany in 1873. From his observations he considered that "it is definitely settled that Mars has an atmosphere whose composition does not differ appreciably from ours, and, especially, the Martian atmosphere must be rich in aqueous vapor."¶

Observations by Maunder‖ at Greenwich in 1877 confirmed in a general way those made by Vogel.

It is seen that the investigations of five eminent spectroscopists lead them substantially to the same result, viz.: *That Mars' atmosphere is similar to our own.* Their conclusion has been very

* *American Journal Science*, Jan. 1863.
† *Comptes rendus*, vol. LXIV, page 1304.
‡ *Monthly Notices Roy. Ast. Soc.*, vol. XXVII, page 179.
§ *Sugli Spettri prismatici di Corpi celeste*, Rome, 1872.
¶ *Untersuchungen ueber die Spectra der Planeten*, page 20.
‖ *Monthly Notices Roy. Ast. Soc.*, vol. XXXVIII, page 35.

generally accepted by astronomers. A careful examination the published data has shown me that some of the observ were made under circumstances *extremely unfavorable*, and between the different sets of observations there is not that agreement which one would like to see. While I believed Mars has an atmosphere and that it contains water vap seemed to me that a repetition of the spectroscopic observ under the very favorable circumstances existing here wo valuable.

Among the favorable circumstances we may mention:

(1) Improved spectroscopic apparatus. The observa mentioned above were made from seventeen to thirty years with spectroscopes comparatively crude.

(2) A telescope of great focal-length and correspondingly aperture. The telescopes used in the early observations small and short, so that the images of Mars formed by the the slit-plates would be less than one-third as large as that by the 36-inch equatorial. This is an enormous advantage, in estimating the relative intensities of spectral lines, and in paring the intensities of the centers of the lines (correspo to the center of Mars' disk) with the intensities of the e the same lines (corresponding to the limb of Mars).

(3) The altitude of the Observatory, which eliminates fro problem the absorptive effect of the lower 4200 feet o Earth's atmosphere, with all its impurities. Most of the o servations were made from near sea-level.

(4) The very dry summer air prevailing here. The av relative humidity is very low at Mt. Hamilton for the mon July and August. In many years it is less than 35 per There is no difficulty in selecting nights for observing the trum of Mars when our relative humidity is not more tha quite frequently it is less than 20. This is a very impo factor, since in examining Mars' spectrum for evidenc aqueous vapor it is very important, as Janssen pointed o 1867, that we eliminate as far as possible the effect of aq vapor in our own atmosphere. The observers do not see have taken this factor into account (except Janssen, the d of whose observations appear to be unpublished). By exam the contemporary weather data, I find that some of the obs tions were made when the relative humidity was 81, 85 and 90. All the principal published observations were made the average relative humidity at those seasons of the ye something like 80.

(5) The southern location of the Observatory and the north declination of Mars permit the observations to be made when the planet's altitude is as great as 59°. At an altitude of 59°, the light from Mars passes through 1.17 times as much atmosphere as it would if the planet were in the zenith. The most important of the published observations were made when the planet's altitude was only from 21° to 26°. That is, its light passed through from 2.75 to 2.28 times as much of our atmosphere as it would had the planet been in the zenith! While the observers sought to eliminate the effect of our atmosphere and its aqueous vapor by observing the lunar spectrum when the Moon was at the same altitudes, it must be evident that the Martian spectrum was observed at a tremendous disadvantage. One observation was made, for instance, when the altitude of Mars was only 24° and the relative humidity of our own atmosphere was 85. The effects of any possible Martian atmosphere would be pretty thoroughly drowned by the effects of the great thickness of our own atmosphere, nearly saturated with moisture.

(6) Finally, we may mention that our knowledge of the spectrum of our own atmosphere has been largely increased in the last few years. Thollon's excellent maps, for instance, are of great assistance in this problem.

With all these favorable circumstances, I expected that a confirmation of previous results would be a simple and easy matter.

We shall now state briefly the elements which enter into this problem.

We know by observation that the hemisphere of Mars which is turned toward the Sun is bright, and that the hemisphere which is opposite the Sun is dark. The planet, therefore, shines by reflected sunlight. The spectrum of Mars must be identical with that of the Sun, except as it is modified by the planet's (supposed) atmosphere.

The highly heated interior of the Sun, constituting its most dense portions, radiates light of all possible wave-lengths: that is, its spectrum is a strictly *continuous* band—not crossed by dark lines. The outer portions of the Sun are gaseous, of very much lower temperature than the inner portions, and made up of the vapors of the chemical elements contained in the Sun. These vapors, mostly those of hydrogen and the metals, constitute a sort of *solar atmosphere*. The light radiated from the hotter interior of the Sun does not pass freely through this surrounding atmosphere. It absorbs some of the rays of every wave-length (but more especially the blue and violet rays). This is called a

general absorption. It also selects light of *particular* wave-lengths and absorbs that light very strongly, producing the dark lines. The absorption which produces the dark lines is called *selective*, and the lines are called *metallic* lines. The solar spectrum consists of the *continuous spectrum* of the *Sun's interior* modified or interrupted by thousands of (dark) *metallic lines* caused by the *solar atmosphere*.

Our own atmosphere modifies the solar light which passes through it. It exercises both a *general* absorption, which weakens the continuous spectrum, and a *selective* absorption, which introduces at least 1200 additional dark lines. These dark lines —called *telluric* lines—constitute what we may term the spectrum of our atmosphere.

If the planet Mars is surrounded by an atmosphere, it no doubt exercises an absorption upon the solar light which enters it. The rays of light coming to us from the planet originate in the Sun; they pass once through the solar atmosphere; they enter Mars' atmosphere, are reflected partly by the planet's surface and partly by the inner strata of its atmosphere, and then pass out through its atmosphere; and they finally reach us by passing once through our atmosphere. The spectrum of Mars is, therefore, the combined spectrum of the solar, Martian and terrestrial atmospheres. If it *has* no appreciable atmosphere, the spectrum of the planet is simply the combined spectrum of the solar and terrestrial atmospheres.

The problem before us would be practically insoluble if we did not have a convenient means of eliminating the solar and terrestrial spectra, and leaving only *the Martian spectrum*. Our Moon has no appreciable atmosphere. Consequently, its spectrum is the combined solar and terrestrial spectrum. If we compare the spectra of Mars and the Moon when these bodies are at the same altitude above our horizon,—that is when their light traverses the same thickness of terrestrial atmosphere,—and find that they differ in any respect, however slight, such difference must be caused by an atmosphere on Mars. If no difference is found to exist then the spectroscope affords no evidence of such an atmosphere. Thus the problem resolves itself into a comparison of the Martian and lunar spectra.

Thollon has found that in the combined solar and terrestrial spectrum three very strong groups of lines are produced by some of the *constant* elements of our atmosphere, probably by the oxygen. These are the Fraunhofer groups A, B and α, comprising about 130 separate lines. The presence of these lines indicates

the presence of *atmosphere*. If they are *stronger* in the Martian spectrum than in the lunar spectrum, that planet must have an atmosphere.

Thollon* found other groups of lines, comprising at least 1100 separate lines, produced by the *aqueous vapor* in our air. They have been divided by Thollon into the following seven groups:

(1)	Wave-lengths		745	to	716	(Fraunhofer's a)
(2)	"	"	716	"	687	(below B)
(3)	"	"	660	"	646	(around Hα)
(4)	"	"	635	"	628	(near α)
(5)	"	"	597	"	585	(around D)
(6)	"	"	578	"	567	(Brewster's δ)
(7)	"	"	548	"	542	

The presence of these groups of lines indicates the presence of *aqueous vapor*. If they are *stronger* in the Martian spectrum than in the lunar spectrum, there is *aqueous vapor* in the atmosphere of Mars.

Now while all these lines can be observed *individually* in the solar spectrum, owing to the high dispersion which can be used, they can only be observed as groups or bands in the Martian and lunar spectra, on account of the faintness of those spectra and the low dispersion which must be employed.

It is impracticable to observe the groups A, 745-716 and 716-687, which are at the extreme red end of the spectrum, and they will not be further considered. The atmospheric bands B and α are easy to observe in both spectra. The vapor groups of lines require great care in observing, for the reason that, owing to the low dispersion which must be used, the individual lines are not only blended with each other but also with the solar *metallic* lines which lie among them. In the 7th group, for instance, the vapor lines are so much fainter than the neighboring metallic lines that we need not consider that band in the present problem. For the same reasons the 6th group, 578-567, is not a sufficiently sensitive test for aqueous vapor, except in the Earth's atmosphere when the body observed is near the horizon. However, the region of the 6th group was carefully observed in the Martian and lunar spectra on several nights. The 4th group, 635-628, is useless as a test for aqueous vapor, since the faint lines composing it are always overwhelmed by the prominent lines in the atmospheric group α. Only the 3d and 5th groups remain available.

* It must not be considered that the credit of this work is due wholly to Thollon. Many observers, Brewster, Gladstone, Janssen and others, investigated along the same lines. But Thollon's work is most complete and his maps are especially convenient and useful.

Of the 3d group I have not found useful that portion of it between 660 and 653, on account of the presence of the very heavy $H\alpha$ solar lines and other solar lines among the relatively faint vapor lines. I have for my own use divided the rest of the 3d group into three parts, each of which was found useful. The first covers wave-lengths 6515-20 and includes about eight tolerably strong lines, the majority of which are vapor lines. Under all of the dispersions used it was simply a very narrow band or line, which I shall call c'. The second part covers the region 6491-6500. It includes half-a-dozen strong metallic lines and a few strong vapor lines, all, however, blending to form a very strong, narrow band or line, which we shall call c''. The third part is included between 6463 and 6490, which contains a great many vapor lines and a few metallic lines. It formed a band of good width which we shall call c'''.

The 5th group, extending from 597 to 585, I divided into four parts. The first covers wave-lengths 5941-5959; it contains a number of strong aqueous lines and several metallic lines, forming a band which I called d'. The second covers 5928-35; it is strong in neither metallic nor vapor lines. It forms a narrow band which I called d''. The third portion covers 5912-25; it contains a few metallic lines and very many strong vapor lines; I called this region d'''. The fourth covers 5884-5906; it contains the two very strong solar lines D_1 and D_2, several faint solar lines, and a great many vapor lines. It would be a very useful band if the D lines were not contained in it; but I found their presence very troublesome. Let us call this region d^{iv}.

For the reasons given above I confined my observations almost wholly to the groups B, a, c', c'', c''', d', d'', d''' and d^{iv}. Of these I found that a, c', c''', d'', d''' were best suited for observation.

I observed the spectrum of Mars on ten nights between June 29 and August 10 of the present year, paying special attention to the nine critical groups of lines just mentioned. On eight of the nights I compared its spectrum with that of the Moon, when these two bodies were at equal distances above the horizon. On two nights, July 24 and 25, when the Moon was near the planet, I turned repeatedly from one spectrum to the other, while, on the former night, the planet passed from altitude 18° to 50°, and on the latter night, from altitude 45° to 55°. The two spectra have been compared when the relative humidity of our atmosphere was only 15 and when it was as high as 55. The observations were made principally with a dense 60° flint prism, with magnifying powers of 13 and 7, and occasionally with a 30°

prism and power 13. When the lunar spectrum was examined, the slit of the spectroscope was always shortened so that the lunar spectrum was of the same width as the Martian spectrum. The slit was directed always upon the brightest region of the Moon in order that the two spectra should be nearly of the same brightness, which is a very important condition in making reliable comparisons. In a word, the spectra have been compared under a variety of conditions, but with the conditions for the two bodies always identical. The atmospheric and aqueous vapor lines have been seen in both Mars and the Moon decreasing in intensity as these objects got higher and higher in the sky, and the aqueous vapor lines varying in intensity with the amount of moisture in our atmosphere. At all times the spectrum of Mars has appeared to be identical with that of the Moon in every respect.

Further, on several occasions when the planet's altitude was large, I examined the critical groups of lines, especially α, to determine whether the ends of the lines, which correspond to the limb of the planet, were stronger than their centers, which correspond to the center of the disk. The lines appeared to be of uniform intensity throughout, so far as the different intensities of different portions of the surface permitted a safe estimate to be made.

The intensity of the critical bands, α, for instance, was appreciably greater when the Moon and Mars were only 30° above the horizon than when they were 55°. The relative thicknesses of our atmosphere traversed by the rays when the bodies were at altitudes of 30° and 55° were as 2 to 1.22. If the rays of light from one of the bodies, Mars for instance, pass through a unit thickness of our atmosphere, and the rays from the Moon pass through 1½ units, the intensity of α in the spectrum of the latter is certainly greater than in the spectrum of the former. In fact, I am quite confident that a difference of 25 per cent in the lengths of paths traversed by the rays from the two bodies would cause an appreciable difference in the intensities of their α bands. The accuracy of the observation is greatly increased by the presence of several neighboring metallic lines which can be used as standards of comparison.

The results of these observations are as follows:

First.—The spectra of Mars and our Moon, observed under favorable and identical circumstances, seem to be identical in every respect. The *atmospheric* and *aqueous vapor* bands which were observed in both spectra appear to be produced wholly by

the elements of the Earth's atmosphere. The observations therefore, furnish no evidence whatever of a Martian atmosphere containing aqueous vapor.

Second.—The observations do not prove that Mars has *no* atmosphere similar to our own; but they set a superior limit to the *extent* of such an atmosphere. Sunlight coming to us *via* Mars would pass twice either partially or completely through his atmosphere. If an increase of 25 to 50 per cent in the thickness of our own atmosphere produces an appreciable effect, a possible Martian atmosphere one-fourth as extensive as our own ought to be detected by the method employed

Third.—If Mars has an atmosphere of appreciable extent, its absorptive effect should be noticeable *especially* at the limb of the planet. My observations do not show an increased absorption at the limb. This portion of the investigation greatly strengthens the view that Mars does not have an *extensive* atmosphere.

While I believe that the polar caps on Mars are conclusive evidence of an atmosphere and aqueous vapor, I do not consider that they exist in sufficient quantity to be detected by the spectroscope. This view has an important bearing upon the questions relating to the low albedo of the planet, and the well known brightness of its limb, in both of which respects the planet resembles our Moon.

MOUNT HAMILTON, 1894, August 14.

ON THE LINE SPECTRUM OF OXYGEN.*

B. HASSELBERG.

Under the above title Herr Eisig has recently published in the *Annalen* the results of his researches on the subject to which reference is made. The object of the researches was to photograph all the lines of pure oxygen, and to determine their wave-lengths with the greatest possible precision, without reference to earlier results To attain this end, the rarified gas was enclosed in Geissler tubes, its spectrum was photographed on plates 50 cm long with the aid of the large Rowland concave grating of Messrs. Kayser and Runge, and by measurement with a dividing engine the lines were referred to the lines of an iron spectrum photographed on the same plate. The wave-lengths obtained were

* Translated from the *Annalen der Physik und Chemie*, No. 8, 1894. An abstract of the observations reviewed by Professor Hasselberg is given in ASTRONOMY AND ASTRO-PHYSICS, June, 1894, p. 505

therefore according to the system of Rowland, which Kayser and Runge made the basis of their determination of wave-lengths in the spectrum of iron. Considering the great sharpness which is afforded by the photographic method, particularly with the aid of Rowland's gratings, one would have good reason to expect that the wave-lengths of the lines of the gas would be obtained with an exactness nearly equal to that of the metallic lines photographed with the same apparatus. This, however, seems to be by no means the case; for the author himself states that, although the measurements were made to hundredths of a tenth-meter, the results for different plates sometimes varied by as much as 0.3 tenth-meter. As the author points out, the cause of this is to be sought in displacements, or changes of temperature of the apparatus during the long exposure of from two to four hours; and one can only agree with him when he rounds off the definitive means to the nearest tenth of a tenth-meter, and estimates the possible uncertainty at from one to two tenths.

Under these circumstances the question may well be raised, whether our knowledge of this spectrum has really been appreciably widened by the investigation referred to. I believe that it has not. It is true that the determinations of the author, as compared with those of Schuster, Deslandres, Hartley and Adeney, must be allowed to have a certain superiority with respect to completeness, and perhaps also to accuracy, but they are hardly superior to those of Trowbridge and Hutchins, which were carried out with the same means, but according to a method less liable to systematic errors. The researches last mentioned, although not entirely free from objection in certain details, are in the main to be regarded as quite satisfactory.

Besides the investigations mentioned above, are some others,* unknown to the author, which were made in the physical institute of this place in 1891, and in which, with far simpler means, a degree of accuracy was reached at least equal to that of the results under consideration. This investigation, the principal object of which was to separate the lines of nitrogen as completely as possible from those of oxygen in the spectrum of air, was carried out with the aid of a spectrograph constructed of two large Steinheil prisms of flint glass and a pair of telescopes, of which the one serving as camera had a focal length of four feet. With the aid of this apparatus the spectrum was photographed with that of the Sun on plates whose dimensions were 7.5 by 3.5 centimeters. The wave-lengths were determined without micro-

* Neovius, *Bihang til K. Svenska Vet. Akad. Handlingar* Bd. 17. Afd. I. Nr. 8.

metric measurement, by comparison with Rowland's map. Comparing the results of Neovius with those of the author, we have the following table:

Eisig.		Neovius.		Eisig.		Neovius.		Eisig		Neovius	
λ	I	λ	I	λ	I	λ	I	λ	I	λ	I
3712.8	4	3712.9	3	4111.2	6	4111.0	3	4369.7	6	4369.7	4
27.5	3	27.4	2	12.4	5	12.2	3	96.4	5	96.3	4.3
49.6	2	49.8	1	14.2	6	14.0	4	4415.3	1	15.0	1
54.7	6	54.7	4	19.5	2	19.3	2	17.4	2	17.2	1
57.3	6	57.1	5	20.5	4	20.4	3	43.6	6	43.0	4.3
60.0	6	59.9	4	21.7	6	21.6	3	48.7	6	48.2	4.3
3851.2	6	51.3	3	33.2	5	33.0	2	52.8	5	52.4	4.3
57.4	6	57.5	4	42.4	6	42.3	3.4	65.8	6	65.3	3.4
64.8	5	64.9	3.2	44.0	6	43.6	3.4	66.7	6	—	—
82.5	3	82.7	2.3	46.3	5	46.1	3	68.4	6	67.8	4
3907.6	6	07.7	4.3	53.7	3	53.6	1	69.9	6	69.4	4.3
12.3	3	12.3	2.1	56.8	6	56.7	3	4591.4	3	91.0	2
19.6	4	19.5	3.2	69.5	6	69.5	3.2	95.5	4	96.3	3.4
45.3	4	45.3	3.2	85.8	4	85.5	1	4639.2	4	38.9	3.4
54.5	3	54.7	1.2	90.0	4	90.0	1	42.1	2	41.9	3.2
73.4	1	73.5	1	4317.4	3	17.1	2.1	49.5	1	49.2	2.3
83.0	4	83.0	3.2	19.9	3	19.6	2.1	51.2	4	51.0	4.3
4070.1	1	70.1	2	26.2	6	25.8	3.4	62.0	4	61.6	3.2
72.5	1	72.4	2	27.8	6	27.5	4.3	74.2	6	73.5	5
76.2	1	76.2	1	29.0	6	28.4	4	76.6	5	76.6	3
79.1	5	79.0	3.4	32.2	6	32.0	5	96.8	6	96.0	4.5
85.5	4	85.2	3	37.3	4	37.0	4.3	99.6	4	99.4	3
93.2	5	93.0	3	45.9	2	45.4	3.2	4701.5	6	—	—
[illegible]	6	[illegible]	1	47.8	3	47.5	[illegible]	[illegible]		—	
[illegible]	4	—		[illegible]	1	[illegible]	2	[illegible]	1	[illegible]	
[illegible]	6	[illegible]	[illegible]	51.7	[illegible]	[illegible]	3	[illegible]	3	[illegible]	
[illegible]	4	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]				

The wave-lengths given above are the means of two series of determinations, made respectively with copper and aluminium electrodes. In general, the probable error of Neovius' results may be estimated at about ± 0.1 tenth-meter, and the accuracy may be regarded as the same in both cases.

If now the two columns are compared, it will be seen that the values of Eisig are almost invariably a little greater than those of Neovius,—on the average by about 0.2 tenth-meter,—and also that the differences increase with the wave-length. Leaving out of consideration this systematic difference, which under the circumstances is of comparatively small importance, we may regard the agreement as quite satisfactory, and consider the two columns to be of equal weight. The series of Neovius is however really somewhat the more complete, provided that several weak lines, not given in the table, belong without doubt to oxygen

From the above comparison it seems to me that the work of the author has not added much to our knowledge of the subject, unless it be the experience that for the investigation of faint spectra, such as those of the gases, the large grating spectroscope has no appreciable advantage over simpler apparatus

ON THE ASTIGMATISM OF ROWLAND'S CONCAVE GRATINGS.

DR. J. L. SIRKS, GRONINGEN.

In a well-known paper of Mr. J S. Ames, On Concave Gratings for Optical Purposes*, the following passage occurs. "Owing to "the astigmatism of the grating, it is not possible to adopt the "usual method of illuminating part of the slit with the solar image and part with the spark or arc; and so a different and far "better plan is adopted. A compound photograph of the two "spectra is taken in the following manner."

Yet this new plan, devised and executed by Professor Rowland with his wonted success, is only applicable by means of photography, as the photographs of the different spectra must be taken one after another; and if the precited statement,—which, so far as I see, neither Mr. Ames nor Professor Rowland, at whose request he wrote, has recalled or modified—were to be accepted in its apparent purport, the beautiful instrument with which Mr. Rowland has endowed the spectroscopist would be unfit for the direct comparison of spectra from different sources by ocular observation, that was always regarded as a precious function of the dioptric spectroscope.

Fortunately however, though in the literal acceptation of the words it is useless to illuminate *part* of the slit with one source of light and *part* with another, it is certainly possible to institute the intended comparison, at least with the first and second spectra, by a slight modification of the common method: the prisms or other equivalent contrivances that are generally used to introduce lateral beams of light, need only be placed *not against the slit*, in A (Fig. 1), *but at a distance* $\rho \sec \nu - \rho \cos \nu$ *from the slit*, $\rho \sec \nu$ from the grating, viz., at a point Q, being the intersection of BA and the tangent in the focus C.

In order to demonstrate the truth of this assertion let us consider the pencil of monochromatic rays that will, after the reflec-

* *Phil. Mag.* XXVII, p. 351, 1889, cf. ASTR. AND ASTRO-PH. 1892, p. 39 *Verhand Kon. Akad. v. Wetensch.* (1e Sectie). Dl. II.

tion at the grating, concur in the focus at C. It may be divided into what I may be allowed to call vertical "fans" of rays, each of them being limited by two vertical planes passing through the slit and including an infinitesimally narrow strip of the grating. Now all the rays contained in such a "fan", in order to concur at C without any difference of path, must issue from an apex situated in the line CQ, being the axis of the spherical surface, part of whose equatorial region is occupied by the grating.

On the other hand the horizontal fans of rays into which the pencil may be divided, by theory of diffraction have their apices in the slit. So *all* the rays that concur at C must have passed successively through two caustics: the one realized by the slit, the other only virtual, lying along the line GH, where it may be realized by another slit, if the source of light be placed at a sufficient distance. It will be easily seen that the length of the first caustic, the available part of the slit, is $b \times QA : QB = b \sin^2 r$, that of the second $GH = a \times QA : BA = a\,tg^2 r$, a and b being the horizontal and vertical dimensions of the grating.

The existence of the second caustic, that is of great importance for the complete theory of the instrument, may be very simply demonstrated *ad oculos* by stretching a thin wire in Q along GH across an incident beam of sunlight: the result is a *perfectly defined* narrow black band passing horizontally across the field of the eyepiece. Any other horizontal strip of the field has its own conjugate horizontal strip, of a somewhat greater width, in the proportion $\rho \sec r : \rho$, a little above or below Q in a vertical plane passing through GH. Yet every single *point* in the strip of the field, belonging to one single λ, derives from the conjegate horizontal caustic GH in its full *length*; conversely every *point* of a horizontal slit above or below GH has its horizontal *linear* image in the field depicted by rays of different λ's.

If the horizontal band, seen in the field, is required to have a width h, the horizontal slit in GH must be replaced by a rectangular diaphragm height $h \times QB : BC = h \sec r$, length as before $GH = a\,tg^2 r$. At the same time the vertical slit ought to be lengthened by the quantity $h \cos r$, until it gives passage to all the rays issuing from the diaphragm that can reach the grating, so the full length becomes $h \cos r + b \sin^2 r$. All the rays that are obstructed by the diaphragm, if admitted would only tend to increase the disadvantageous illumination of the field by scattered light.

Any incident ray passing through the diaphragm *over* (*under*) the line CQ and through the slit will come at a focus in the *lower*

Fig. 1.

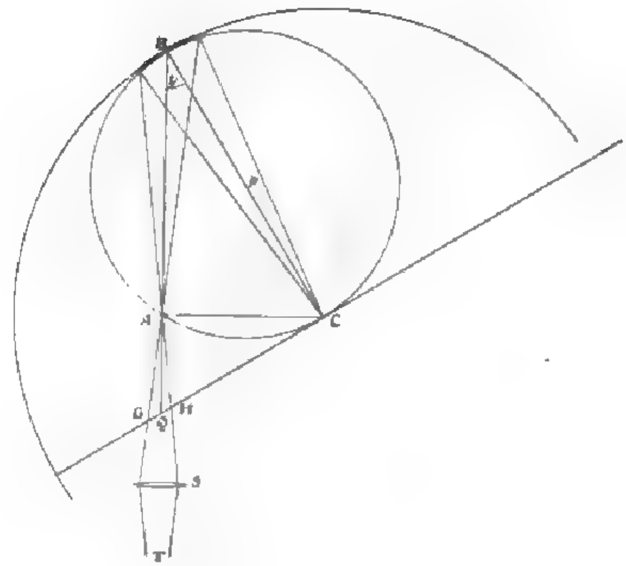
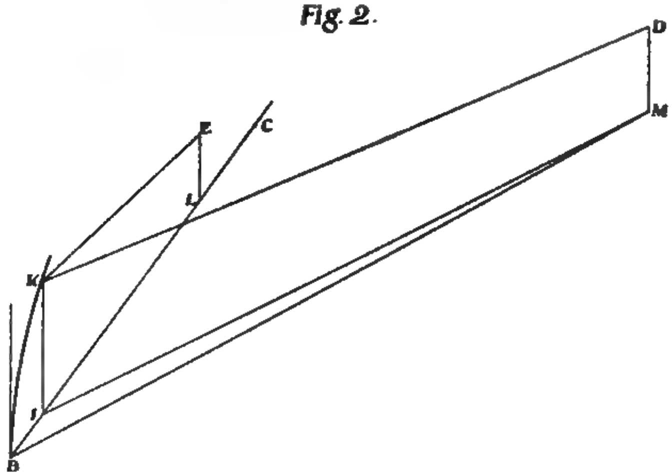


Fig. 2.



J. L. Sirks' Astigmatism of Rowland's Concave Gratings.

(upper) half of the field. A short but rather broad prism, 2 or 3 mm. in height, placed at Q and reflecting lateral solar light will give a narrow solar spectrum with perfectly defined edges passing through the center of the field; at the same time it will obstruct *none* of such rays, emanating from a sodium flame or arc light placed somewhere about T, as may concur in forming a sodium or metal spectrum in the remaining part of the field. Of course if we wish to get the metal spectrum as bright as possible, the cone of light furnished by the condensing lens S must be wide enough to fill up the wedge formed by the rectangular diaphragm and the slit.

With the third and ulterior spectra and with a very large grating the condensing lens should be of rather great dimensions, so I think the method will only be quite applicable with the first and second spectra. I may add that probably the very best plan would be to have a bicylindrical lens, or two cylindrical lenses put crosswise, of such a curvature that both its orthogonal caustics might coincide with the above named caustics of the grating, but every different angle r or at least every successive spectrum would require its especial lens.

Through the kind permission and efficacious assistance of Professor Haga I have been able to control the above by a provisional experiment. A narrow central band of the field on a black ground showed the first sodium-spectrum originating from a strip of mirror-glass, height 2.5 mm., placed along the caustic at Q, at 171 mm. from the slit, and upon which the light of a lateral Bunsen-flame was concentrated through a lens, $f = 150$ mm. The strip of glass just arrested the superfluous central part of a direct beam of sunlight that filled out the upper and lower parts of the field with its spectrum. The sunlight had to be passed through several layers of wire-gauze in order to bring down its intensity to that of the reflected sodium light. Now in the compound spectrum the two positive sodium-lines ended abruptly where the negative sodium-lines began; yet two very narrow sharp black lines, about 0.1 mm. wide, separated the three contiguous spectral bands: this was occasioned by the strip of glass having been simply cut with a diamond without any ulterior grinding or polishing; so the somewhat rugged edges, while they were unable to take part in the reflection of the sodium-flame only acted as a barrier against the sunlight grazing them.

In order to try to what limit, if need be, the method can be applied, we turned the moveable girder of the spectroscope on to the last or fourth spectrum with $r = 68°$, $\sin r = 0.928$. A knit-

ting needle held in the horizontal caustic, that now lay at 714 cm. from the slit, was accurately represented by a narrow black line across the solar spectrum. This proves that the definition in the images of horizontal lines, produced by the vertical fans holds good even at this great angle of incidence.

I still may remark that the whole action of the hollow grating, with a radius ρ, may *for these fans* be regarded as the result of three successive operations: one being that of a first concave mirror, with a radius 2ρ, but reduced by astigmatism to a radius $2\rho \sec \nu$, that brings the incident rays to parallelism; the second that of a plane grating, which occasions the diffraction at an angle ν; the third that of another concave mirror 2ρ, which makes the diffracted parallel rays converge into a focus. The distances and dimensions of two conjugate images may be simply calculated by the formulæ for one mirror with $f = \rho/(1 + \cos \nu)$, as may be proved in the following manner.

Let BK (Fig. 2) be part of a very narrow vertical strip, and B the center of the mirror, C the center of curvature; D and E two conjugate foci determined by their height $z_1 = \mathrm{DM}$, $z_2 = \mathrm{EL}$ over the horizontal plane LBM, by $\mathrm{BM} = \mathrm{R}$, $\mathrm{BL} = r$ and angle $\mathrm{MBL} = \nu$; ρ being the radius BC of the sphere, $\mathrm{KI} = l$.

Now with a sufficient degree of approximation we successively find

$$\mathrm{BI} = \frac{l^2}{2\rho}, \quad \mathrm{IM} = \mathrm{R} - \frac{l^2 \cos \nu}{2\rho},$$

$$\mathrm{KD}^2 = \mathrm{IM}^2 + (l - z_1)^2 = \mathrm{R}^2 - \frac{\mathrm{R} l^2 \cos \nu}{\rho} + (l - z_1)^2$$

$$\mathrm{KD} = \mathrm{R} - \frac{l^2 \cos \nu}{2\rho} + \frac{l^2 - 2lz_1 + z_1^2}{2\mathrm{R}};$$

consequently

$$\mathrm{KE} = r - \frac{l^2}{2\rho} + \frac{l^2 - 2lz_2 + z_2^2}{2r}.$$

For the point B, $l = 0$, we have

$$-\mathrm{BD} = -\mathrm{R} \qquad -\frac{z_1^2}{2\mathrm{R}},$$

$$-\mathrm{BE} = -r \qquad -\frac{z_2^2}{2r}.$$

Hence by addition we find for the difference $\varDelta$ of the two paths DKE — DBE

$$\varDelta = l^2\left(-\frac{1 + \cos \nu}{2\rho} + \frac{1}{2\mathrm{R}} + \frac{1}{2r}\right) - l\left(\frac{z_1}{\mathrm{R}} + \frac{z_2}{r}\right).$$

Now if indeed D and E be conjugate foci, Δ must vanish for every value of l, and both the factors included in brackets must be $= 0$. So the first factor gives for the relation of the distances

$$\frac{1}{R} + \frac{1}{r} = \frac{1 + \cos \nu}{\rho}$$

as with a mirror of ρ $(1 + \cos \nu)$ focus; the second makes

$$\frac{z_1}{z} = -\frac{R}{r}$$

so that the heights of the images are proportional to the distances as in common optics.

I think I have shown that the astigmatism of the grating, while securing to the instrument some precious qualities, is no impediment against a method of observation that seems to be reputed incompatible with astigmatism. On the other hand the valued quality of the concave grating, that it shows no dust-lines, and that the image of a star or a spark on the slit is broadened out into a band, may be imparted to a dioptic spectroscope by giving a slight convex spherical curvature to one side of one of the prisms, so that the instrument becomes slightly astigmatic.

Dec. 28th, 1893.

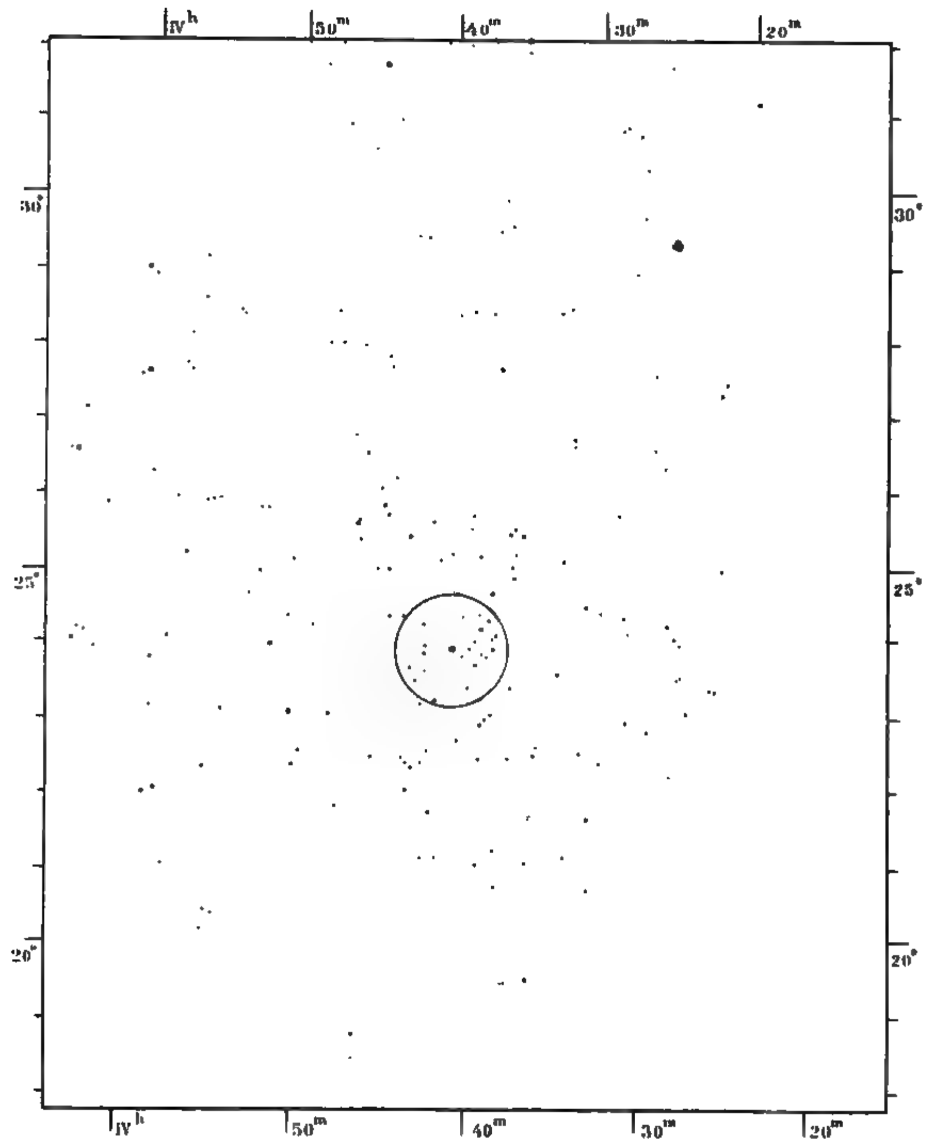
ON THE EXTERIOR NEBULOSITIES OF THE PLEIADES.

E. E. BARNARD.

For many years during my comet seeking I have known of a vast and extensive but very diffused nebulosity north of the Pleiades. Other masses of this diffused matter make their presence known by a general dulling of the field when sweeping in the region of the cluster.

As is well known the immediate group of the Pleiades is filled with nebulosity which in general attaches itself to the various stars and is of a wispy and streaky nature. This is well shown on the photographs made by the Henry Brothers, by Roberts, Wilson and others. In these pictures, however, the nebulosity seems limited to a small area included in the cluster itself. The principal and best known of these nebulosities is the one discovered by Tempel in 1859 and which envelopes Merope and extends southwesterly from that star. The other nebulosities have revealed themselves through the aid of photography in the past ten years.

PLATE XXXIII.



Distant Nebulosities Around the Pleiades.

This cluster and its nebulosities have been more thoroughly studied visually and photographically than any other group of stars in the sky. Consequently anything new about it is of the highest interest.

It has been my hope during the past two or three years to some time be able to secure a photographic impression of these vague nebulosities that I had seen in the telescope. It was evident this would require a long exposure. The mounting of our Willard lens does not permit an exposure to be carried beyond the meridian. To get sufficient time would therefore require more than one night.

This past winter I have been able by carefully inclosing the camera box in thick black cloth and by taking other precautions, to extend the exposure through two nights with success.

Previous to this I gave an exposure on the Pleiades of four hours, which showed all the well known nebulosities, and gave faint suggestions of more distant wisps of nebulæ.

December 6, 1893, an exposure was begun which was continued for five hours. The lens was then carefully covered without disturbing the plate. The next night was cloudy but on December 8 the exposure was continued for five hours and fifteen minutes—making thus a total of $10^h\ 15^m$.

The resulting picture confirmed the first photograph and showed a number of singular curved and streaky nebulosities apparently connected with the Pleiades and extending all about the group.

Some of these streams extend irregularly for several degrees each side of the cluster—especially towards the east.

To give an idea of the affected regions, I have made the inclosed drawing from the photograph which will explain itself. On this I have drawn a circle about the Pleiades inside of which all the nebulosity shown in previous photographs has been confined.

I have not attempted to sketch the nebulosities connected immediately with the stars of the cluster, and shown on the photograph, for these are already well known to everybody.

For the more ready location of these outer nebulosities I have, very roughly, put a set of coördinates around the edge of the drawing.

To the north of the Pleiades, from $\alpha = 3^h\ 20^m$ to 4 hours and beyond, and from $\delta = +30°$ to several degrees further north, is a region singularly devoid of small stars but covered with large masses of very diffuse nebulosity; this part of the sky will attract the attention of any one in sweeping over it with a very low

power on an ordinary telescope. The field is dull with feeble nebulosity. This region is partly shown on the northern part of the plate and the nebulosity is evident. This is about and west of the stars ζ and o Persei.

During the coming winter I hope to be able to further extend the exposure time for the delineation of other and fainter nebulosities, in the region of the Pleiades, that are too vaguely shown on the present plate to make much out of.—*Astronomische Nachrichten*, No. 3253.

MOUNT HAMILTON, 1894, July 25.

ASTRO-PHYSICAL NOTES.

All articles and correspondence relating to spectroscopy and other subjects properly included in ASTRO-PHYSICS, should be addressed to George E. Hale, Kenwood Observatory of the University of Chicago, Chicago, U. S. A. Authors of papers are requested to refer to last page for information in regard to illustrations, reprint copies, etc.

A Large Telescope for the Cape Observatory.—We learn from the October *Observatory* that Mr. Frank McClean has offered to present to the Royal Observatory of the Cape of Good Hope a large equatorial for photographic and spectroscopic work. The photographic objective will have an aperture of 24 inches, and an objective prism of 7½° refracting angle and 24 inches aperture is to be provided. The visual telescope which is to be mounted with the photographic instrument, will be of 18 inches aperture. A slit spectroscope is also to be provided, for work on the radial motion of stars. The construction of the instrument is already well under way in the workshops of Sir Howard Grubb.

The importance of this splendid gift is greatly increased by the fact that it is intended for use in the southern hemisphere, where the opportunities for astro-physical research are practically unlimited. Of Mr. McClean's excellent spectroscopic work at Tunbridge Wells we have frequently had occasion to speak in this journal. He is to be congratulated for his broad-minded generosity and scientific discrimination in making this munificent gift.

Professor Hartley's Observation of the C Line of Hydrogen in the Bessemer Flame.—Many attempts have been made to produce the spectrum of hydrogen by means other than electrical, but if a single observation by Watts of the C line in a Bessemer flame be excepted, no certain proof is offered that success has been attained. Professor W. N. Hartley has been engaged for some time upon an exhaustive study of flame spectra at high temperatures, and several papers dealing with these investigations have appeared in the *Philosophical Transactions* and the *Proceedings* of the Royal Society. The last number of the *Proceedings* (Vol. LVI, No. 337) contains an interesting paper on the spectrum of the Bessemer flame. About ninety spectra were photographed, the region studied extending from λ 7697 to about λ 3380.8. The least refrangible line photographed was that of lithium at λ 6707. About ninety-two lines were identified with lines in the solar spectrum, with lines in Kayser and Runge's photographic map of the

iron spectrum, and with lines in the oxyhydrogen flame spectra of steel and ferric oxide. In the flame issuing from the mouth of the converter during the first period of the "blow" the lines of the alkali metals sodium, potassium and lithium were seen unreversed on a bright continuous spectrum caused by carbon monoxide. "The C line of hydrogen and apparently the F line were seen reversed during a snow storm." No special attention appears to have been given to this observation, but there seems to be no doubt in the mind of the author that the C line was actually seen. The paper is unfortunately an abstract, and the dispersion employed is not stated, though the context seems to indicate that it was high enough to leave little doubt, if the identification was carefully made. It thus appears that Watts' observation, which was made in rainy weather, is confirmed. If this is the case a result of some importance in astro-physics has been obtained. It was once supposed that the H and K lines of calcium could be produced only by electrical means, but they were finally discovered in the flame of burning magnesium ribbon which contained calcium as an impurity. In the case of hydrogen it has been argued that solar prominences are electrical phenomena, because they show the lines of this gas, which are obtained in the laboratory only by electrical means. If Professor Hartley's observation is substantiated this argument must fall to the ground. Whether the chemical action involved in the present case is necessary to the production of the hydrogen lines is a separate question, about which there will be much difference of opinion.

Note on the Spectrum of Mars.—Professor Campbell in his paper on the Spectrum of Mars (Publ. A. S. of the Pacific, Vol. VI p. 228)* in speaking of the early observations of myself and others, says:—

"It is very important, as Janssen pointed out in 1867, that we eliminate as "far as possible the effect of aqueous vapour in our own atmosphere. The observers "do not seem to have taken this factor into account (except Janssen, the details "of whose observations appear to be unpublished.)"

So far is this statement from being correct that the method of eliminating the effect of our atmosphere by observations of the Moon at the same time was that originally employed by me in 1867. In speaking of faint lines seen on both sides of D and b which appeared to indicate terrestrial gases or vapours in the atmosphere of Mars, I say expressly:—

"That these lines were not produced by the portion of the Earth's atmosphere "through which the light of Mars had passed, was shown by the absence of similar lines in the spectrum of the Moon which at the time of observation had a "smaller altitude than Mars."—(*M. N. Roy. Ast. So.*, Vol. XXVII p. 178.)

In 1879 I took photographs of the spectra of Mars and some other planets in the twilight, simultaneously with spectra of the light from the sky immediately about the planets. In these spectra extending from *b* to S in the ultra-violet, no lines, or modifications of the spectrum appear, which are peculiar to the planet's spectrum.

The apparatus necessarily employed by me at the early date of 1862-7 was very imperfect as compared with the instruments now in use, but I have no reason to doubt the substantial accuracy of the observations which were made with much care.

WILLIAM HUGGINS.

Upper Tulse Hill, October 6th, 1894.

* See page——

The Advantage of the Short-Focus Camera in Spectrum Photography.—The notes by Dr. Huggins and Professor Campbell (ASTRONOMY AND ASTRO-PHYSICS, No. 127, p. 568, and No. 128, p. 696), on the advantage of short-focus camera in photographic work with the astronomical spectroscope, seem to leave some points not quite clear, and a few further comments on this subject may not be out of place. The advantage of the short camera in ordinary spectrum photography is well known (it is stated for instance on page 374 of the *Encyclopedia Brittannica*), and it applies equally to the spectroscope used in connection with a telescope when the source of light is an object of considerable angular magnitude. I do not see that the conditions on which it is based hold when the source of light is a star. The photographed spectrum must have a certain breadth, which is determined by the following considerations: it must subtend a sufficient angle when seen under the measuring microscope, which usually has a power of from ten to thirty diameters, and it must be so large that accidental combinations of silver grains, specks of dust, or blemishes on the plate may not be mistaken for lines. This minimum breadth is, according to my experience, about one quarter or one-third of a millimetre, and the experience of the Potsdam observers leads them to the same conclusion. As the breadth of the spectrum is obtained by allowing the star to travel along the slit, it is evident that if the camera is shortened the amount of drift given to the star must be proportionally increased, and hence the exposure must be longer. The same law does not hold, therefore, as in the case of a large object, when drift becomes unnecessary.

When the image on the slit-plate is kept stationary, its size determines the breadth of the spectrum. Hence the short camera might be advantageously used with a large telesocpe, and not perhaps, with a small one on the same object. It may also happen, in cases where exact measurement of the photographs is required, that the greater narrowness of the lines on the smaller image is not a sufficient compensation for the reduction of the scale. We may say, therefore, it seems to me, that the short camera may be used to advantage when the size of the image on the slit-plate, as determined by the size of the telescope and the angular magnitude of the object, or the general purpose of the investigation, allows us to sacrifice the scale of the photographs for the sake of obtaining intensity.

Some of these points were touched upon by Professor Campbell in his first paper on the spectrum of the Orion Nebula (ASTRONOMY AND ASTRO-PHYSICS, May, 1894). I desire more particularly to call attention to the facts that the case of a star is different from that of other objects, and that a large telescope has a great advantage over a small one in the number of cases to which the short camera is applicable.

J. E. KEELER.

The Dispersion of Fluorite and of Rock-Salt. In connection with his researches on the radiation of heat by gases, Dr. Paschen has made a study of the dispersion of the two substances mentioned above. The most remarkable result is that beyond a certain wave-length the dispersion begins to increase, *i. e.*, the curve $n = f(\lambda)$ is concave toward the axis of λ. The observations in the lower spectrum are not satisfied by Briot's formula with constants derived from observations in the upper spectrum, and Dr. Paschen gives a mathematical proof of the impossibility that they should be. It follows that all previous determinations of wave-lengths beyond 7μ are too great, as they are based on extrapolations which assume that the curve beyond this point is either straight or slightly convex. Dr. Paschen considers it doubtful whether any heating effect has ever been observed in the spectrum beyond 15μ.

A consideration of the relative dispersion and absorptive action of fluorite and rock-salt in different parts of the spectrum shows that a prism of fluorite is the more advantageous for wave-lengths above 8μ; for longer wave-lengths rock-salt should be used.

PLANET NOTES FOR DECEMBER.

H. C. WILSON

Mercury will be morning planet during December and may be seen toward the southeast between six and seven o'clock during the first half of the month. The best observations will be obtained, however, about ten or eleven o'clock when the planet is near the meridian.

We would remind those of our readers, who receive this number of ASTRONOMY AND ASTRO-PHYSICS before Nov. 10, of the transit of Mercury which is to occur on that date. The transit will begin at $9^h\ 55^m$ A. M. and end at $3^h\ 12^m$ P. M. Central time. For other data see last number.

Venus will be evening planet but will set too soon after the Sun to be seen.

Mars will be in excellent position for evening observation. He will be near the meridian between seven and eight, at a good altitude so that although this is a cold month, some good views ought to be obtained. Mars will be in conjunction with the Moon, about 2° south, Dec. 8 at noon.

The seeing at Northfield has not been of the best this year and we have but few really good views of Mars. Our best view was the night of Oct. 8, when the gulf Auroræ Sinus, longitude 55° was on the Martian meridian, (see Schiaparelli's map reproduced in our last number). The appearance of the planet was quite different in many respects from that indicated by the map. Solis Lacus was not round or oval, but was broader at the east end, agreeing almost exactly with the map drawn by Proctor years ago. The "canals" Ambrosia, Nectar, Tithonius, Fortuna, Chrysorrhoas and Ganges were seen, some of them in slightly different positions from those shown in the map. Fons Juventæ was also seen and a canal, not on the map, running northward from it.

Mare Erythraeum with the light regions Deucalionis Pyrrhæ and Protei appeared much as shown in the map but Mare Australe did not show dark at all. All the south polar region except the snow cap itself appeared of the same yellowish hue with the north equatorial regions.

The south polar cap was quite small and on Oct. 10 and 12 was still smaller, measuring only a small fraction of a second of arc. On the 16th it had entirely disappeared. Since that time to the present writing (Oct. 23) the snow-cap has been entirely invisible. We do not remember that such a total dissappearance has ever been recorded before, although the great diminution of the polar cap in the summer season has been a well known fact.

Jupiter may be observed during the whole night in December. He will be in conjunction with the Moon, 5° to the south of the latter on the morning of the 13th, and at opposition to the Sun on the 22d. The nights will be cold but a good view of Jupiter's belts well repays the observer for the little suffering that must be endured to get it.

Saturn and *Uranus* are morning planets and will probably not be observed much by the amateur in the cold weather. Saturn is at the feet of Virgo moving slowly eastward. Uranus is in Libra a little south of the star ν.

Neptune may be observed all night. He is in Taurus a little south of the star ι, and moving very slowly westward. Neptune will be at opposition Dec. 6.

Planet Tables for December.

[The times given are local time for Northfield. To obtain Standard Times for Places in approximately the same latitude, add the difference between Standard and Local Time if west of the Standard Meridian or subtract if east].

Date.	R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m
		MERCURY.			
Dec. 5	15 33.4	− 17 31	5 44 A. M.	10 35.6 A. M.	3 27 P. M.
15	16 33.1	− 21 33	6 23 "	10 55.6 "	3 28 "
25	17 38.7	− 24 08	7 02 "	11 21.9 "	3 42 "
		VENUS.			
Dec. 5	16 54.3	− 22 37	7 34 A. M.	11 56.3 A. M.	4 19 P. M.
15	17 49.0	− 23 51	7 55 "	12 11.4 P. M.	4 25 "
25	18 43.0	− 23 51	8 11 "	12 27.0 "	4 43 "
		MARS.			
Dec. 5	1 23.3	+ 9 10	1 44 P. M.	8 23.9 P. M.	3 04 A. M.
15	1 31.4	+ 10 18	1 08 "	7 52.6 "	2 37 "
25	1 42.9	+ 11 40	12 34 "	7 24.7 "	2 15 "
		JUPITER.			
Dec. 5	6 15.0	+ 23 09	5 32 P. M.	1 14.8 A. M.	8 58 A. M.
15	6 09.5	+ 23 12	4 46 "	12 29.9 "	8 13 "
25	6 03.6	+ 23 14	4 01 "	11 44.7 P. M.	7 28 "
		SATURN.			
Dec. 5	14 06.8	− 10 24	3 47 A. M.	9 9.6 A. M.	2 32 P. M.
15	14 10.6	− 10 42	3 13 "	8 34.0 "	1 55 "
25	14 14.0	− 10 58	2 39 "	7 58.2 "	1 18 "
		URANUS.			
Dec. 5	14 59.9	− 16 41	5 07 A. M.	10 02.1 A. M.	2 56 P. M.
15	15 02.2	− 16 51	4 31 "	9 25.0 "	2 20 "
25	15 04.2	− 17 00	3 54 "	8 47.7 "	1 42 "
		NEPTUNE.			
Dec. 5	4 53.1	+ 21 01	4 20 P. M.	11 52.9 P. M.	7 25 A. M.
15	4 51.9	+ 21 00	3 40 "	11 12.5 "	6 45
25	4 50.7	+ 20 58	3 00 "	10 32.0 "	6 04
		THE SUN.			
Dec. 5	16 49.0	− 22 27	7 21 A. M.	11 51.0 A. M.	4 21 P. M.
15	17 33.0	− 23 19	7 30 "	11 55.5 "	4 21
25	18 17.4	− 23 23	7 36 "	12 00.5 P. M.	4 24

Occultations Visible at Washington.

Date 1904.	Star's Name.	Magnitude.	IMMERSION. Washington M. T. h m	IMMERSION. Angle from N. Pt.	EMERSION. Washington M. T. h m	EMERSION. Angle from N. Pt.	Duration. h m
Dec. 5	B. A. C. 8184	6	8 21	76	9 39	207	1 18
7	70 Piscium	8	11 07	100	12 00	198	0 53
9	27 Arietis	6	3 49	40	4 43	260	0 54
10	9 Tauri	7	8 51	126	9 27	181	0 36
10	g Pleiadum	6	13 38	33	14 27	304	0 48
10	17 Tauri	4	13 25	71	14 35	262	1 10
10	23 Tauri	5	14 15	128	14 59	212	0 44
10	21 Tauri	8	14 38	96	15 38	245	1 00
10	η Tauri	3	14 42	99	15 41	243	0 59
10	B. A. C. 1171	8	15 14	57	16 08	287	0 54
10	27 Tauri	4	15 32	128	16 13	217	0 41
10	28 Tauri	6	15 28	107	16 21	238	0 53
13	47 Geminorum	6	17 16	85	18 11	311	0 55
16	31 Leonis	6	15 42	114	16 57	319	1 15

Jupiter's Satellites for December.

Phases of the Eclipses of the Satellites for an Inverting Telescope.

I. 

III. 

II. 

IV. 

No Eclipse.

Configurations at 11ʰ for an Inverting Telescope.

Day.	West.		East.
1	·3 2·	○	1· ·4
2	·2	○	·3 4· ·1●
3	1·	○	·2 ·3 4·
4		○	·12· 3·4·
5	2· 1·	○	3· 4·
6	3· 4· ·2	○	1·
7	3· 4· ·1	○	2·
8	4· ·3 2·	○	1·
9	4· ·2 ·1	○	·3●
10	○1· ·4	○	·2 ·3
11	·4	○	·1 2· 3·
12	·4 2· 1	○	3·
13	·4 3· ·2	○	·1
14	3· ·1	○ 4	·2
15	3 2	○	1· ·4
16	·2 ·1	○	4 ·3●
17	○1·	○	·2 3· ·4
18		○	·1 2· ·3 ·4
19	2 ·1	○	3· 4·
20	3··2	○	·1 4·
21	3· 1·	○	·2 4·
22	○2· ·3	○	4. 1·
23	·24· ·1 ·3	○	
24	4·	○	1··2 3
25	4·	○	2· ·3 1●
26	4· 2· 1·	○	3·
27	·4 ·2 3·	○	·1
28	·4 3· 1·	○	2·
29	4· ·3	○	2· 1·
30	2· ·1 ·3 ·4	○	
31		○	·2 1· ·4 ·3

Phenomena of Jupiter's Satellites.

Central Time

		h m			
Dec.	1	2 53	P. M.	II	Sh. In.
		2 57	"	II	Tr. In.
		5 29	"	II	Sh Eg.
		6 33	"	II	*Tr. Eg.
	2	12 39	A. M.	I	*Sh. In.
		1 11	"	I	*Tr. In.
		2 56	"	I	*Sh. Eg.
		3 27	"	I	*Tr. Eg.
		3 05	P. M.	III	Ec Dis.
		7 51	"	III	*Oc. Re.
		9 50	"	I	*Ec. Dis.
	3	12 34	A. M.	I	*Oc. Re.
		10 03	"	II	Ec. Dis.
		1 36	P. M.	II	Oc. Re.
		7 08	"	I	*Sh. In.
		7 37	"	I	*Tr. In.
		9 24	"	I	*Sh. Eg.
		9 53	"	I	*Tr. Eg
	4	4 18	"	I	Ec. Dis.
		7 00	"	I	*Oc Re.
	5	4 11	A. M.	II	*Sh. In.
		5 04	"	II	*Tr. In.
		6 47	"	II	Sh. Eg.
		7 41	"	II	Tr. Eg.
		1 36	P. M.	I	Sh. In.
		2 03	"	I	Tr. In.
		3 53	"	I	Sh. Eg.
		4 19	"	I	Tr. Eg.
	6	5 12	A. M.	III	*Sh In.
		6 54	"	III	Tr. In.
		7 39	"	III	Sh. Eg.
		9 42	"	III	Tr. Eg.
		10 47	"	I	Ec Dis.
		1 26	P. M.	I	Oc. Re.
		11 20	"	II	*Ec Dis.
	7	2 43	A. M.	II	*Oc. Re.
		8 05	"	I	Sh. In.
		8 29	"	I	Tr. In
		10 21	"	I	Sh Eg.
		10 43	"	I	Tr. Eg.

		h m			
Dec.	12	6 47	A. M.	II	Sh.
		7 19	"	II	Tr.
		9 24	"	II	Sh.
		9 56	"	II	Tr.
		3 31	P. M.	I	Sh.
		3 47	"	I	Tr.
		5 47	"	I	*Sh.
		6 03	"	I	*Tr.
	13	9 11	A. M.	III	Sh
		10 10	"	III	Tr.
		12 00	M.	III	Sh.
		12 41	P. M.	I	Ec.
		12 59	"	III	Tr.
		3 11	"	I	Oc.
	14	1 56	A. M.	II	*Ec.
		4 56	"	II	*Oc.
		9 59	"	I	Sh.
		10 12	"	I	Tr
		12 16	P. M.	I	Sh.
		12 28	"	I	Tr.
	15	7 10	A. M.	I	Ec.
		9 36	"	I	Oc.
		8 05	P. M.	II	*Sh.
		8 27	"	II	*Tr.
		10 42	"	II	*Sh.
		11 03	"	II	*Tr.
	16	4 28	A. M.	I	*Sh.
		4 38	"	I	*Tr.
		6 44	"	I	Sh.
		6 54	"	I	Tr.
		11 04	P. M.	III	*Ec.
	17	1 39	A. M.	I	*Ec
		2 25	"	III	*Oc.
		4 02	"	I	*Oc.
		3 13	P. M.	II	Ec.
		6 03	"	II	*Oc.
		10 56	"	I	*Sh.
		11 04	"	I	*Tr.
	18	1 13	A. M.	I	*Sh.
		1 20	"	I	*Tr.

Date	h	m		Sat.	Phenomenon
Dec. 22	9	03	A. M.	I	Oc. Dis.
	11	19	"	I	Oc. Re.
	10	41	P. M.	II	*Tr. In.
	10	41	"	II	*Sh. In.
23	1	18	A. M.	II	*Tr. Eg.
	1	19	"	II	*Sh. Eg.
	6	22	"	I	Tr. In.
	6	22	"	I	Sh. In.
	8	38	"	I	Tr. Eg.
	8	38	"	I	Sh. Eg.
24	2	52	"	III	*Oc. Dis
	3	29	"	I	*Oc. Dis.
	5	45	"	III	Ec. Re.
	5	45	"	I	Ec. Re.
	5	48	P. M.	II	*Ec. Dis.
	8	16	"	II	*Oc. Re.
25	12	47	A. M.	I	*Tr. In.
	12	51	"	I	*Sh. In.
	3	03	"	I	*Tr. Eg.
	3	06	"	I	*Sh. Eg.
	9	55	P. M.	I	*Oc. Dis.
26	12	14	A. M.	I	*Ec. Re
	11	48	"	II	Tr. In.
	11	59	"	II	Sh. In.
	2	25	P. M.	II	Tr. Eg.
	2	37	"	II	Sh. Eg.
	7	13	"	I	*Tr. In.
	7	19	"	I	*Sh. In.
	9	29	"	I	*Tr. Eg.
	9	37	P. M.	I	*Sh. Eg.
27	4	21	"	I	Oc. Dis.

Date	h	m		Sat.	Phenomenon
Dec. 27	4	41	P. M.	III	*Tr In.
	5	11	"	III	*Sh In.
	6	42	"	I	*Ec Re.
	7	29	"	III	*Tr. Eg
	8	02	"	III	*Sh Eg.
28	7	06	A. M.	II	Ec Dis.
	9	23	"	II	Oc. Re.
	1	39	P. M.	I	Tr In.
	1	48	"	I	Sh. In.
	3	55	"	I	Tr. Eg
	4	04	"	I	Sh Eg
29	10	47	A. M.	I	Oc Dis.
	1	11	P. M.	I	Ec. Re
30	12	55	A. M.	II	*Tr In.
	1	18	"	II	*Sh. In.
	3	32	"	II	*Tr. Eg.
	3	55	"	II	*Sh. Eg.
	8	05	"	I	Tr In.
	8	17	"	I	Sh In.
	10	21	"	I	Tr Eg
	10	33	"	I	Sh. Eg.
31	5	13	"	I	*Oc. Dis
	6	02	"	III	Oc. Dis.
	7	40	"	I	Ec. Re.
	9	46	"	III	Ec Re.
	8	23	P. M.	II	*Ec. Dis.
	10	30	"	II	*Oc Re.
Jan. 1	2	31	A. M.	I	*Tr In
	2	45	A. M.	I	*Sh In.
	4	47	"	I	*Tr Eg.
	5	02	"	I	*Sh Eg

NOTE.—In., denotes ingress; Eg., egress; Dis., disappearance; Re., reappearance; Ec., eclipse. Oc., denotes occultation; Tr., transit of the satellite; Sh. transit of the shadow; * Visible at Washington.

Phases and Aspects of the Moon.

		Central Time. d	h	m
Apogee	Dec.	2	5	00 P. M.
First Quarter		5	6	15 A. M.
Full Moon		12	1	46 P. M.
Perigee		14	9	00 A. M.
Last Quarter		19	5	16 A. M.
New Moon		26	8	20 P. M.
Apogee		30	5	20 A. M.

Maxima and Minima of Variable Stars.

[From ephemerides by Dr. Loewy in the "Companion to the Observatory," and by Dr. Hartwig in the "Vierteljahrsschrift der Astronomische Geselschaft."]

MAXIMA.

Dec. 3	S Delphini.
4	χ Cygni.
7	S Vulpeculæ
11	T Canis Min.
11	W Cygni.
12	U Cancri.
13	U Monocerotis.
16	V Monocerotis.
17	S Ceti
20	R Lyræ
23	R Vulpeculæ.

MAXIMA CONT.

Dec. 25	U Puppis.
25	U Boötis
27	S Scorpii.
29	S Libræ.
30	R Hydræ.

MINIMA

Dec. 2	R Aquilæ.
3	U Piscium.
4	V Cancri
4	X Boötis.
5	R Lyræ.

MINIMA CONT.

Dec. 6	V Coronæ
6	U Cassiopeiæ.
8	T Virginis.
15	S Virginis.
17	R Centauri.
19	W Tauri.
19	R Sagittæ.
21	S Arietis.
25	T Libræ
27	S Aquilæ.
27	η Geminorum.
28	V Libræ.

Minima of Variable Stars of the Algol Type.

[Given to the nearest hour in Central Standard Time.]

U CEPHEI.

Alternate Minima.

		h
Dec.	2	12 M.
	7	11 A. M.
	12	11 "
	17	11 "
	22	10 "
	27	10 "

ALGOL.

Alternate Minima.

Dec.	6	10 A. M.
	12	4 "
	17	10 P. M.
	23	3 "
	29	9 A. M.

λ TAURI.

Alternate Minima.

Dec.	7	7 A. M.
	15	5 "
	23	2 A. M.
	30	midn.

R. CANIS MAJORIS.

Every third Minimum.

Dec.	4	2 A. M.
	7	12 M.
	10	10 P. M.
	14	8 A. M.
	17	5 P. M.
	21	3 A. M.
	24	1 P. M.
	27	11 P. M.
	31	8 A. M.

S CANCRI.

		h
Dec.	6	3 P. M.
	16	2 A. M.
	25	2 P. M.

S. ANTLIÆ.

Every third Minimum.

Dec.	1	10 P. M.
	2	10 "
	3	9 "
	4	8 "
	5	8 "
	6	7 "
	7	6 "
	8	6 "
	9	5 "
	10	4 "
	11	4 "
	12	3 "
	13	2 "
	14	2 "
	15	1 "
	16	12 M.
	17	12 "
	18	11 A. M.
	19	10 "
	20	10 "
	21	9 "
	22	8 "
	23	8 "
	24	7 "
	25	6 "

S. ANTLIÆ CONT.

Every third Minimum.

	h
26	6 A. M.
27	5 "
28	4 "
29	4 "
30	3 "
31	2 "

U. Coronæ.

Alternate Minima.

Dec.	1	6 P. M.
	8	4 "
	15	2 "
	22	12 M.
	29	9 A. M.

Y CYGNI.

Alternate Minima.

Dec.	1	6 P. M.
	4	6 "
	7	6 "
	10	6 "
	13	6 "
	16	6 "
	19	6 "
	22	6 "
	25	6 "
	28	6 "
	31	5 "

The Satellite of Neptune.

S

CENTRAL TIMES OF GREATEST EL

NEWS AND NOTES.

Those who may be in arrears for subscription to volume XIII will greatly oblige the publisher by early settlement of such dues that the books may be promptly closed for the year 1894.

Subscribers are kindly requested to bear in mind the fact that our next number will close volume XIII. It will contain a full general index prepared with the greatest care.

The Transit of Mercury, Nov. 10.—Professor Howe, Director of the Chamberlin Observatory, Denver, Colo., calls our attention to an error in the calculated times of beginning of the transit as given in our last number. The minus sign in the formula for ingress was overlooked by both the computer and the one who checked the work. The Greenwich times should be as follows:

Observatory.	Longitude		Latitude.		Transit Begins.			Transit Ends.		
	°	′	°	′	h	m	s	h	m	s
Harvard	71	08	+ 42	22	3	55	42	9	12	07
Washington	77	03	+ 38	54	3	55	55	9	12	06
Goodsell	93	09	+ 44	28	3	55	52	9	12	15
Chamberlin	104	57	+ 39	41	3	55	59	9	12	17
Lick	121	39	+ 37	20	3	56	05	9	12	23

North Greenland Aurora Observations.—Those co-operating, and others, will be interested to learn that the records of observations of the aurora made at the station of Mr. Peary in North Greenland the past winter have been received, and that their comparison with similar records from other parts of the Earth is now in progress. It already appears that the conclusions heretofore announced in regard to certain phases of the subject are being substantially confirmed, and that the forces, of which the aurora is the visible expression and type, play an exceedingly important part in many ways. The relations to certain very definite solar conditions, and to thunder storms, and to certain phases of atmospheric control are becoming especially clear. It is fitting that all who have so kindly contributed to the success of the scheme should have assurance that their labor is not in vain. It is proposed to continue these observations and if possible make them even more valuable. All data bearing upon the subject will be thankfully received and employed to the best possible advantage. M. A. VEEDER.

Lyons, N. Y., U. S. A., Oct. 18th, 1894.

Since above was mailed yesterday, very fine records have been received from Mr. George Comer, of East Haddam, Conn., made at lat. 63° 55′ N. and long. 90° 20′ W., the nearest the magnetic pole of any thus far at hand.

Transactions of the Royal Irish Academy.—We are in receipt of Part 3, Vol. 30 of the transactions of the Royal Irish Academy. It is a recent publication of micrometrical observations of nebulæ made at the Armagh Observatory, by J. L. E. Dreyer, Ph. D., and read before the Academy, Dec. 11, 1893. These observations were undertaken to supply some of the materials thought necessary to aid in determining the question whether or not the nebulæ are endowed with sensible proper motion. From a knowledge of the proper motion of the stars generally it seems probable that the nebulæ are so endowed, although there is yet wanting the positive proof that a single nebula is affected by such motion. The

question is an important one in the study of the constitution of the Universe, on this account the brighter nebulæ have been much observed by leading Euro astronomers. Dr. Dreyer calls attention also to the work of Schönfeld on positions of many of the fainter nebulæ, indicating that his right ascension rather more affected by systematic errors than those of other observers, an this account that he had thought it useful to re-measure a number of these ob and at the same time to observe some of the brighter nebulæ by means of wh comparison might be made between his own measures and those of Schultz, V Engelhardt and others.

The work of this catalogue was done by the aid of the 10-inch refract the Armagh Observatory, made by Sir Howard Grubb and set up in 1885 object glass is of excellent quality and the Observatory is furnished with a d 16 feet in diameter. After describing at length the instrument used and the m of reducing his observations, Dr. Dreyer speaks of some results obtained as lows:

"In order to establish the relation of my measures to those of other obser I have compared my results with those of Schonfeld and Schultz with wh have most objects in common. Their measures are referred to the epoch of 18 and they had therefore first to be corrected for precession to my epoch 1890.

G. C.	*h.*	Corr. to $\Delta\alpha$	Corr. to $\Delta\delta$	Star.
		s	"	
107	46	− .02	− 0.2	
342	128	− .04	− 1.6	$2^m\ 15^s p$
351	132	+ .01	+ 0.7	
352	131	− .12	+ 1.2	
355	133	− .07	+ 0.7	
363	137	.00	+ 0.4	
470	183	− .02	− 1.6	
549	226	− .09	+ 1.1	
600	262	.00	+ 3.1	$2^m\ 15^s f$
604	264	+ .10	+ 4.2	
627	II. 619	− .02	+ 0.7	
826	2618	− .01	− 3.9	
1202	IV. 33	.00	+ 3.0	
1225	365	.00	− 5.6	$2^m\ 34^s p$
1225	365	− .04	− 1.1	$34^s p$
1519	444	+ .11	+ 2.0	
1532	450	− .02	0.0	
1567	3095	.00	+ 0.4	
1672	513	− .02	+ 0.2	
1771	564	+ .01	+ 1.2	
1861	604	− .11	− 2.4	
1973	657	+ .02	+ 0.3	
2028	680	.00	+ 0.1	
2030	678	− .10	− 1.1	
2204	755	− .14	− 1.2	
2220	768	+ .04	+ 0.3	
2347	840	+ .05	+ 0.6	
2501	945	− .01	− 0.2	
2757	1112	+ .04	0.0	
2768	1119	+ .04	0.0	
2776	1126	.00	0.0	

G. C.	*h.*	Corr. to $\Delta\alpha$	Corr. to $\Delta\delta$	S
		s	"	
2806	1148	− .02	+ 0.1	
2886	1209	− .07	+ 0.2	
2890	1211	− .07	+ 0.3	
3132	1376	− .05	− 0.5	1^m
3702	1703	+ .01	− 1.2	1
3702	1703	+ .02	− 1.3	1^m
3704	1705	− .04	+ 1.9	2^m
3846	1779	− .03	+ 1.5	
3900	1813	.00	− 0.1	
3964	1857	.00	− 1.0	
4024	3587	− .03	− 2.4	1^m
4485	2030	+ .01	+ 0.4	
4608	2090	.00	− 1.6	
4632	2102	+ .08	− 0.9	
4632	2102	− .01	− 2.1	1^m
4670	2120	− .04	− 1.6	
4678	2125	− .02	+ 0.8	
4718	2135	− .03	− 0.2	
4815	2172	+ .15	+ 1.6	
4879	2199	− .07	− 1.0	
4880	II. 249	− .01	− 0.2	
4883	2201	− .06	− 0.4	
4886	2202	.00	+ 0.1	
4887	2203	.00	+ 0.2	
4892	2205	− .04	− 0.9	
4909	2216	− .03	− 0.7	
4928	2226	+ .05	− 1.5	
4932	2227	− .05	+ 1.0	
4939	2232	− .02	− 0.2	
5029	2282	+ 0.1	.00	

Only such objects were compared which had been measured from the comparison star. The "Dumb-bell nebula" and *h* 2205 were omitted, as diff points appear to have been observed, and here again my few observatio oblique transits were left out. The results are

	$\Delta\alpha \cos\delta$	$\Delta\delta$	Nebulæ.
Schönfeld I—Dreyer	$-0^s.106$	$-0''.65$	27
Schönfeld II—Dreyer	$+0.042$	$+0.74$	38
Schultz—Dreyer	-0.071	$+0.08$	26

The first two comparisons give the right ascension, Schönfeld I-II $= -0^s.15$, while Schönfeld found by direct comparison between his two series $-0^s.21$ and through comparison with Schultz $-0^s.19$. The reality of the systematic errors in right ascension observations of nebulæ has thus again been confirmed, by the present series of observations, while the differences in declination must be considered as mere accumulations of accidental errors. It appears that I differ less from Schönfeld in right ascension than any previous observer has done, but his right ascensions of nebulæ remain the smallest of any as yet determined. I think, however, that it would at the present moment be premature to attempt to combine the results of all known micrometrical measures in order to form a catalogue of the exact places of nebulæ, but it seems likely that photography might here lead to interesting results, and supply the standard right ascensions which appear so difficult to get hold of."

The Orbit of the Fifth Satellite of Jupiter.—M. Tisserand reports the results of his researches on the orbit of the satellite of Jupiter discovered the 9th of September 1892 by Barnard of the Lick Observatory, in California. This small star is very difficult to observe, nevertheless, the observations of Barnard made with the aid of the greatest telescope actually in existence, are very precise.

M. Tisserand has endeavored to represent these by a circular orbit, a fixed elliptic orbit, and a variable elliptic orbit. The last method gives the most satisfactory results. The eccentricity of the orbit is very small—about 0.01. The ellipse is therefore almost a circle. The major axis makes a complete revolution in five months. This last motion is due to the equatorial protuberance of Jupiter.—Translated from *La Nature* of Oct. 13, 1894.

The Mass of Mercury.—M. Backlund's recent researches on the mass of the planet Mercury, and the acceleration of the mean movement of Encke's comet, are described by M. Callandreau in *Comptes rendus* of October 1. Encke's comet is interesting not only on account of the diminution of its period of revolution (about two hours from one apparition to the next), but also from the fact that its movement is disturbed by Mercury. A discussion of the seven apparitions of the comet between 1871 and 1891 has led M. Backlund to conclude that Mercury has a much smaller mass than has hitherto been ascribed to it. The value obtained is

$$\text{Mass of Mercury} = \frac{1}{9{,}647{,}000}$$

It would, therefore, take about 9,700,000 bodies like Mercury to make up the mass of the Sun.

To account for the acceleration of Encke's comet, it has been supposed that a resisting medium of some kind is uniformly distributed round the Sun. M. Backlund, however, thinks that all hypotheses of a continuous resisting medium of uniform density ought to be discarded, and that the resistance is very probably met only in certain regions. This idea is a very plausible one, for, according to Laplace's hypothesis, in the formation of the planets from the solar nebula, all the substance of the rings would not be used up in the process, and some of it would, without doubt, travel along the planetary orbits as clouds of very light

material. It is suggested that Encke's comet passes through nebulous clouds of this kind, and that the resistance they offer causes the observed acceleration of the mean motion.—*Nature*, Oct. 18, 1894.

The Chicago Academy of Sciences.—*Section of Mathematics, Astronomy and Physics.*—The regular monthly meeting was held at the Commerce Club, Auditorium Building, Professor G. W. Hough, President, in the Chair.

Professor Geo. E. Hale read the first paper of the evening on *Experimental Investigations of the Effective Temperature of the Sun*, by W. E. Wilson and P. L. Gray.

The speaker gave an interesting historical account of the former efforts to determine the effective temperature of the Sun, detailing especially the methods employed by Herschel, Pouillet, Viole, Langley and Rosetti. The principles involved in the experiments were discussed, and illustrations of the apparatus exhibited by means of the lantern. Professor Hale then gave an account of Wilson's work, and pointed out the superior character of his apparatus and the care with which he had corrected for every possible source of error, such as solar and terrestrial atmospheric absorption. The radio-micrometer was explained, and diagrams of work on the melting points of gold, silver, and palladium exhibited.

Wilson's effective temperature of the Sun, 6200 degrees centigrade, was thought to confirm in a general way the value found by Rosetti, which was about 10000 degrees.

In conclusion, Professor Hale thought a great gain had been made by reducing the supposed temperature of the Sun from several million to a few thousand degrees, which he regarded as a first approximation to the truth. A general discussion followed, in which Professor Burnham, Professor Hough, Professor Crew and others took part.

Professor Burnham, who had just returned from Boston, announced to the Academy that he had tested the 40-inch glass of the Yerkes Observatory at the shops of Alvan G. Clark, and had found it first class.

When tried on double stars like ϵ Lyræ and β Delphini it gave good images, and when pointed on Vega and α Cygni the field was dark, so that Professor Burnham was satisfied that when finished the glass would be the crowning glory of its illustrious manufacturer.

Professor Wadsworth made a few remarks on Professor Michelson's work with the refractometer, and its application to astronomical measurement. After some general discussion the session adjourned.

The Perseid Radiant.—Sir: I have only just seen Mr. Denning's letter in your August number on my return from my vacation trip. The question at issue between Mr. Denning and myself is of the simplest possible character, and I hope his personalities will not induce your readers to lose sight of it. It is this: Do Mr. Denning's observations (or any other published observations) *prove* a shifting of the Perseid radiant? If I alleged that I had made observations which led to a different result, his strictures on my skill as an observer and his charge of being located in my feather bed when he was observing would have some relevance. At present they have none. I accept every one of his published observations as correct. He affirms that these observations prove a shifting of the radiant. I say they do not. The question is not one of observation but of proof. Let Mr. Denning show how the observations recorded by him prove that the radiant shifts and I will either accept his proof or point out its defects. But I venture

to remind Mr. Denning that assertion is not proof. He may go on repeating that his observations prove or demonstrate the shifting "to the end of time" (if he should so long live) but I do not think he is likely to convince astronomers generally until he condescends to give the details of his proof. Indeed he writes as if the shifting of the radiant was a thing that could be seen. Of course it is nothing of the kind. Mr. Denning has recorded fully and accurately what he saw. I do not dispute his catalogue in the least. The question is, what does that catalogue prove? As to the late Dr. Kleiber's computations they proceed on an assumption as to the shape of the meteor-swarm which appears to have been suggested to the author by Mr. Denning's supposed discovery. There is no proof that the swarm has this shape.

I have occupied much of your space in treating of the relations between the proper motions and the spectra of stars. I did not observe either the spectra or the proper motions. I was located in my feather-bed when others were doing so. Consequently, according to Mr. Denning, I knew nothing of what I was writing about and your space has been absolutely wasted.

To be brief, I entirely dispute Mr. Denning's fundamental assumption that no person who is not a practical observer is competent to criticise the inferences drawn by a practical observer from his observations. A very able observer may be a very poor logician and one who never made an observation but understands the elementary principles of the theory of proof may signalise the defects in his argument.

I do not however "aver" that the Perseid radiant does not move. I only assert the "negative result" referred to by Mr. Denning, viz., that its shifting has not been proved. I ask for further observations in order to decide the question whether it shifts or not, and I hope that observers will make these observations regardless of Mr. Denning's *ipse dixit* that they are unnecessary.

Dublin, Oct. 3. W. H. S. MONCK.

P. S. I have seen very few observations of the Perseids of the present year. The only observer who sent me his results (Mr. Milligan of Belfast) writes "No trace of displacement from night to night." He had been on the lookout from about July 24 but did not notice any Perseids until Aug. 4.

The Progress of Astronomical Photography.—All our readers interested in astronomical photography should read H. C. Russell's paper on that theme which was read as the president's address before the Section of Astronomy, Mathematics and Physics of the British Association at its last meeting. We give elsewhere the first part of it. More will appear later.

E. E. Barnard's Present to the Royal Astronomical Society.—At the June meeting of the Royal Astronomical Society it was announced that Professor Barnard of the Lick Observatory, had sent a series of positives of his astronomical photographs as a present. These arrived in due course, and a very beautiful series they are, well worth a visit to the Burlington House specially to see. There are altogether more than 60 glass positives (10 in. by 8 in., 8½ in. by 6½ in.) chiefly of comets, but including one or two of stars, eclipses, etc. The most remarkable are perhaps those of the Brooks' comet where the tail is in process of being shattered—as though by some cyclone in space. But all are worth careful study and it is to be hoped that several, if not all, may be published by the Society in some form or other. The whole of the manual labor of taking the originals and copying them has been undertaken by Professor Barnard personally. At the Lick Observatory manual assistance is no more plentiful than some other luxuries which we have cause to regard almost as necessities.—*Observatory for October, 1894.*

Telescope for the Illinois Wesleyan University.—An 18½-inch equatorial
tor provided with driving clock, circles and 2-inch finder has been presented
Illinois Wesleyan University of Bloomington, Ill., by Mr. A. C. Behr a f
resident of that city. It is of the Newtonian form, focal length 10 feet
Some changes will be made in its mounting and a visual and photographi
troscope added.

A position micrometer and a time transit have been ordered and the
refractor used heretofore will be provided with apparatus for viewing the
prominences. Professor M. P. Lackland will direct the work of the Observa

Publications of the Lick Observatory.—Vol III, 1894 of the publications o
Lick Observatory is received. The present organization for astronomical
at the Observatory is Dr. E. S. Holden, Director and Astronomer, J. M. S
berle, E. E. Barnard, W. W. Campbell, R. H. Tucker, Jr., astronomers, Al
Cotten, assistant Astronomer and C. D. Perrine, secretary.

The contents of Vol. III are. Selenographical studies, based on neg
of the Moon taken at the Lick Observatory, by Professor L. W
Report on specimens of crown and flint-glass belonging to the Lick Ob
tory, and manufactured by Messrs. Feil, by Professor C. S. Hastings. Inve
tion of the glass scale "A" of the Lick Observatory measuring engine, by
Tittmann. Spectroscopic observations of nebulæ made at Mount Ham
California, with the 36-inch telescope of the Lick Observatory, by Professor
Keeler.

The introduction by Professor Holden is a description of the optical pa
the 36-inch equatorial with some of the accessories in use in photographic
a detailed account of work done in lunar photography and a description of
of the very beautiful plates which the Volume contains. The plate represe
the region about Mare Crisium when on the terminator of the Moon i
tainly the finest representation of that interesting field we have ever seen.
part by Professor Weinek ought to be read by everyone interested in lunar
ies. But the especially important part of the volume is by Professor Keeler.
have only space for the summary of his results pretaining to spectroscopic
vations of the nebulæ made while he was at the Lick Observatory. We
them in full except the concluding note as follows:

The principal conclusions which have been reached as a result of these in
gations are summarized below. Some of them are but a confirmation, by p
ful apparatus and exact measurements, of results which had been ren
extremely probable by the labors of other observers. It is on account o
increased weight which is given to these results that they are included i
summary.

1. The normal position of the chief nebular line, or position which th
would have in the spectrum of a nebula at rest relatively to the observ
λ 5007.05 ± 0.03 on Rowland's scale.

2. The normal position of the second nebular line on Rowland's sc
λ 4959.02 ± 0.04. This line is 1.39 ± 0.04 tenth-meters less refrangible th
center of the double line of iron at λ 4957.63.

3. The first and second nebular lines are not represented by absorptio
in the solar spectrum, at least not by any lines sufficiently strong to app
Rowland's photographic map.

4. The relative brightness of these two lines is, within the limits of e
estimation, the same for all nebulæ. Hence, there is some reason for th

that they are due to the same unknown substance, perhaps an element of somewhat the same nature as the so-called helium.

5. The spectrum of the bright line nebulæ indicates either a high temperature of the gases emitting the light, or a state of strong electrical excitement, and shows that the temperature and pressure are greatly increased at the nucleus.

6. The distance between the Great Nebula of Orion and the Sun is increasing at the rate of 11.0 ± 0.8 miles per second. This result shows that the nebula has little or no motion relatively to the center of figure of the stars from whose proper motions the drift of the solar system has been determined.

7. No relative motion of different parts of the Orion nebula could be detected; the limit of accuracy of the method, for the brighter parts of the nebula, being something like 4 or 5 miles per second.

8. The nebulæ are moving in space with velocities of the same order as those of the stars. A table, showing the measured velocities of particular nebulæ in the line of sight is given elsewhere. Of the nebulæ observed, that having the greatest motion of approach, 40.2 miles per second, is *G. C. 4373*, that having the greatest motion of recession, 30.1 miles per second, is *N. G. C. 6790*. Most of the nebulæ have considerably smaller velocities than these.

9. The only direct attempt to detect rotation of a planetary nebula gave the negative result that there was no differential motion of opposite limbs amounting to as much as 7 or 8 miles a second.

10. The visible spectrum of the nucleus of a planetary nebula has in many cases a strong resemblance to that of stars of the Wolf-Rayet class, although the two principal nebular lines are absent from the latter, and still other differences exist, the importance of which cannot at present be exactly estimated.

The observations which have been described in this paper were intended to form part of a more extended research, which was brought to an end (so far as my own part in it was concerned) by my removal from Mount Hamilton to Allegheny. I have to express my obligations to Professor Holden for every encouragement in the work, and for every facility in its prosecution which the equipment of the Observatory could furnish. I have also to thank Mr. W. W. Campbell, then of the University of Michigan, and now of the Lick Observatory, and Mr. A. O. Leuschner, of the University of California, for their efficient aid in the observations during the summer of 1890.

Observations of Mars.—The current number of the *Observatory* contains a short article in which Mr. Stanley Williams directs attention to certain important features of Mars, which, it will be remembered, is in opposition on Saturday. With regard to the canals or channels, he remarks that a few points upon which observations are desirable are: "How far is the visibility of the canals in different parts of the planet affected by seasonal changes? Their duplication, when does it occur? How long does it last? How does it occur? And again, how far is it subject to seasonal changes?" Mr. Williams commenced observations in the latter part of August, and he found that the planet canals were conspicuous, and even those of average distinctness could be seen without much difficulty. At the date of writing (September 18) he had observed about thirty of the canals, although only about two-thirds of the planet's face had been examined. Ganges was seen double on August 29, but not so clearly as in 1892. Gehon was also seen plainly double on the same date. Three other canals—Eunostos, Cyclops, and Cerberus—were found distinctly duplicated, and the gemination of Phison was suspected. The observations were made almost exactly at the time of the

summer solstice of Mars' southern hemisphere. Mr. Williams has obser small dark spots similar to the "lakes" detected by Professor W. H. Pick Arequipa in 1892.

The Algol Variable discovered by Dr. Hartwig, 19 B. D. + 15°.3311, error in translating from code, it was announced by me to be B. D. + 1 Will you kindly note the correction? The period of the star is given as onds less than 2 days.

JOHN RITCHI

From Canada.—Professor Holden of the Lick Observatory has forwa third volume of the reports of the Lick Observatory to the Astronon Physical Society of Toronto, Canada, and the work was highly spoken recent meeting of the Toronto society. There are contained in the bo magnificent heliogravure plates, from negatives of the Moon taken a Hamilton, which may be considered the most perfect reproductions ever

Mr. A. Elvins, of the Astronomical and Physical Society of Toronto, has been working industriously on the planet Mars with the 6-inch ref the Toronto Observatory. As a result this gentleman has presented to hi a number of drawings of the ruddy orb Colorings of the diverse par Martial disc are faithfully shown in these delineations, but the rectiline ings, or so-called canals, do not appear. No doubt a 6-inch objective i ficiently powerful to reveal those details.

At a recent meeting of the Astronomical and Physical Society of T Mr J. R. Collins exhibited some photographs of the magnetic lines of for by sprinkling steel filings upon sensitized paper placed over a magnet, fin a short exposure to the light.

JOHN A. CORT

The Mass of Jupiter.—In *A. N* 3249, Professor Newcomb has an artic gives valuable information concerning the mass of Jupiter. Its concludi graph is as follows

The following table shows the values, and the relative weights to have judged each one entitled. I do not deem it necessary at the present give in all detail the considerations which led to the adoption of these we may remark, however, that von Haerdtl's excellent result from the pertur of Winnecke's comet which has by far the smallest probable error of an mination yet made, has not been assigned a corresponding weight, becau distrust on my part whether observations on a comet can be considered ing always been made on the centre of gravity of a well defined mass, mo if its center were a material point subject to the gravitation of the Sun a ets. This distrust seems to me to be amply justified by our general expe the failure of comets to move in exact accordance with their ephemerid mass of Jupiter from

All observations on the satellites................................	$\mu =$ 1047
Action on Faye's comet (Möller)	1047
Action on Themis (Krueger)................................	1047
Action on Saturn (Hill)	1047
Action on Polyhymnia	1047
Action on Winnecke's comet (v. Haerdtl)................................	1047
Weighted mean	1047
	m. e

I propose to regard this mass of Jupiter as a definitive one to be adopted in my work on the planetary theories.

In the interest of the astronomy of the future, it seems very desirable to apply Gill's heliometer method to the continuous observation of a selected number of the minor planets, especially Polyhymnia. I include this planet, although it is to be feared that it can be reached with the heliometer only at opposition near its perihelion.

Brorsen's Comet 1851 III.—This comet first appeared in the month of August, 1851, moving in the constellations of Bootis and Draco. On forty-one evenings observations were made, besides numerous measures of position with micrometers, and many have been the attempts to deduce an accurate orbit. Among these may be mentioned Rumker (*Astr. Nach.*, No. 771), Vogel (*Astr. Nach.*, No. 774), Brorsen (*Astr. Nach.*, No. 775), and Tuttle (*Astr. Journal*, II.), who found parabolic elements, none of which satisfied the observations sufficiently. At a later date Brorsen obtained elliptical elements (*Astr. Nach.*, No. 782), which he compared with all the then known observations. In the communication before us, on a new determination of the orbit of this comet by Dr. Rudolf Spitaler (*Denkschriften der Math. Naturwiss. Classe der k. Ak. der Wissenschaften*), the writer makes use of some new observations and more accurate places for the comparison stars. To limit this note we will state in a few words the result he has obtained. The most probable parabolic elements after two or three "verbesserungen" were

$$T = 1851 \text{ August } 26.2523 \text{ Paris Mean Time}$$

$$\left.\begin{array}{l} \pi = 316^\circ\ 57'\ 25''.7 \\ \Omega = 223\ \ 40\ \ 21.2 \\ i = \ \ 38\ \ 12\ \ 57.5 \end{array}\right\} \text{Eq. } 1851.0$$

$$\log q = 9.9933272$$

An attempt to improve this led to elliptic elements as follows:

$$T = 1851 \text{ August } 26.249997 \text{ Paris Mean Time}$$

$$\left.\begin{array}{l} \pi = 310^\circ\ 57'\ 19''.2 \\ \Omega = 223\ \ 40\ \ 33.9 \\ i = \ \ 38\ \ 12\ \ 52.9 \end{array}\right\} \text{Eq. } 1851.0$$

$$\log q = 9.9933235$$

$$e = 0.9999151$$

Both these elements give ephemerides which agree well with the observations, and can be looked upon as accurate within the limit of error of the observations.—*Nature*, Oct. 18, 1894.

The Moon.—The map of the Moon, compiled by Jules A. Colas and published by Messrs. Poole Brothers, of Chicago, is now nearly ready for general sale. We have before called the attention of the readers of this magazine to this important aid to lunar study. The map is now quite complete and a copy is before us, and we can speak of it more definitely than before.

The size of the map is 2 feet by 29 inches. It is mounted on heavy paper which is cloth lined on the back. The background for the lunar disk is a deep blue, which well represents the color of the sky in full moonlight. The diameter of the disk is a little more than 20 inches. It was made so large that all the prominent features on the surface might be well defined in character and be plainly numbered for easy reference. There are nearly five hundred and fifty such features presented on this map, besides a great number of others of less importance that are never named on any map. The markings that are named are so clearly and neatly

designated that the map is well adapted for class uses in study or lect
admirably well suited for the public or private libraries as a reference ma
After considerable study of the shading on the disk to represent t
plateaus, mountains, craters and streaks, we are glad to say that the
has more nearly represented the true average color of these features, as
by the telescope, regardless of lunar phase, than is found on any sim
of the Moon we know of. Although guided by excellent lunar photog
getting these slightly varying shades, no one knows better than the comp
difficult it is to correct the errors of a photographic picture which alwa
the seas too dark and the plateaus too bright so as to get a true relation
for all. In this we think he has succeeded admirably. In photograp
Moon it is very difficult to get any but the most prominent marking
bottom of the seas or craters, because a length of exposure that would sh
details would destroy all markings on the plateaus by over exposure. Bu
map the student will be pleased to notice that a great variety of marked
dulating surface is recorded on every sea bottom. This will make the
alistic in a large sense to those who have the aid of a telescope.

The hardest thing to get in such a picture as this, are the streaks that
from the great mountains Tycho, Copernicus, Kepler and some others. T
fairly well given, even better than photographs generally show them
phase, yet the differences between the systems of Copernicus and Kepler a
even in a small telescope. This difference is of no consequence, however
present knowledge of the causes of these great markings on the surfac
Moon.

We especially commend this new map of the Moon to all interested i
studies, for we do not know of another so good and complete at so small
as that asked by the publishers.

An Exercise Book in Algebra, by Matthew S. McCurdy, M. A., conta
pages of exercises intended as supplementary drill work and adapted for
any text of medium grade, it also states a few definitions and brief ru
thus becomes suitable for use in general review work on the subject of
At its close is a series of specimen examination papers from leading
The neat binding, excellent paper and clear type make the volume an at
one, the exercises in their grading and arrangement are the evident wor
experienced teacher. The book is admirably adapted to meet the need
has in view. Publishers Leach, Shewell and Sanborn. Price, 60 cents.

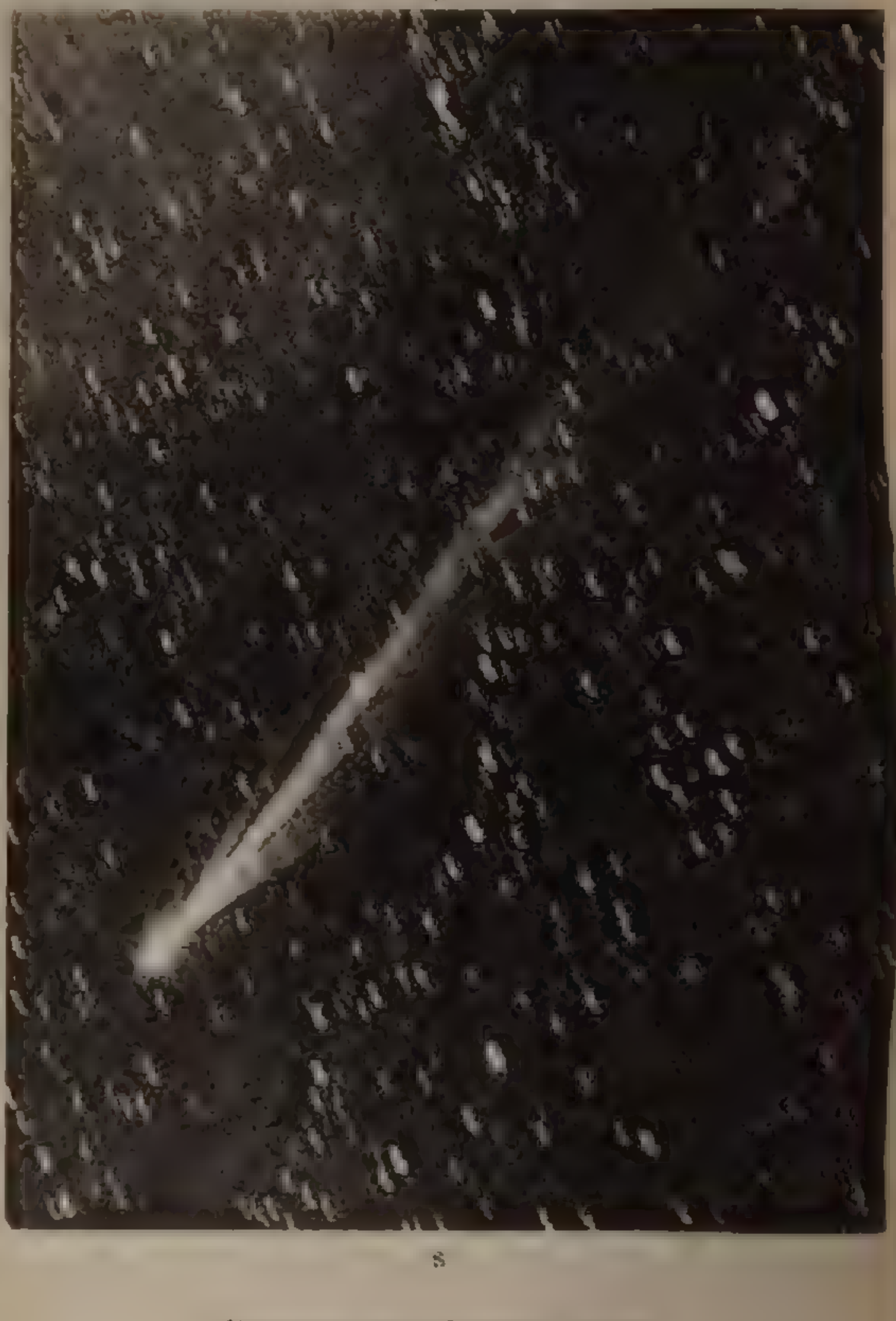
Star Names and Meanings.—Richard H. Allen is preparing a book to b
Star Names and Meanings. We have before us sample pages taken fro
part of the book which treats of the constellations. A brief history of th
of the constellation is given first, then follows special notice of each of the
nent stars belonging to the constellation. The Greek letters and the name
stars are both given in heavy faced type so as to be easily caught on the
the eye, and the historical meaning of the names is quite fully presented.
is given, in these pages, on the pronunciation of the names of the more
stars. This is to be regretted for that is *one* of the lacking things in almo
book about the stars. On the whole these advance leaves promise well
work will be looked for with interest.

Solid Geometry by Arthur Latham Baker, Ph. D., is a compact, con
hand-book of the elements of solid geometry. Its notation is new and c
planned with a view to increased clearness, its demonstrations are brief
ing given under the five distinct heads, Notation, To Prove, Constr
Analysis, Proof. Test exercises are found at the close of each chapter e
the case of the brief closing chapter on conic sections. The author's one ai
secure for the student a comprehensive, unified, working knowledge of th
ciples of solid geometry. Publishers, Ginn & Co.

Erratum.—Line 20 from bottom of page 744 should read 3921.32 and 3
instead of 3821.32 and 3821.38.

PLATE XXXV.

N



S

PHOTOGRAPH OF BROOKS' COMET.

1893, Nov. 10. $15^h\ 35^m - 17^h\ 35^m$ Pacific Standard time. Made with the [illegible] Willard lens of Lick Observatory.

ASTRONOMY AND ASTRO-PHYSICS, No. 130.

Astronomy and Astro-Physics.

VOL. XIII, No. 10. DECEMBER, 1894. WHOLE No. 130.

General Astronomy.

PHOTOGRAPHS OF A REMARKABLE COMET.*

E. E. BARNARD.

Mr. Ranyard has published in the February 1894 number of *Knowledge*, two photographs of the set which I obtained of Brooks' October, 1893, Comet.

These two were made on the dates October 21 and 22. They show the remarkable disturbance which utterly transformed the tail of the comet in the 24 hours intervening between the two pictures.

In the May number of the same journal, Mr. Ranyard has again reproduced an admirable enlargement of the picture of October 21.

Of the set of fifteen nights' photographs, these two are perhaps the most startling pictures. Many of the others, however, are extremely remarkable and some are very beautiful. One of these, on November 10th, though it does not present any startling features, is nevertheless so gracefully beautiful that I have thought the readers of ASTRONOMY AND ASTRO-PHYSICS would be interested in seeing it. I have, therefore, made an enlargement of it which accompanies this article.

This enlargement is $2\frac{1}{6}$ times the original size, and is on the scale of about $0^\circ.9$ to one inch.

In this picture it will be noticed that the star trails (produced by making the telescope move with the comet) are very irregular and broken. The comet was faint in the guiding telescope and no nucleus could be seen to guide by, so that it was impossible to accurately follow its motion; this, however, does not effect the image of the comet itself.

In the present photograph the phenomena apparently seem to be due to peculiarities of emission from the nucleus alone. The head is small and round, with a slender neck-like tail that branches out into three beautiful fan-like streams some 18′ from the head.

* Communicated by the author.

The central stream forms the main tail which can be tra
six degrees or more. This tail is clouded here and there w
equal masses of matter. A prominent one of these masses
noticed nearly 3° back from the head. Beyond this the ta
out rapidly and $4\frac{1}{2}°$ from the head is almost discontinu
yond this weak spot it again brightens up as a prominen
gated mass almost wholly detached from the tail. A s
mass to this is shown on several dates—especially on Nov
11th and 12th completely detached from the tail and whic
be the same object of November 10.

On the plate of November 6, a large mass one degree l
shown about 6° from the head and completely detached fro
tail.

There are many other interesting and important features s
on the various photographs, each one of which would be i
a study, but at present, until I can place all these pictures
erly before astronomers, it is not possible to go into detail
cerning them.

Though this comet was a small affair visually, and appa
of little consequence beside many of the great comets of
years, it unquestionably far exceeded any of them in the ri
of the phenomena it photographically presented.

It seems to have been passed unnoticed by photograph
tronomers elsewhere, which makes the Lick Observatory s
solutely priceless.

Like Swift's comet of 1892 the photographs of this
mark an epoch in cometary photography as decisive as it
portant.

In reference to the remarkable shattering of the tail
comet as shown on the photographs of Oct. 21 and 22, I
suggested that this might be due to an encounter of the ta
some kind of resisting medium—a cosmical cloud—a sw
meteors—certainly a region of resistance of some form
firmly adhere to this supposition. Mr. Ranyard, however
interesting and important article on "Irregularities in th
of Comets" in the February *Knowledge* referred to, pr
other explanations that in the main seem to me to very sa
torily account for the appearances of the tail; but he al
sumes a resisting medium and differs from me principally
manner in which he supposed this medium to act on the co

As his remarks will bear upon many of the phenomena o
ets I will here quote a portion of his paper.

"* * * Professor Barnard has noticed the detached cloud

pletely separated from the end of the tail on the 22d of October, and the rest of the tail in the photograph taken on the 22d of October is broken into fragments, indicating short spasmotic outbursts, during which matter must have been driven away in considerable quantities from the nucleus, followed by quieter intervals.

That these outbursts were of comparatively short duration is, I think, proved by the slight curvature of the tail, which indicates that the motion of the nucleus was not considerable during the time occupied by the passage of matter from the nucleus to the end of the tail.

On the other hand, the velocity of the matter of the tail away from the nucleus, does not seem to have been sufficiently great to disturb the definition of the notches on the edge of the tail in the thirty-five minutes during which the photograph was exposed, on the 21st of October.

Such rapid changes in the amount of matter driven away from the nucleus would seem to point to an irregular evolution of energy, such as might be caused by the passage of the nucleus through an irregularly distributed resisting medium, rather than to the evaporation of matter due to a steady increase of heat on approaching the Sun.

The notches and irregularities in the edge of the tail, as well as the branching structures in the photograph of the 21st of October, seem to me to point to outrushes of matter from the nucleus through a resisting medium, in different directions, which outrushing matter has afterwards been driven away from the Sun, rather than to a disturbance of the regular form of cometary tail due to an encounter of the matter in the tail with a resisting medium at a distance from the nucleus * * *"

At the request of M. Bredichin, Director of the Pulkowa Observatory, a full set of the photographs of this comet has been sent to him by the Lick Observatory through the courtesy of the Smithsonian Institution. M. Bredichin wishes to see how far the phenomena of this comet's tail will conform to his theories.

A set has also been sent to the Royal Astronomical Society

PHOTOGRAPH OF M 8, AND THE TRIFID NEBULA.*

E. E. BARNARD

In lapping my photographs in the southern part of the Milky Way, I have repeatedly secured impressions of the Trifid nebula

* Communicated by the author

and M8. The latter object which is seldom mentioned, is fa remarkable than the celebrated trifid. It is a great mass o losity mixed up with many bright stars and is a beautiful in a good telescope—the stars shining freely in the neb and especially rich in the following and fainter portion nebulosity is heaviest preceding the center. In the followin of the nebula is a conspicuous star on the following edg small very black hole.

The impressions of M8, and the trifid have always happ fall near the edge of the plates, as I was not intent on get picture of the nebulæ but to secure impressions of th clouds of the Milky Way in this region.

I have selected the best image among these of the two and have made an enlargement of it, which accompani paper. As the nebulæ are to one side of the plate the st not round but are elongated by the distortion of the field.

In this enlargement M8 is shown to be a very singular o an unequally condensed mass of nebulosity mixed with bright stars. Its southern edge is singularly well define several saw tooth-like projections. The small star and th hole in the following part of the nebula are well shown.

In a paper dated 1892, July 8, on "Photographic Nebul and Groups of Nebulous Stars" which was printed in *Astr* No. 3111, I have called attention to the appearance of these photographs.

"The singular mixture of stars and nebulosity, M8, is sh several of the photographs and is a very remarkable East and west its diameter is about 45′ and north and some 42′. The southern side is sharply defined and serrat three distinct pointed projections. From its north fol corner a wisp of nebulosity extends nearly to the group o lous stars just mentioned, and possibly with a longer ex would be found to connect with them."

A diagram of the group of nebulous stars mentioned is g the above number of *A. N.* One singular thing about patches of nebulosity is that they each inclose from two t stars.

I think the supposed connection of M8 with the group of lous stars by the nebulous wisp is real but the wisp beco faint just before it reaches the nebulosities that the actual c tion is not positive. In the enlargement this faint conn wisp is diffused and lost, though it is easily seen on the orig

In the present picture, the trifid nebula, which is the small

PLATE XXXVa.

N

S

PHOTOGRAPH OF THE TRIFID NEBULA AND M 8.

1894, July 5 $9^h\,20^m$ – $13^h\,20^m$, Willard 6-inch portrait lens of the Lick Observatory.

PLATE XXXVII. THE ASTROLABE. PLATE XXXVI

northern of the two, is very well shown and the various dark lanes that ramify it are conspicuous.

The enlargement is about 3½ times and the scale about 1 inch = 0 .5.

MT. HAMILTON, 1894, Oct. 20.

THE ASTROLABE.*

A SUMMARY.

MARGARET L. HUGGINS, LONDON ENGLAND

"I n'am but a . . . compilatour of the labour of olde Astrologiens . . .
I prey mekely every discret persone that redeth . . . this litel tretis to have my rewde endyting for excused . . .

CHAUCER [*Treatise on the Astrolabe Prologus*]

It would be difficult to say anything new at the present time about the Astrolabe: but it is by no means only what is new that is worth saying.

Much interesting and ever precious intellectual treasure is continually being lost through forgetfulness, and becomes in a sense again new if faithfully set forth once more.

We live it is true in the present, and in it we have to act: but the present is the child of the past, and intimate knowledge of its achievements and failures not only makes clearer our perception of the continuity of human effort and the progressive advance of humanity, but may often guide us with many a hint good for the present,—good for the future. *Experientia docet*: the past is concentrated experience.

The idea of this paper is quite unambitious. A perfect and very charming Astrolabe having been given to Dr. Huggins and myself, I have been led to consider the Astrolabe more closely than I had hitherto done; to look into its literature of "oldè bokis;" and to examine many Astrolabes. I have found the subject so fascinating that it seemed to me a short paper summarizing the leading historical and structural particulars relating to the instrument might be useful as well as interesting. The Summary is the outcome of study of my own; but I frankly own that I have worked into it whatever struck me in my reading as of value for my purpose. Let not anyone feel robbed! Let all, dead or living, from whom I have borrowed, be wise and call my procedure "convey-

* Communicated by the author.

ing,"—conveying, to the end that there may be, as old Thomas Browne hath it,—"no monopoly but a communit[y of] learning."

In illustrating the structure of the Astrolabe I have used [my] own instrument, partly on account of its beauty and perfec[tion,] partly because it is of a very usual size. Plates XXXVI, XX[XVII] and XXXVIII represent its back, front and four interior t[ables] or plates. It is made for about latitude 32° N. In diamet[er it] measures just 5 inches, while it has a length of a little over [8;] is ⅜-inch thick, and weighs 2 lbs. 2 oz. The material is bra[ss;] work is Persian inscribed in Arabic, and it dates probably fro[m the] beginning of the 18th century. The engraving on it is excellent[, and] the ornament introduced is skillfully relieved with slight retr[ousse] work. It is very similar indeed in design and in workmanshi[p, on] a small scale, to the splendid Astrolabe of Shah Husain, Ki[ng of] Persia, now in the British Museum, which was made A. D. [1712] (A. H. 1124) and which is not merely the only "compl[ete"] (Támm) Astrolabe known, but is also probably the most be[auti]ful one existing.

The usual material for Astrolabes was brass: but somet[imes] they were of gilded copper, or of hard wood; and sometimes [silver] inlaying was used. The size varied much. The largest I [have] seen is one in the British Museum made in London at the e[nd of] the 13th century having a diameter of about 18 inches. [The] next largest I have seen is the one of Shah Husain already ref[erred] to, the diameter of which is 16 inches. There were many [made] of 7 or 9 inches diameter: some much smaller, of 3 or 3½ in[ches.] None I believe have had a diameter less than 2 inches. Upo[n the] whole the oriental ones are the most beautiful; but m[any] European ones are also ornamented; while some oriental [ones] are very plain.

The student of aesthetics indeed finds much to delight him i[n the] study of Astrolabes. The singular suitability of the instru[ment] for artistic treatment seems to have inspired more or less e[very] maker,—even in times when most instruments were touched [with] beauty,—and it is rarely that one meets with a specimen wit[hout] some peculiar charm. The two parts upon which artistic in[ven]tion and skill were chiefly lavished were the *Kursi* and the [Rete] (see Plates XXXIX and XXXVI). But the subsidiary part[s are] often very interesting, and in some French Astrolabes the zod[iacal] signs are given in animal and human forms which are exceedi[ngly] effective. Oriental characters lend themselves of course admi[rably] to artistic effect, and in oriental Astrolabes texts from the K[oran] are often (surely most suitably) introduced.

Astrolabes are of two kinds, spherical and planispheric. This paper deals only with the latter to which the term Astrolabe in the 16th and 17th centuries was practically, exclusively applied. The general appearance and structure of the instrument will be understood from an examination of Plate XXXVI, (face) Plate XXXVII, (back) Plate XXXIX (index). It consists essentially of a single piece of some weight with a hollow on the *face side* into which fitted certain *Tables* or *Plates*, and an upper piece called the *Rete*. The weight of the Astrolabe saved the use of a plumb-line. Plate XXXIX gives an index to the particular parts. The *Rete*, which formed the face of the instrument (see Plate XXXIX), had the names of certain stars on its pointers or projecting pieces, and the *tip* of each projection was understood to give the star's centre. The *Rete* rotated, and the almacantars and azimuths of a table placed underneath it can be seen through the reticulations for fixing the positions of stars. The inner circle of the *Rete* contained the Zodiacal signs in degrees.

It is not possible in this Summary to go into minute details, but a few notes in addition to those supplied in Plate XXXIX may be acceptable. Movable indices are not found in Eastern Astrolabes, but they occur in various forms in European instruments. The *Label* (Plate XXXIX), when there was also a *Rule*, was used on the face of the instrument. The parallelogram on the back is almost universally present, but in Eastern instruments there are no lines from the corners to the pole. A figure is given in Plate XXXIX of the European *Umbra Recta and Versa* parallelogram. The parallelogram is for measuring heights of terrestrial objects accessible or inaccessible. It is obvious that for altitudes greater than 45° the base lines called *Umbræ rectæ* are used; and the perpendicular sides, the *Umbræ versæ*, for lesser heights than 45°. These lines are divided into 12 parts, and the terms *Umbræ rectæ* and *Umbræ Versæ* are borrowed from the Arabic. (For details of the method of working the parallelogram, see Chaucer's Treatise, Part II, § 41-43, Skeat's Edition of Chaucer's Works.) The upper quadrant of the central figure in Plate XXXIX contain sines and arcs for various purposes. The arrangement of the back indeed varied much. The scale of degrees on the circumference was of course an invariable feature; and so was a Zodiacal circle. But the other circles in Eastern Astrolabes might have the days of the year;—lunar mansions, etc., while in European instruments there might be circles of the Saint's days and of Sunday letters.

The *Tables*, *Climates*, or *Plates* (see Plates XXXVIII and XL) number, and also in matter. They were always thin and were

varied in used under the *Rete*—the whole series packing into the hollow of the Astrolabe, and the one required for the time being placed uppermost. On some European *Tables*, a map of the World in polar projection was given: but they usually showed projections of the sphere drawn for various latitudes. It was usual to mark the latitude for which each *Table* was constructed, under the horizon obliquus. But even without this it is easy to see at a glance for what latitude a *Table* is drawn by reckoning the degrees from the zenith to the S. point of the equinoctial or equator, or from the pole to the horizon obliquus. The vertical circles or azimuths cross of course the almucantars or latitude circles, and proceed by intervals of 10 degrees, *i. e.*, every 10th azimuth is traced. Astrological *Tables* are found on both Eastern and Western Astrolabes and relate usually to the zodiac and the planets. In Northern Astrolabes, that is those for use in northern latitudes, the tropic of Capricorn is at the border of the *Tables*. In Southern instruments the tropic of Cancer takes this place.

The term *climate* used above, it may be added, means the belt of the Earth included between two fixed parallels of latitude. In Plate XL is shown a *Table* on a small scale drawn for about latitude 52° N. The east and west of the Astrolabe are read as are dexter and sinister in heraldry, the Astrolabe being supposed to be held as is a shield. Astrolabes are classed in the east according to the number of circles or almucanters inscribed on them.

The Támm or *Perfect Astrolabe* was inscribed with 90 circles each answering to 1°. (One only is known to exist.)
The *Nisfi* or *Bipartite* with 45 circles each answering to 2
The *Thulthi* or *Tripartite*, 30 circles each = 3
The *Khumsi* or *Quinquepartite*, 18 circles each = 5
The *Sudsi* or *Sexpartite*, 15 circles each = 6

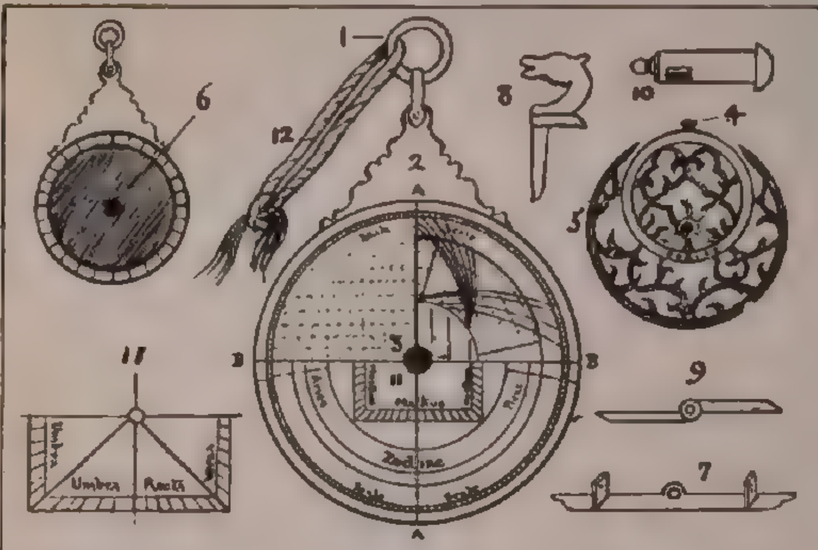
Other modes of inscription were quite uncommon.

It is impossible to handle a good Astrolabe without being struck with its comprehensiveness. It combines in itself quadrant, planisphere, theodolite, globe,—and the combination is a marvel of portability and convenience. It is easy to understand Chaucer calling it a "noble instrument;" it is easy to enter into the feeling of Blagrave who spoke of one form of it as a "mathematical jewel." Alas! that the Astrolabe is not capable of higher precision.

History.

"L'invention d'iceluy," says Jacquinot [1545] of the Astrolabe,—"les uns l'ont attribuée à Mesahalach, les autres à Ptolo-

PLATE XXXIX.

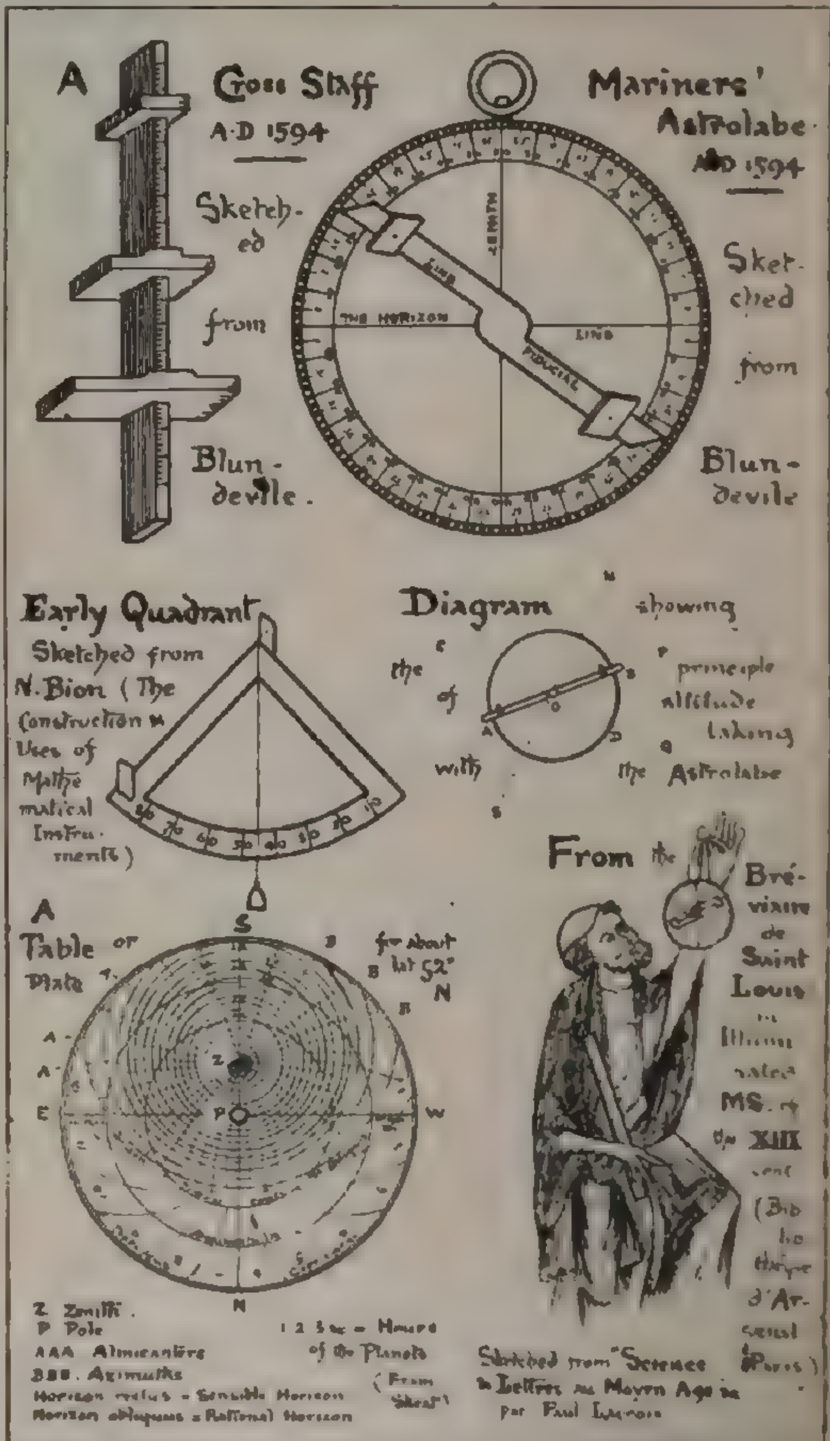


Latin	English	Arabic
1. Armilla suspensoria.	1. Ring.	1. Halkah.
2.	2.	2. Kursi.
3.	3.	3. Mahan
4.	4. Calculor.	4. Murī
5. Aranea or Volvellum	5. Rete.	5. 'Ankabūt.
6. Mater or Rotula.	6 Mother	6 Umm.
7. Alidade or Verticulum	7. Rule.	7 'Izadah.
8. Equus restringens.	8 Wedge.	8. Faras.
9.	9. Label	9
10 Axis. Clavus.	10. Pin.	10 Kutb.
11. Scala altimetra or Quadrans.	11 Umbra recta or versa	11. Mustawi & Ma'kūs.
12.	12. Cord.	12. 'Ilākah.
Tympani	Tables. Plates.	Safāih, plural of Safihah.

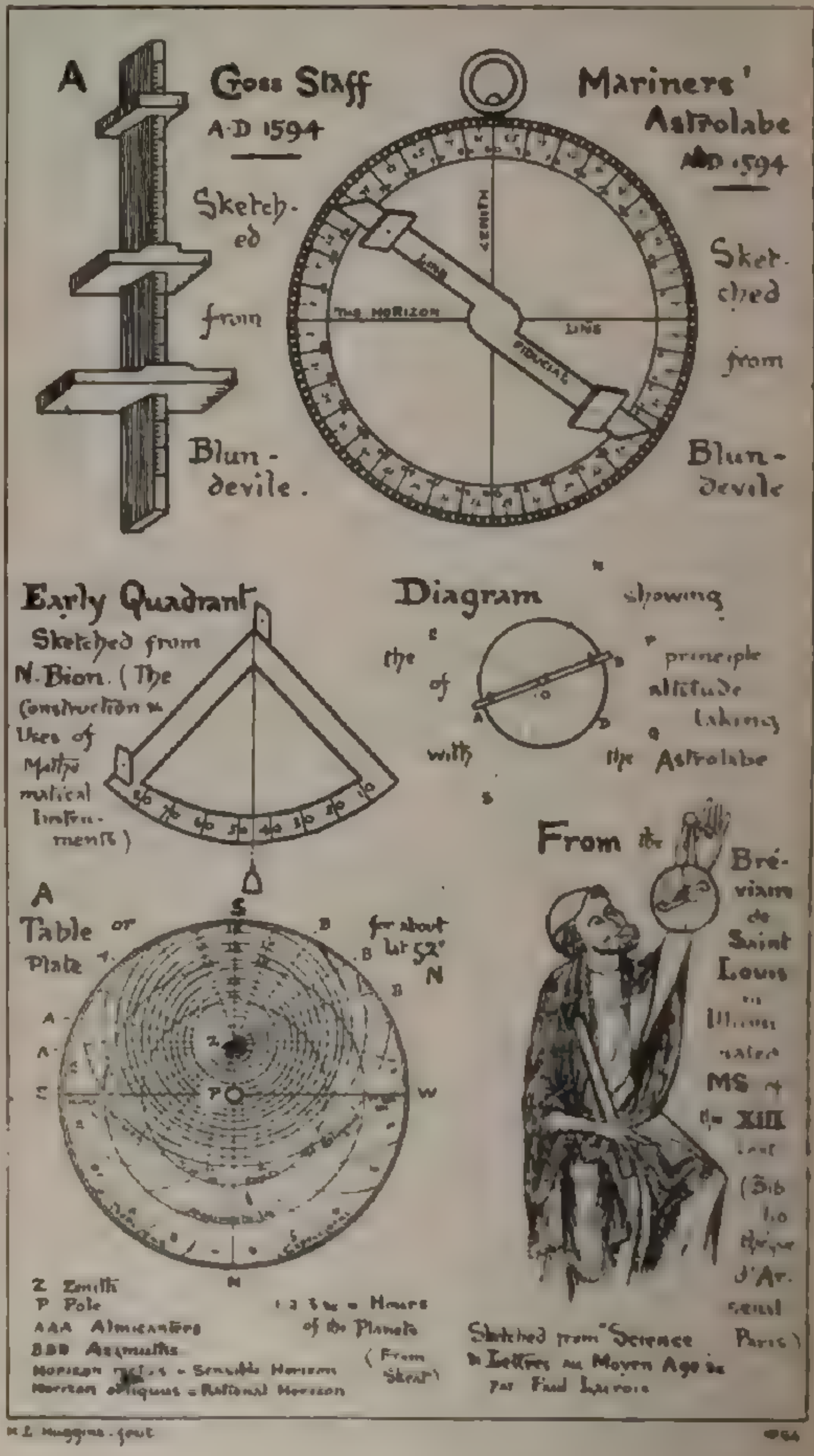
AA. Linea meridionalis. -	Line of Mid-day or South Line -	Khatt Nisf-an Nahar.
BB. Horizon rectus. -	Level Line.	- Khatt al-Istiva

Circuli progressionum -	Almicantars.	- Almucantarāt (Bridges).
Circuli verticales -	Azimuths	- Sumūt.

M. L. Huggins pinx 1894



M E Huggins, fecit 1894


A Cross Staff
A.D 1594
Sketched from Blundevile.
Mariners' Astrolabe
A.D 1594
Sketched from Blundevile
THE HORIZON LINE
FIDUCIAL LINE
Early Quadrant
Sketched from N. Bion. (The Construction & Uses of Mathematical Instruments)
Diagram showing the principle of altitude taking with the Astrolabe
A Table or Plate for about Lat 52° N
Z Zenith
P Pole
AAA Almicantors
BBB Azimuths
From the Bréviaire de Saint Louis
Sketched from "Science & Lettres au Moyen Age" par Paul Lacroix

mée, combien que longtemps auparavant avoit esté inventé d'Abraham on d'un nommé Lab, dont quelques-uns ont voulu dériver ce nom Astrolabe, comme du premier auteur."

It is amusing to read the old Frenchman's words: but even now there is no certainty as to the inventor of the Astrolabe. Ptolemy who may have got his ideas about it very probably from Hipparchus, describes a planisphere in the 8th book of the Almagest, and the planispheric Astrolabe of the Arabs is a modification of Ptolemy's planisphere; but the precise date of the first Astrolabe such as we are now considering, cannot be ascertained. It certainly, however, originated in the East, and was early used in India, Persia and Arabia. It became known in Europe by its introduction into Spain by the Arabs, and as the Arabian astronomers had excellent Astrolabes in the dark ages there is good reason for believing the instrument to be much older. The perfection of Arabic astronomical instruments about A. D. 700 was very great indeed.

We have evidence in various illuminated MSS of the astronomical use of the Astrolabe in Europe in the 13th century. A figure from one of these is reproduced on Plate XL which is of considerable interest, for not only is an Astrolabe shown in use, but the observer is represented with one of those *view-tubes* which have often—but erroneously—been taken for early telescopes. By the 14th century the Astrolabe was in general use in Europe; in the 16th and 17th centuries its use was universal. There were different forms of the instrument; but we know which one was most popular in the 16th century for Blundevile, writing in 1594, speaks thus of the Astrolabe of *Stöffler*,—"which for these hundred yeares or thereabouts, hath beene had in most price and estimation, as an instrument containing all the uses, or at least the most part of all other Mathematicall Instruments." The Astrolabe was only completely superseded at the beginning of the 18th century. The invention of Hadley's quadrant in 1731 gave it its death blow in Europe.

Europe adopted the Arabian Astrolabe. Indeed Astrolabes actually Arabian were common in Europe as late as the end of the 16th century. This is proved by the fact that E. Danti [1578] gives Arabic as well as Latin names in his tables of the fixed stars for the convenience of those who employed Arabian instruments. European makers, however, seem only to have produced Northern Astrolabes, and they very frequently added the piece known as the Label which is not found in oriental instruments. The derivation of the word Astrolabe from *ἄστρον* and *λαβη* is obvious.

In the East the instrument is almost universally named the Usturlâb.

The Astrolabe is essentially the same wherever its use has obtained. It only varies. One European variation is, however, of so much historical interest that it deserves a word of notice.

The spirit of maritime discovery stirred men mightily in the 15th century; but long ocean voyages called imperatively for improved instruments of navigation and especially for one for finding latitude easily. A rude quadrant has been used by Diego Gomez for this purpose, but the adaptation of the astronomer's Astrolabe to mariners' use by Martin Behaim in 1480 was a distinct and important advance and one which Columbus keenly appreciated. It was the complement of the advance made towards the determination of latitude by the first publication of tables of the Sun's declination in 1475 by Regiomontanus in his *Calendarium Novum*. The mariner's Astrolabe of Behaim was some 8 or 9 inches in diameter and of some thickness. A figure of such an instrument taken from Blundevile's work [1594] is given in Plate XI. A cross-staff of the same period and from the same work is also given.

The history of the Astrolabe is closely connected with that of Astrology. Nor is this surprising, considering the necessity astrologers were under of constantly observing the heavenly bodies in the practice of their calling. The handiness and comprehensiveness of the instrument could not fail to commend it to so shrewd a class of men. In the West astrology has practically disappeared. It has no basis that will bear scientific investigation. But it is well to remember that the requirements of judicial astrology led to the production of a great number of useful tables and observations. It is a fact worth noting that the first lunar tables constructed on the Newtonian Theory were intended for use in the calculation of nativities.

In the East—their birth quarter—both astrology and the Astrolabe still flourish. How amazingly they did flourish may be gathered from the statement of Marco Polo that in the city of Cambaluc alone, there were 5000 astrologers and soothsayers; and he mentions the Astrolabe as in use among them.

The amusing use of the Astrolabe described in the 161st night of the Arabian Nights is worth quoting once again because so typical of Eastern custom.

A young tailor had fallen desperately in love with the daughter of the Kádee of Bagdad and was looking forward with feverish impatience to visiting her. Naturally wishing to make the most

favorable impression he decided to shave and go to the bath beforehand. So he sent for a barber, who instead of proceeding to the shaving, after an exasperating amount of talk, "took out a handkerchief and opened it: and lo, there was in it an Astrolabe consisting of seven plates; and he took it and went into the middle of the court where he raised his head towards the Sun, and looked for a considerable time; after which he said to me (the tailor), "Know that there have passed of this our day,—which is Friday, and which is the tenth of Safar, of the year 263 of the flight of the Prophet,—upon whom be the most excellent of blessings and peace!—and the ascendant star of which according to the required rules of the science of computation, is the planet Mars,—seven degrees and six minutes; and it happeneth that Mercury hath come in conjunction with that planet; and this indicateth that the shaving of the hair is now a most excellent operation." (Arabian Nights, Lane's Trans., Vol I, p 331-2)

In the East of to-day as has been remarked the Astrolabe is frequently used. One use to which it is put by Mohammadans is to find the exact position of Mecca as regards the place of the observer. It may be remarked that Lane mentions (Arabian Nights, Vol I, p. 384, note 57) that the Arabs sometimes use a plumb-line quadrant instead of the Astrolabe. Such an instrument would be essentially that figured in Plate XL, though there was much variety in the details of such quadrants. Many had rings for suspension; and many were solid, and engraved with considerable complexity.

To English speaking people it is of great interest that their classic Geoffrey Chaucer,—the Father of English poetry—wrote *A Treatise on the Astrolabe* Of Astrolabes in Europe, I think it well to mention the following.

Two at Merton College, Oxford.

One at King's College Library, Cambridge.

Several at the S. Kensington Museum in the Oriental collections.

Twenty-eight at the British Museum. This collection, which is of great interest, includes the superb Shah Husan instrument. It is inscribed in Arabic. It is not fully perfect as it wants Rule, Horse, Fals and Cord.

Ten or eleven, some of them belonging to learned societies, are described by Mr. Morley in his work on the Astrolabe

Eight were in the Spitzer collection of antiquities recently sold in Paris, and may be studied to some extent in the fine illustrated

catalogues published of the collection.

Eleven (some very good specimens) were sold in London at Puttock and Simson's this spring.

One belongs to Mr. Knobel, F. R. A. S. This is perfect in all its parts, engraved in Kufic character, and is of the 13th century.

One is in the library at Nuremberg. This Astrolabe belonged to Regiomontanus.

One in the Bibliothéque Nationale, Paris. This was made A. D. 905 and is the earliest Eastern Astrolabe known to us I believe.

One in a private collection in France, mentioned by Morley.

Use.

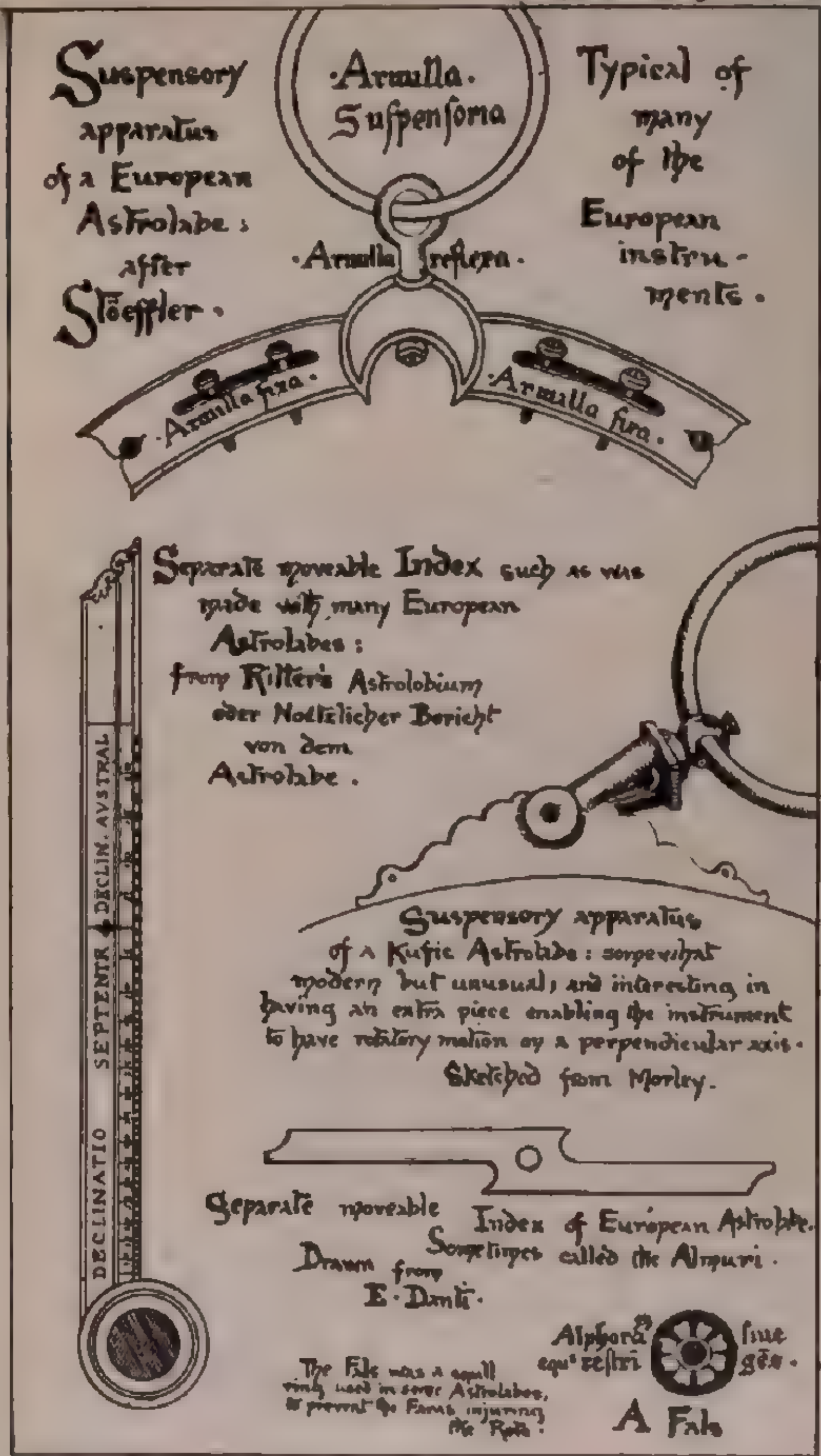
The principle upon which the use of the Astrolabe for altitude taking rests is obvious. Let the circle in the diagram (Plate XI.) be considered as a solid flat fixed in one position with Rule attached to its centre round which it is free to move. If a horizon line CD drawn upon the circle points towards a point Q in the heavens in the plane of the circle, it is obvious that by turning the Rule AB towards any object P in the plane of the circle the angle BOB will be the angle subtended by P and Q at the eye or their angular distance on a globe, and this angle may be measured if the circumference of the circle be graduated: Thus if the plane of the circle pass through the poles N and S, and CD point towards the equator, then when the Rule points towards P, NOB, its N polar distance, or BOD, its declination, may be measured.

The mode of using the Astrolabe recommended by Chaucer is as follows:

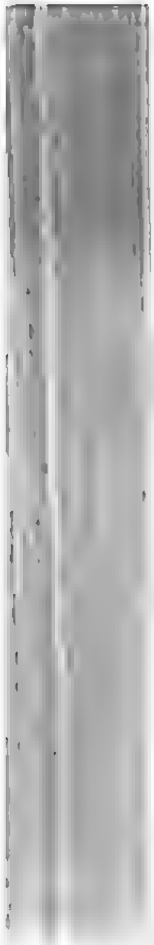
"Put the ring of thyn Astrolabie up-on thy right thoumbe and turne thy left syde agayn the light of the sonne And remeve thy rewle up and doun, till that the stremes of the sonne shyne thorgh bothe holes of thy rewle. Loke thanne how many degrees thy rewle is areised fro the litel crois (east point) up-on thy est, and tak ther the altitude of thy sonne."

When the Astrolabe was to be used at sea "the observer was instructed to sit down and place himself with his back to the mainmast, hold the Astrolabe by the ring, hanging on the 2d finger of the left hand and move the *alidada* or *Label* up and down with the right until the Sun was on with both sights" (Markham).

The Sun was never observed directly through the sights. The stars were. In the better Astrolabes there were two pair of holes



H. L. Huggins fecit.



in the sights; a very small pair for solar observations; a larger pair for stellar ones.

Briefly, the Astrolabe was used for taking observations of the Sun, Moon and stars; for determining latitude; for determining approximately the points of the compass; for determinations of time; for measuring heights of mountains, buildings, and distances generally; for construction of horoscopes, and for many other operations connected with judicial astrology. In the East, as has been stated, one of its very important uses was, and is, to find the exact position of Mecca as regards the place of the observer.

Bibliography.

The best catalogue of works on the Astrolabe with which I am acquainted is that forming part of the *Bibliographie Générale de l'Astronomie* by *J. C. Houzeau* and *A. Lancaster*, pp. 630-649.

It appears to me that the most useful works on the whole to consult, are the following:

G. Chaucer, [1391] *Treatise on the Astrolabe.*—Edition by W. W. Skeat in his edition of the Complete Works of Chaucer. Skeat's introduction, notes and illustrations to the treatise are all of the utmost value. A small book on Chaucer's Treatise by A. E. Brae is of interest.

J. J. Stöffler, [1512] *Elucidatio Fabrice ususque Astrolabii, etc.*—This work is very full and complete and has many excellent illustrations. It is too a beautiful book as regards printing, etc. There is a French translation of Stöffler's book by J. P. de Mesmes (published 1556). This work contains the text of Stöffler and the most necessary illustrations.

M. Blundevile, [1594] *His Exercises.*—Interesting and useful, and has good plates.

F. Ritter, [1599] *Astrolabium das ist, etc.*—Useful. Excellent plates.

W. H. Morley, [1856] *Description of a Planispheric Astrolabe constructed for Shah Husain, King of Persia.*—This work is much more than an admirable and exhaustive monograph on Shah Husain's Astrolabe. It contains besides an excellent account of Astrolabes generally, and has many useful notes. It is an invaluable work. The plates give Shah Husain's Astrolabe full size. This unfortunately makes the book awkward and unwieldy to use, but this is its only fault.

An account of the Astrolabe based on oriental information will be found in the *Voyages du Chev. Chardin en Perse* [1811] Tome IV, p. 335 *et seq.*

Among minor works may be mentioned an article in the *English Encyclopædia*, unsigned, but which I venture to think must have been written by the late Professor De Morgan.

Columbus by Clements Markham contains a little respecting the Mariner's Astrolabe.

ON THE QUADRUPLE STAR ζ CANCRI.

PROFESSOR SEELIGER

The results of numerical computations of the kind which I have published in my two papers upon ζ Cancri*, bear more or less the character of interpolation formulæ. It is not possible to determine their constants with so great exactness that a completely satisfactory accordance with the observations of future years can be guaranteed. This fact I have expressly insisted upon, especially in II.

On this account it is on the one hand not surprising if the places deduced from theory depart slightly from the observations after a time, and on the other hand, it is useless to make continuous readjustments of the theory at short intervals, for the purpose of reconciling it with the observations, if the characteristics of the latter results are already found in the earlier.

At the time of the discussion in I, observations up to the year 1880 were at my disposal, while in II, measures to 1888, though, it is true, partly in insufficient number, could be used.

The warrant for the second research, therefore, lay not so much in the increased observational matter as in the circumstance, that I could then more fully and thoroughly develop the theory in several directions, and considered that by this discussion, I could arrive at a certain conclusion.

I cannot therefore look upon the appearance of new observations as a challenge to a present renewal of my discussion of ζ Cancri.

On the other hand, there have appeared, of late years, attacks by Mr. Burnham, a meritorious double-star observer, upon my theory of the motion of the distant star C, which are calculated to obscure the subject in the eyes of those not well acquainted with my papers.

The arguments advanced with great confidence by Mr. Burnham are indeed calculated only to show that their author neither possesses sufficient knowledge of the subject nor has given himself the trouble to thoroughly examine my papers. On this score I could be content to leave the proper characteriza-

* *Untersuchungen über die Bewegungsverhältnisse in dem dreifachen Sternsystem ζ Cancri.* Denkschriften der Wiener Akademie 1881.

Fortgesetzte Untersuchungen über das mehrfache Sternsystem ζ Cancri. Abhandlungen der k. bayer. Akademie 1888.

In the following pages the first will be designated by I, the second by II.

tion of Mr. Burnham's assertions to the future and to others. On the other hand, I cannot consent to allow the results of my labors to be brought into question through entirely unfounded assertions, and therefore I have brought out in the following pages certain computations, which I consider should be sufficient to put the case in a full light in the eyes of all.

In the following pages I will concern myself singly and alone with the motion of the distant companion C, about the centre of gravity of the two inner stars A and B. Special researches in I and II have arrived at the result, that the observations show no noticeable difference between the centre of gravity of A and B, and the middle point between the two stars, $\frac{AB}{2}$. Furthermore, undulations show themselves in the observations of C, in a manner completely accordant both in position-angle and in distance, which show the position-angle to oscillate about $\pm 2°$, and the distances about $\pm 0''.2$, about a mean value. The constant period of oscillation amounts to very nearly 18 years, and it has been possible to prove the same by more than three full revolutions. The entire phenomenon is fully explained on the assumption that the star C has a near dark companion.

The yearly means from 1880 forward, discussed in II, were not sufficient for a complete confirmation, which is now made possible by the publication of several valuable series of observations. From the year 1888 forward, however, the establishment of reliable yearly means would have been to a certain extent impossible, had not Messrs. Schiaparelli, H. Struve, and Lovett had the great kindness to communicate to me the desired extracts from their records. Especially do the very numerous and excellent measures of Schiaparelli form an essential support of the following remarks. It was thus possible to adduce 15 new and certain yearly means, from 1880 to 1894. After the above remarks, there can be no question of connecting these new results with the earlier computations. On the contrary, I consider that such attacks as those of Mr. Burnham can best be met by treating the newer measures, which have almost no personal connection at all with those made in the forties or fifties, entirely by themselves. After I have made it apparent that my earlier formulas accord with the new observations as closely as can at all be expected, I shall show that the newer measures also, considered by themselves, in turn fully satisfy the assumption of a dark companion to C, corresponding, in fact, quantitatively, to the earlier formulas, and that without such an assumption, residual errors

remain which no one, certainly, will seriously attempt to exp by the accumulation of personal errors

I now proceed to give the annual means of the observatio C, referred to the middle point of A and B. The names of observers, Hall, Sr., Jedrzejewicz, Schiaparelli, and Herm Struve, will be designated by the abbreviations, H., J., Sp., H

1800 +	Observer	Wt	p	ρ	Mean
				"	
80.16	Franz.	2	130.00	5.545	
80.21	H.	4	132.45	5.405	1880.21 (11.10) 132.25 5
80.22	J.	4	132.50	5.193	
80.31	Seabroke	1	133.72	——	
81.24	J.	4	131.29	5.467	
81.25	Doberck.	2	131.75	5.400	
81.26	Seabr.	1	131.37	——	1881.28 (17.16) 131°.42 5'
81.28	O. Σ	2	130.80	5.210	
81.30	Sp.	4	131.57	5.445	
81.30	H.	4	131.54	5.508	
82.20	H.	4	132.03	5.587	
82.25	Seabr.	2	129.83		1882.24 (14.13) 131°.13 5'
82.26	Sp.	4	131.00	[illegible]	
82.27	J.	4	130.94	5.497	
83.13	Engelm.	4	129.43	5.653	
83.29	Sp.	4	130.16	5.508	
83.31	H.	4	[illegible]	5.567	1883.26 (15.14) 129°.73 5'
83.32	Seabr.	1	128.33	——	
83.35	Kustner	2	129.20	5.557	
84.21	Perrotin.	2	128.05	5.561	
84.25	Sp.	4	129.21	5.589	
84.26	O. Σ.	4	129.22	5.318	
84.28	H.	4	129.50	5.025	1884.28 (17.14) 128°.63 5'

1800 +	Observer.	Wt.	p	ρ	Mean.
88.25	H.	4	124.13	5.520	1888.29 (17.15) 124°.27 5″.625
88.27	Sp.	4	124.31	5.684	
88.28	Smith.	2	125.20	(4.65)	
88.33	O. Σ.	[illegible]	123.84	5.667	
88.33	H. Σ.	4	124.01	5.610	
88.36	Maw.	1	124.70	5.790	
89.12	Seabr.	1	125.30	(4.85)	1889.22 (18.17) 123°.79 5″.534
89.18	Highton.	1	124.60	5.310	
89.19	Leavenworth	2	123.80	5.530	
89.22	Sp.	4	123.01	5.536	
89.23	H. Σ.	4	123.51	5.590	
89.23	H.	4	123.91	5.680	
89.29	Maw.	2	124.50	5.240	
90.23	Sp.	4	123.43	5.514	1890.28 (13) 123°.51 5″.507
90.24	Comstock.	1	124.80	5.470	
90.28	H.	4	123.51	5.432	
90.33	H. Σ.	4	123.26	5.580	
91.21	Sp.	4	122.43	5.527	1891.26 (13) 122°.76 5″.490
91.22	H.	4	122.87	5.479	
91.26	Maw.	2	122.50	5.300	
91.27	H. Σ.	2	122.31	5.570	
91.65	Byers & Collins	1	125.10	5.610	
92.26	Sp.	4	122.40	5.443	1892.26 (4) 122°.40 5″.443
93.16	Jones	1	(115.70)	5.320	1893.24 (6.7) 122°.54 5″.288
93.21	Lewis	2	123.70	5.185	
93.25	Sp.	4	121.96	5.331	
94.16	H. Σ.	4	122.39	5.430	1894.19 (10) 122°.46 5″.405
94.16	Lovett.	2	123.10	5.540	
94.24	Sp.	4	122.22	5.313	

On this collection I would remark as follows:

1. The assignment of weights has been made according to the scheme set forth in II. This is at any rate sufficient for the purpose proposed, although, by its use the new observations, made with such superior means, are certainly assigned too small a weight.

In a definitive discussion, the yearly means of Schiaparelli, made from very numerous and apparently very accurate nightly means, should be assigned a greater weight. The yearly means of Schiaparelli are, according to the series, formed from 13, 12, 14, 8, 10, 10, 14, 14 evenings' work.

2. The reduction of the measurements taken from A or B to C, to $\frac{A+B}{2}$ has been carried out with the aid of assumptions as to the relative positions of A and B, which are not entirely certain, and could not be exactly determined without more considerable

computations. This inexactness, which besides is scarcely perceptible, can only affect the last places in the observations of HΣ, in the above figures.

3. With regard to the constant personal errors, the following corrections were obtained by comparison with the ephemeris given in II, p. 71.

Sp	−0°.85	+0″.074
H	+0 .51	−0 .025
HΣ	−0 .41	+0 .097

The last observation of HΣ seems, however, to contradict this correction. It is separated from the earlier ones by a three years interval, and is derived from 6 comparisons of C with A and B which give entirely accordant mean values. It therefore seems better to leave this last measure of HΣ uncorrected. Besides, it appears pretty plainly at the first glance, that on this account the last yearly mean is uncertain as to distance, and this will be found confirmed as we proceed.

4. The measures of OΣ from the year 1881, and those made with the 30-inch refractor, are taken directly without correction from the appendix to II.

The grounds for this proceeding I leave unexplained, as it cannot have a perceptible effect on the results. All other observations are, as before, left uncorrected.

As respects the constant personal errors applied in II, and in the continuation of it in the present notice, they make no pretence at being anything more than the mean of the departures from the ephemeris, which in its turn is deduced from provisorily corrected observations. On this account the observations appear to be referred to a more or less arbitrary system of position-angles and distances.

The corrections found, however, may be regarded as actual personal equations, if the mean of all the applied corrections does not differ noticeably from zero, otherwise the adopted system is not the normal one.

The corrections employed in II and the present paper, if where different corrections have been found for the same observer, the simple means are used (with the exception of Sp., in whose case on account of the great disparity in the instrument, this is hardly allowable), are then,

W. Σ	+1°.82	−0″.070
O. Σ	0.47	0.125
D.	+0.84	0.011
S.	+0.02	−0.064
J.	−0.26	0.031
Mädler	+0.30	——

	°	″
Du.	− 1.56	− 0.050
Sp. I	+ 1.04	+ 0.004
Engelmann	+ 1.38	+ 0.217
J	+ 0.71	− 0.103
Kaiser	+ 1.79	0.289
Sp. II	0.85	+ 0.074
H	+ 0.31	− 0.025
H. Σ.	− 0.41	+ 0.097
Mean	+ 0.35	- 0.024

If we take the most certainly determined corrections, namely, W Σ., Δ, Sp_{I}, Sp_{II}, H, H Σ, we get the mean value $+0^{\circ}.31 + 0''.008$. According to this the system should give position-angles somewhat too great, while the distances correspond very nearly to the truth. As, however, a constant correction to the angle has no influence on the theory, the selected system must be regarded as nearly normal. Besides, this positive correction is due principally to the unusually large correction to W Σ.

In any case, there is no apparent ground for doubt that my researches in II are based upon observations which have been referred to an essentially correct system, although the possibility is not excluded that in the future, when it is to be hoped that many of the large telescopes which now stand prepared for the work will be employed on the measures of ζ Cancri, a modification may be made in the one or the other direction.

The above quoted annual means were next, to facilitate comparison, reduced to the same tenth of their respective years. Attention should, however, again be called to the circumstance, that the last places, to the hundredths of degrees in position-angle, and the thousandths of seconds in distance, are uncertain to a few units; this results from the manner in which they have been computed. To the fifteen newly deduced yearly means I have prefixed the four next preceding from II. I am not in a position to undertake any alteration or completion of these.

To begin with, the observed position-angles and distances, p_{B}, and ρ_{B}, should be compared with the values resulting from the theory, p_{R} and ρ_{R}.

The differences observed—computed are found in the collection given below, in the column O—C. A merely passing glance at these figures shows that on the whole the accordance with the theory is satisfactory.

The peculiar undulations, at any rate, which the observations show in p and ρ have almost entirely disappeared. Outstanding differences remain, of a systematic character indeed, but which

for the objective critic, have no longer any striking significance, since systematic errors are to be expected in the observations, and besides, it is a question of an extrapolation of some ten years. In consideration of this, the accordance between theory and observation must be called satisfactory.

The sums of the absolute differences amount in position-angle to 8°.56, in distance to 0″.769.

The undulations which the observed p and ρ show, and which, as I have shown in II, follow with almost mathematical regularity a cycle of about 18 years, come out more plainly if we seek to represent them by a circular motion. It is well to note here that the whole matter is a question of a progressive change of only about 9° in position-angle. If we take this progressive change of p according to II, and make the equation

$$p_o = 145°.46 - 0°.513\ (t - 1850.2),\ \rho_o = 5''.459$$

expressing the simple mean value of all ρ_B, found without regard to weight, the differences $p_o - p_B$, $\rho_o - \rho_B$, are very nearly what was above regarded as the outstanding errors of the observations if the assumption that the star C is not double, is admitted.

Differences of a similar character have now begun upon their fourth repetition. It is repugnant to me, in view of these figures, to criticise the contention of Mr. Burnham, that such departures should be considered as a remarkable accumulation of personal errors, now occurring in the same order for the fourth time. To criticise the like assertions with a parliamentary expression is scarcely possible.

	p_B	ρ_B	p_R	ρ_R	O—C		p_o	$p_o - p_B$	$\rho_o - \rho_B$
	°	″	°	″	°	″	°	°	″
1876.2	130.51	5.317	130.01	5.281	− 0.40	+ 0.036	132.12	+ 1.61	+ 0.162
77.2	131.15	258	131.21	255	− 0.06	+ 0.003	131.60	+ 0.45	+ [illegible]
78.2	131.42	295	131.57	259	− 0.15	+ [illegible]	131.09	− 0.33	+ 0.174
79.2	132.62	258	131.86	244	+ 0.76	[illegible]	130.58	2.04	+ [illegible]
80.2	132.25	372	131.05	351	+ 0.30	+ 0.021	130.06	− 2.19	[illegible]
81.2	131.46	425	131.76	422	− 0.30	+ [illegible]	129.55	− 1.91	+ [illegible]
82.2	131.10	514	131.28	494	− 0.12	+ 0.020	129.03	− 2.13	− [illegible]
83.2	129.79	597	130.54	581	− 0.75	+ 0.036	128.52	− 1.27	− [illegible]
84.2	128.72	543	129.59	615	− 0.87	− 0.072	128.00	− 0.72	− [illegible]
85.2	128.19	642	128.49	651	0.30	− 0.009	127.49	− 0.70	− [illegible]
86.2	127.06	591	127.32	667	− 0.26	− 0.076	126.99	− 0.07	− 0.122
87.2	126.04	598	126.12	605	− 0.08	− 0.007	126.48	+ 0.44	− [illegible]
88.2	124.38	627	124.93	642	0.55	− 0.015	125.96	+ 1.58	− [illegible]
89.2	123.81	535	123.84	601	− 0.03	− 0.066	125.45	+ 1.64	− [illegible]
90.2	123.58	511	122.91	554	+ 0.67	− 0.043	124.93	+ 1.35	− [illegible]
91.2	122.80	434	122.20	475	+ 0.60	+ [illegible]	124.42	+ 1.62	− [illegible]
92.2	122.43	447	121.75	403	− 0.68	+ 0.044	123.90	+ 1.47	+ [illegible]
93.2	122.51	360	121.60	335	+ 0.91	− 0.045	123.39	+ 0.88	+ [illegible]
94.2	122.46	409	121.72	287	+ 0.74	+ 0.122	122.87	+ 0.43	+ [illegible]

In order to present clearly the decisive fact, that the last quoted nineteen yearly means, to which so many and distinguished observers, who do not appear previous to 1876, have contributed (and attention is here called to their extremely close agreement, especially in position-angle) accord with the same period that is deduced in II from the assemblage of the earlier available measures, I have attempted to represent the differences $p_\circ - p_n$ by a formula expressing a circular motion with the period named, by the use of the method of least squares.

The result was

$$\text{(II).} \quad p_\circ - p_n = -0^\circ.06 + 1^\circ.913 \sin 19^\circ.947t + 0^\circ.137 \cos 19^\circ.947t$$

in which the time t is to be expressed in years from 1850.2. If we wish to get a still better approach, we change the variation of $p_\circ$ somewhat, so that the equation reads

$$\text{(III).} \quad p_\circ - p_n = -0^\circ.06 - 0^\circ.0681 \frac{t}{(10)} + 2^\circ.268 \sin 19^\circ.947t + 0^\circ.286 \cos 19^\circ.947t$$

In the following table the values of position-angles computed from these formulæ are given under II and III, and the outstanding errors are under Δ and Δ_1. The representation attained is a satisfactory one, and indeed, no other was to be looked for. The systematic character of the differences, it is true, can be in part eliminated by a more elaborate theory, possibly the one earlier proposed by myself, and by a discussion carried through with reference to the weights, but for the most part, they are purely personal errors that are here expressed. In my earlier papers I have expressly called attention to this point. The sums of the absolute values of Δ and Δ_1 are from $5^\circ.58$ to $6^\circ.05$; from this it appears hardly necessary to give the formula III any preference over II.

	$p_\circ - p_n$	II	Δ	III	Δ_1	a	Δ_2
	°	°		°		″	″
1876.2	+ 1.61	+ 0.51	+ 1.10	+ 1.10	+ 0.51	5.493	− 0.031
77.2	+ 0.45	− 0.15	+ 0.60	+ 0.25	+ 0.20	130	0.023
78.2	− 0.33	− 0.49	+ 0.16	− 0.21	− 0.12	495	+ 0.003
79.2	2.04	1.26	0.78	1.29	0.75	591	0.071
80.2	2.14	1.77	− 0.42	1.81	0.38	456	0.006
81.2	− 1.91	1.97	+ 0.06	− 2.08	+ 0.17	449	− 0.013
82.2	− 2.13	− 1.93	− 0.19	2.15	+ 0.02	473	+ 0.013
83.2	− 1.27	1.60	+ 0.32	1.80	+ 0.53	430	+ 0.037
84.2	− 0.72	− 1.24	+ 0.52	1.30	+ 0.58	361	− 0.063
85.2	− 0.70	0.65	− 0.05	− 0.45	− 0.25	468	+ 0.006
86.2	− 0.07	0	− 0.07	+ 0.07	− 0.14	140	− 0.092
87.2	+ 0.44	+ 0.66	− 0.22	+ 0.77	− 0.33	420	− 0.033

	$\rho - \rho_n$	II	Δ	III	Δ_1	a	Δ
1888.2	+ 1.58	+ 1.22	+ 0.36	+ 1.35	+ 0.23	[illegible]	+ [illegible]
89.2	+ 1.64	+ 1.63	+ 0.01	+ 1.73	[illegible]	[illegible]	[illegible]
90.2	+ 1.35	+ 1.84	− 0.49	+ 1.57	[illegible]	[illegible]	+ [illegible]
91.2	+ 1.62	+ 1.82	− 0.20	+ 1.73	0.11	[illegible]	+ [illegible]
92.2	+ 1.47	+ 1.60	− 0.13	+ 1.32	+ 0.15	547	+ [illegible]
93.2	+ 0.85	+ 1.12	− 0.22	+ 0.70	+ 0.15	434	− [illegible]
94.2	+ 0.43	+ 0.43	0	+ 0.08	− 0.35	(582)	(+ [illegible])

A not unimportant control, if indeed such is needed, is further afforded by the discussion of the distances. The formula II, if it is assumed that C describes a circular orbit about a dark companion, gives naturally very accordant changes of distance.

If we take a as the radius of the circle which the centre of gravity of C and its companion describes about $\frac{AB}{2}$ we can compute from each ρ_n a value for a. The assembled a, with the differences $\Delta_1 = 5''.462 - a$, where $5''.462$ represents the simple mean, without regard to weights, give an entirely independent confirmation of the theory. If we look at the small residuals, ($\Sigma\Delta_1 = 0''.600$ to $0''.720$) we can not remain in doubt that all the larger periodical undulations in the distances have entirely disappeared.

I have always, in both my earlier papers, looked upon this control as a very important support to the almost demonstrative certainty of the assumption, that C must have a dark companion. I can further only here repeat what I said in II (p. 14) about this assumption: "I for my part do not hesitate to claim for it a certainty so great as is attained by few attempts at explanations in stellar astronomy, which are not at once indicated on inspection."

In conclusion, I must once more, though unwillingly, return to Mr. Burnham. After I had expressly controverted the attacks of this gentleman,† and had shown how peculiar are the ideas he has formed respecting systematic observational errors, Mr. Burnham has seen fit, in No. 120 of the periodical ASTRONOMY AND ASTRO-PHYSICS, not only to reiterate his assertions, but to do this in a tone which I must most decidedly qualify as entirely improper. There is not the least occasion to go into his arguments, since these have been completely refuted in my former papers. I cannot, however, refrain from here reprinting No. III of Burnham's last named notice, since the author's peculiarity herein characterises itself. "It is evident that Professor Seeliger has had little experience in double star work, or he would not have com-

† *Über Herrn Burnham's "Invisible Double Stars" und insbesondere über ε Hydrae. Astronomische Nachrichten, band 132.*

cised my remark that the close pair of ε Hydræ could not possible effect the measures of C. The truth of this statement must be so obvious to every practical astronomer who is accustomed to use the micrometer, that it can hardly be considered a debateable question " I can only express my sincere regret that a practical astronomer should be so surprisingly ignorant of the conditions which must necessarily be considered as indicated by systematic differences, and still more that he should put this ignorance in evidence in so conspicuous a manner

It naturally follows from this, that no scientific gain can result from any discussion with Mr. Burnham upon the present question. I shall therefore decline to consider any further remarks by this gentleman upon ζ Cancri and my papers, as of no consequence in the case, and shall allow any renewed attacks to remain unanswered.

[Mr. Burnham, at whose request the foregoing article by Professor Seeliger is reprinted in this Journal, desires to state that he finds nothing in this paper with reference to its astronomical assertions or personal peculiarities which calls for, or would justify any reply on his part. For determining the true relations and movements of stellar systems, the micrometer, he thinks, is mightier than the pen.—Ed.]

THE GREAT PHOTOGRAPHIC NEBULA OF ORION, ENCIRCLING THE BELT AND THETA NEBULA.*

E. E. BARNARD

Experiments With a Very Small Lens in Photographing Very Large Nebulæ, etc.

I have recently been experimenting with a small short-focus lens. Some of the results are very interesting.

This lens belongs to a cheap (oil) projecting latern and is 1½ inch in diameter and 3½ inches focus (from the rear lens). It gives a field of about 30°, only one-half of which, however, is at all flat—but on this portion the stars are fairly good. The scale is about 10°.3 to the inch.

The ratio of the aperture to the focal length is 1 : 2.3 while that of the Willard lens is 1 : 5.

This large light ratio makes the lens very suitable for certain

* Communicated by the author

work where the smallness of the scale is not objectionable—or is really desired,—such for instance as very large diffused nebulosities, large comets, the Milky Way, etc. It will doubtless be also admirably suited for photographing meteors—catching from its great light ratio and large field many meteors that would be entirely missed by such telescopes as the Willard lens.

So far I have made nearly 20 photographs with this lens, which for identification I shall call the "lantern lens."

These exposures range from one second up to four hours.

On account of this light ratio the diffused light of the stars scattered over the sky also photographs, so that very prolonged exposures are only possible with it when the sky is free from milkiness—or whiteness. Its penetrating power is not far from that of our Willard lens.

When the moon is very young, the dark or earthlit portion can be photographed with it in from 1 second to 3 seconds.

The cloud forms of the Milky Way, such as those in the region of M11, are well shown in from 10 to 15 minutes.

An exposure of one hour showed all the great mass of nebulosity near Alpha Cygni, and doubtless 15 or 20 minutes would show it clearly.

One hour showed the full extent of the great Andromeda nebula, and I have no doubt but that it could as well be shown with far less than half that time.

Four hours' exposure was given on the region about the Pleiades. Besides showing the nebulosities of the cluster it showed also the large diffused nebula N. G. C. 1497. This nebula, which was discovered by me with the 6 inch Cooke equatorial of the Vanderbilt University, Nashville, Tenn., on Nov. 3, 1885, was photographed by Dr. Archenhold in October, 1891 (See A. N. 3082). I have a fine photograph of it with the Willard lens with three hours' exposure, and of which I shall have more to say in a later paper. The impression with the lantern lens is very strong and does not materially differ (except in point of size) from that with the Willard lens. The nebula is a very singular object, however, and well worth study. It is somewhat over 2° long and seems to be quite complicated in structure.

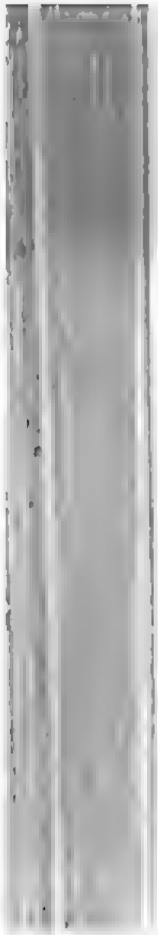
Probably an exposure of less than half an hour would show this object with the lantern lens. By the way, Dr. Archenhold is wrong in speaking of this object as having been discovered by photography. Visually, on account of its very diffused nature, it is a very faint object in any telescope.

The most interesting, however, of these lantern lens pictures,

PLATE XLI.

THE GREAT PHOTOGRAPHIC NEBULA IN ORION.

ASTRONOMY AND ASTRO-PHYSICS, No. 130.



are two of the constellation of Orion (for it takes in nearly the entire constellation).

These were made 1894, Oct. 3 and Oct. 24, with 2 hours', and 1 hour 15 minutes' exposure, respectively.

To my surprise these pictures showed an enormous curved nebulosity encircling the belt and the great nebula, and covering a large portion of the body of the giant. A description of this nebula would not only be complicated but it would fail, also, to give any impression of its form and magnitude; I have, therefore, made the enclosed drawing of it which will show at once its exact location and form. The drawing is on nearly twice the scale of the original negative and the stars are taken from Proctor's Chart.

After I had made this drawing and partly written this paper, I remembered having seen somewhere that Professor W. H. Pickering had once spoken of a great nebula shown on his photographs of Orion and previously unknown. I have looked up his paper on the subject and find it in the *Sidereal Messenger* for January, 1890 (vol. 9, p. 2). I will quote here what Professor Pickering has to say concerning this remarkable object:

"An interesting structure brought out upon our plates is a large spiral nebula whose outer extremity starts in the vicinity of γ Orionis. It passes about four degrees north of ζ, extends to ν thence to β, then north to η, with an outside stream lying nearly north and south, and preceding β about four degrees. Another stream lying nearly east and west precedes η about the same amount. This nebula is about seventeen degrees in length, by nearly the same in breadth, and surrounds a cluster of bright stars including the belt and sword handle, and extending towards γ. The region containing the nebula is noticeably lacking in stars brighter than the eighth magnitude, but contains the very bright stars γ and β. It is possible that a plate with double our present exposures, which we are soon going to try, will fill the space between η and ζ, thus making the great nebula the inner termination of the spiral. This nebula is shown by three different exposures and is very distinctly marked."

Professor Pickering's photographs were made at Wilson's Peak in southern California (altitude 6250 feet) with a Voightländer portrait lens of 2.6 inches aperture and 8.6 inches equivalent focus, with an exposure of three hours. Stars from the 11th to the 12th magnitude were well shown.

In the present pictures the shorter exposure shows the nebula best; this was perhaps due to a darker sky.

On my drawing, I have marked a portion of the nebulosity, from 1° to 2° east of Tau, with dots, as it is so feeble at this point that I cannot be certain of it. Two other portions, very slightly uncertain, I have also marked with dots; these, however, I am confident exist on the negatives. The rest of the nebula is well shown. It is brightest near 56 and 60 Orionis. Its extreme diameter is about 14° or 15°. Compared with this enormous nebula the old θ, or so-called "great nebula," is but a pigmy.

That this object shown on my plates is the same photographed by Professor Pickering in 1889 there is no doubt, as will readily be seen upon comparing his description with my drawing. The present photographs therefore, fully confirm the pictures of 1889. This confimation is all the more valuable as it was unconsciously and independently made.

MT. HAMILTON, 1894, Oct. 27.

MARS.

PERCIVAL LOWELL.

SEASONAL CHANGES ON THE PLANET'S SURFACE.

That seasonal changes take place upon the surface of Mars, changes of sufficient magnitude to be visible from the Earth, and due directly to the changing seasons of the Martian year, I have recorded in previous papers. In view, however, of the importance of the subject and of the fact that each fresh presentation has added to the evidence of this it seems to me well to embody the matter in a paper by itself, supported by drawings made at different dates and placed for comparison side by side. In these I have reproduced as nearly as possible the values of the markings on the planet on an absolute scale, so that the various parts of the different drawings are all comparable.

For the substantiation of changes on the surface of Mars it is of par-amount importance that the drawings to be compared should all have been made by the same person at the same telescope under as nearly as possible the same atmospheric conditions,—since otherwise the subjectivity of the observer, the objectivity of his instrument and the special atmosphere in which he works play so large a part in the result as to mask that trifling factor in the case, any change in the planet itself. How easily this masking is accomplished appears from drawings by different observers of

the same Martian feature at practically the same instant of time. Several interesting specimens of such personal peculiarities may be seen by the curious in Flammarion's admirable thesaurus "*La Planète Mars.*" In some of these drawings purporting to be likenesses of the planet it is pretty certain that Mars would never recognize himself.

To have drawings swear at one another thus across the page is, in the interests of deduction, objectionable. If Mars is to be many, his draughts—man must be one. So much, at least, is fulfilled by the drawings presented with this paper. For they were all made by me at the same instrument under the same general atmospheric conditions as the same personality enters all, it stands, as between themselves, eliminated from all to increased certainty of deduction. Even the different eye-pieces used vary chiefly in a manner to minimize, if anything, and so emphasize the differences observed. For with increasing image the higher power used tends to decrease the contrast. The result is that it largely offsets the difference in contrast due to nearer approach and leaves simply a case of magnification, with the values untouched.

Since, furthermore, the drawings were all make in the months preceding and following one opposition, secular changes are practically out of the question; any changes that appear starting with the presumption of a seasonal character. They constitute of themselves a kinematical as opposed to a statical study of the planet's surface.

The resulting phenomena are, as will be seen from the drawings, much more evident than might be supposed. Indeed they are quite unmistakable As for their importance it need only be said that deduction from them furnishes, in the first place, strong inference that Mars is a very living world subject to an annual cycle of surface growth, activity, and decay; showing in the second place that this Martian yearly round of life must differ in certain interesting particulars from that which forms our terrestrial experience.

The phenomena evidently make part of a definite chain of changes of annual development. So consequent and, in their broad characteristics, apparently so regular are these changes that I have been able to find corroboration of what appears to be their general scheme in drawings made at previous oppositions. In consequence I believe it will be possible in future to foretell, with something approaching the certainty of our esteemed weather bureau's prognostications, not indeed what the weather will be on Mars, for it seems doubtful if Mars has much of what

we call weather to prognosticate, but the aspect of any part of the planet at any given time.

The changes in appearance presented by the planet here to be described refer primarily not to the melting of the polar snows, except as such melting forms the necessary preliminary to what follows, but to the subsequent changes in look of the surface itself. To their exposition, however, the polar phenomena become inseperable adjuncts since they are inevitable ancillaries to the result.

With the familiar melting of the polar snow-cap, therefore, this account properly begins, since with it begins the yearly round of the planet's life. With the melting of our own Arctic or Antarctic cap might, similarly, be said to begin the Earth's annual activity. But there appears to be one important difference here at the very outset between the two planets. In the case of the Earth the relation of the melting of its polar snows to the awakening of surface activity is chiefly one of *post hoc* simply; in the case of Mars it seems to be one of *propter hoc* as well. For unlike the Earth which has water to spare, Mars is apparently in straits for the article and has to draw on its polar reservoir for its annual supply. To the melting of its polar cap and to the transference of the water thus annually set free to go its rounds seems to depend all the phenomena upon the surface of the planet.

The observations upon which this deduction is based extend over a period of more than five months; from the last day of May of this year to the seventh of November. They cover the regions from the south pole to about latitude thirty north. That analogous changes to those recorded, differing, however, in certain marked particulars, occur six Martian months later in he planet's northern hemisphere, I hold to be probable. For though it is likely that the general system is one for the whole planet, it is also likely that the distribution of the planet's surface details alters the action to some extent.

To an appreciation of the meaning of the changes it is to be borne in mind throughout that the vernal equinox of Mars' southern hemisphere occurred on April 7th, 1894, the summer solstice of the same hemisphere on Aug. 31st, and that its autumnal equinox will take place on Feb. 7, 1895.

On the 31st day of last May, therefore, it was toward the end of April on Mars. The south polar cap was then very large, upwards of 45 degrees across, and already in active process of melting. The tilt of the planet's axis toward the Earth enabled it to be well seen, and disclosed the fact that it was bordered

persistently by a dark band, broader in some places than in others, but keeping pace with the snow's retreat. The average breadth of the dark band was, in June, 220 miles. It was the darkest marking on the disk and was blue.

As the season advanced and the snow cap diminished its dark girdle diminished in breadth, with fluctuations dependent doubtless on the draining capacity of the ground. In August it showed as a slender dark thread.

That it was water is practically beyond a doubt. That it was of the color of water; that it so faithfully followed the melting of the snow; and that it subsequently vanished are three independent facts mutually confirmatory to this conclusion.

That it was the darkest blue marking on the disk implies that it was the deepest body of water on the planet. That it subsequently entirely drained off implies that its depth could not have been very great Both facts together make a first presumption in favor of its being not only the chief body of water on the planet but the only one of any size.

This polar sea plays *deus ex machina* to all that follows.

So soon as the melting of the snow was well under way, long straits of deeper tint than their surroundings made their appearance in the midst of the dark areas. I did not see them come, but as I have since seen them go, it is evident that they must have come. They were already there on the last day of May. The most conspicuous of them lay between Noachis and Hellas in the Mare Australe and thence through the Mare Erythaeum to the Hour-glass Sea (Syrtis Major). The next most conspicuous one came down between Hellas and Ausonia. Although these straits were very distinguishably darker than the rest of the seas through which they ran, the seas themselves where then at their darkest The fact that these straits ran through the seas suffices to raise a second doubt whether the seas be seas. The subsequent behavior of the so-called seas renders their aquatic character still more doubtful.

The appearance of things at this initial stage of the Martian Nile-like inundation is shown in Fig. 1. That the seas were then at their darkest is probably due both to the fact that some water had already found its way down from the pole and also to the fact that moisture had been deposited there on the water's journey up and had quickened the vegetation of those relatively amphibious lands. The date which this drawing represents was June 1st, that is, about the Martian first of May.

For some time the dark areas continued largely unchanged in

appearance; during, that is, the earlier and most extensive
of the melting of the snow-cap. After this their history be
one long chronicle of drying up. Their lighter parts grew lig
and their darker ones less dark. For even to start with
were composed of every grade of tint. Indeed one of the m
significant features about them was that at this epoch it was
possible to fix any definite boundaries to the south tempe
chain of islands. The light areas and the dark ones merge
distinguishably into each other. Viewed from the standpoi
maps of Mars, the landmarks of this whole region lay oblite
by a deluge; not directly but indirectly. Probably the re
was in various stages of vegetal fertility in consequence
comparatively small body of water then inundating it The c
of the dark areas was then and is now, to my eye, a bluish-gr
quite unmistakably so. This tint gradually faded out to
place to orange-yellow.

The first marked sign of change was the reappearanc
Hesperia. This took place in July. In August, when I saw i
presence was marked. As yet nothing could be seen of Atla
It was not till the end of October, on the 30th, that I ca
sight of it. About the same time the straits between the isla
Zanthus, Scamander, Ascanias and Simois came out sali
dark, a darkness due to contrast.

Meanwhile the history of Hesperia continued to be ins
tive. From having been invisible in June and conspicuou
August, it returned in October to a mid-position between
two. Figs. 3, 4 and 5 show it in its three metamorph
Vacillating as these fluctuations may seem at first sight
will all be found to be due to one progressive change in the s
direction, a change that showed itself first in Hesperia itself
then in the regions round about it. From June to August
peria changed from a previous blue-green, indistinguish
from its surroundings, to yellow, the parts adjacent rema
much as before. In consequence the peninsula stood o
marked contrast to the still deep blue-green regions by its
Later the surroundings themselves faded and their change
the effect of once more partially obliterating Hesperia.

While Hesperia was thus causing itself to be noticed, all th
of the south temperate zone, as we may call it for identifica
sake, was unobtrusively pursuing the same course. Wh
in June all that part of the disk comprising the two T
Argyre II and like latitudes was chiefly blue-green, by Oct
it had become chiefly yellow. The separate identity of the

lands became then for the first time apparent. Still further south what had been first snow and then water turned to yellow land. This metamorphosis went on till on Oct. 13th the remains of the snow-cap entirely, or practically entirely, disappeared; the first complete disappearance of it on record. After this event the whole south polar region was one yellow stretch.

Toward the end of October a strange and, for observational purposes, distressing phenomenon took place. What remained of the more southern dark regions proceeded unexpectedly to fade in tint throughout. This was first noticeable in the Cimmerium Sea; then in the Sea of the Sirens and in November in the Mare Erythraeum about the Lake of the Sun. This fading steadily progressed until it got so far that in poor seeing the markings were almost imperceptible and the planet presented a nearly uniform yellow disk.

Now this fading out of the dark areas is a highly significant fact, with a direct bearing upon their constitution. For it is not simply that portions of the planet's surface have changed tint but that taking the disk in its entirety, the amount of the blue-green upon it has diminished and that of the orange-yellow proportionately increased. Mars appears more Martian than he did in June. Now if the blue-green areas represent water, where has this water gone? Nowhere on the visible disk. That is certain. For in that case the amount of the dark areas should not be perceptibly lessened; which it is. Nor can it all very well have gone to that part of the planet that is hidden from view. For Schiaparelli's observations in 1882 go to show that the northern snow-cap forms late; one month *after* the vernal equinox of the northern hemisphere on that year. Since therefore the water fails to prove an alibi, presumption is instantly raised in favor of the alternate hypothesis, that the blue-green areas represent vegetation, fertilized by a comparatively small amount of water whose direct presence or absence is not very perceptible to us, but whose indirect effects are. For vegetation might change from green to yellow without requiring any corresponding inverse change elsewhere.

Now though the passage of the water may not be traced by its amount; there is a further change which has lately appeared on the disk which hints at what has become of it. The canals have darkened. What is more their darkening has pursued a perfectly definite course, proceeding steadily from south to north.

The following observations of mine show first: that the canals are not equally visible at all times; and secondly: that their invisibility is a matter of the Martian seasons.

In June the canals were very faint markings indeed. The l faint were those in the Solis Lacus region As the planet proached us they all became naturally easier to make out until October no change apparently occurred in any of them cept those in the region about the Lake of the Sun. These September were already dark In October they began to sh symptoms of growing lighter again. At the next presentatio November they showed further signs of change though not di ing as yet very unmistakably in tint. Meanwhile when the S Titanum region came round in November I, found that its ca had begun likewise to darken. Figs. 6 and 7 show the cha that had taken place. The canals were not only darker relati to the Mare Cimmerium and the Mare Sirenum than they been, but actually darker themselves. In the next few nigh found the more northern canals about Ceraunius had follo suit. They had darkened relatively to the southern ones a the Lake of the Sun.

Now on looking at a map of Mars, it will be seen that the l Lacus region is that part of the continental areas which nearest the south pole Similarly that the region about S Titanum is the next farthest south. The matter of lati therefore affects the point

The same progressive change in the visibility of the cana shown in Figs. 1, 2 and 3 By October 30th the canals to northwest of Hesperia had become more pronounced

The canals and so-called lakes share therefore in the ar metamorphosis, with a season change dependent in a gen way upon their latitudes A wave of deepening tint passes cessively through the blue-green regions from south to no timed to the seasonal wave that travels from pole to pole F being pale in winter, their color comes with the spring, dee through the summer and dies out again in the autumn. In given locality the change comes early or late in proportion a place lies, other things equal, distant from the pole

That this change of tint is due indirectly to water and di ly to the vegetation that water induces seems probable For as there is great difficulty in disposing of the water on the supposition so the second would lead us to expect just the nomena observed We may therefore conclude that the sea Mars are probably midway in evolution between the sea Earth and the seas of the Moon No longer bodies of w they have not yet become barren ocean beds but are in that way stage of the process when their low level helps them c

PLATE XLII.

SOUTH.

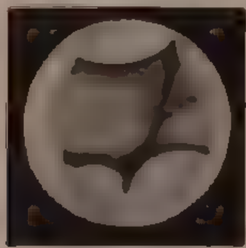


Fig 1.—SYRTIS MAJOR
At June Presentation.
Long. [illegible]
Lat. centre of Disk 24°
(Marth.)

Fig 2.—SYRTIS MAJOR
At October Presentation
Long. [illegible]
Lat. centre of Disk [illegible]
(Marth.)

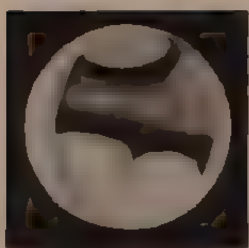


Fig 3.—HESPERIA
At June Presentation
Long. 260°
Lat. centre of Disk 20
(Marth)

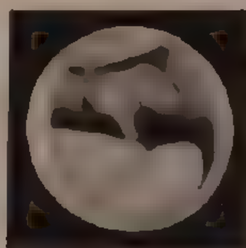
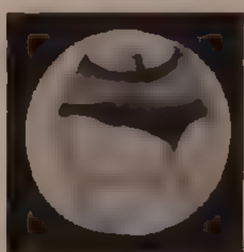
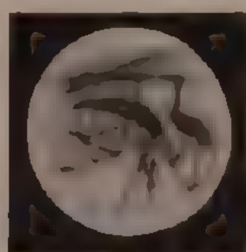


Fig 4.—HESPERIA
At August Presentation
Long. [illegible]
Lat. centre of Disk [illegible]
(Marth.)

Fig 5.—HESPERIA
At October Presentation
Long. [illegible]
Lat. centre of Disk [illegible]
(Marth)





NORTH

Fig 6.—SINUS TITANUM
At June Presentation
Long. [illegible]
Lat. centre of Disk 20
(Marth.)

Fig. 7.—SINUS TITANUM
At November Presentation
Long. [illegible]
Lat. centre of Disk [illegible]
(Marth)

what water still voyages upon the planet's surface though they have long since parted with their own.

As for the lakes and canals I defer what I have to say about them to a later paper. For the purposes of this one it is enough that they prove to be, like the seas, functions of the seasons.

Throughout all these interesting changes that follow the seasons across the face of Mars there is but one feature approaching permanence: the great continental areas. Except for a possible variation in brightness here and there this great area has remained unchanged. Like the reddish desert regions of our Earth its color and immutability point to like character for cause. It does not change because it is already past such possibility. It is one vast desert waste.

LOWELL OBSERVATORY,
Nov. 11, 1894.

THE FORM OF THE DISC OF THE III SATELLITE AND PHENOMENA OF THE OCCULTATION OF A SATELLITE OF JUPITER.

E. E. BARNARD.

In ASTRONOMY AND ASTRO-PHYSICS for 1894, November, page 728, Professor William H. Pickering predicts the probable elongation of the disc of the 3rd satellite of Jupiter.

On that date (Nov. 5) I examined the satellite closely with all powers up to 1000 on the 36-inch with good seeing. From 14^h 0^m to 15^h 10^m standard Pacific time frequent examinations of the satellite showed it to be round, with the eyes held in different position angles.

The usual markings were seen on it, but no distortion of the disc was apparent.

Professor W. H. Pickering has also called attention to the appearance of a twilight or glow of light at the limb of Jupiter just preceding the first appearance of a satellite, when coming out from occultation, which would prove a dense atmosphere to Jupiter, extending above the visible surface. When the satellite has been cut by the limb of Jupiter he has also observed a distortion of the small disc as if seen through a refracting medium.

I had never witnessed these phenomena, and on two occasions lately (when the opportunity offered) have watched specially and carefully for them with the 36-inch. Both these observations were of satellite I at emergence from occultation and the seeing in each case was all but perfect.

EMERGENCE OF I FROM OCCULTATION.

1894, Oct 8, $14^h\ 37^m\ 31^s$ first glimpse of I.
1894, Oct 8, $14^h\ 40^m\ 08^s$ just free from Jupiter.

The limb of Jupiter was sharp and clear. Watched carefully fo any appearance of "down" but no trace of it. The satellite w first seen as the tiniest white speck and gradually grew to ro tundity at the last contact. There was no distortion of the dis of I. There was no transparency of the limb of Jupiter Th satellite was sharply and clearly cut, the limb of the planet be ing strongly and sharply contrasted against the face of I. Th reappearance was at a point on the limb where the north reddis belt would be.

EMERGENCE OF I FROM OCCULTATION, STANDARD PACIFIC TIM

1894 Oct. 15, $16^h\ 29^m\ 48^s$ one-half out
$16^h\ 31^m\ 38^s$ last contact

Watched carefully in this case also for any appearance "down," for any distortion of the satellite and for any tran parency of the limb of Jupiter. None of these phenomena w present, the seeing was fine and the limb of Jupiter clear an sharp on the face of the satellite, which was first caught as th tiniest speck of light—the time of the first glimpse was not re corded as I wished to devote my whole attention to the progre of the reappearance.

These observations show that on these two occasions, at leas there was no perceptible illumination, a Jovian atmosphere pr ceding the rising of the satellite. There was no distortion of th disc of the satellite at any time during the reappearance Th limb of Jupiter was perfectly and absolutely opaque,—as it ha always been at every observation that I have made of the occu tation of a satellite with instruments varying all the way fro 5 inches to 36 inches

On this latter subject, *i e.*, the transparency of the limb of Jup ter—I have had something to say in ASTRONOMY AND ASTRO PHYSICS for April, 1894, page 272, and June, 1894, page 438.

I would simply quote here from the first of these papers an when I quote it I unhesitatingly endorse and emphasize the stat ments

"These observations have been made with care, with the mo powerful telescope that to-day can be applied to such observ tions, and the limb of Jupiter has appeared perfectly opaque a at all previous observations with smaller telescopes

"I think it is high time that astronomers reject the idea that the satellites of Jupiter can be seen through his limb at occultation. When the seeing is bad there is a spurious limb to Jupiter that well might give the appearance of transparency at the occultation of a satellite. But under first class conditions the limb of Jupiter is perfectly opaque.

"It is quibbling and begging the question altogether to say that the phenomenon of transparency may be a rare one and so have escaped my observation."

There are too many things of this kind (transparency of the limb of Jupiter) that have gained currency in astronomical literature. It is the duty of every conscientious observer to see that they are weeded out, and astronomy will be all the better and purer for the purging.

LICK OBSERVATORY, Nov. 9, 1894.

THE DISTRIBUTION OF HEAT ON THE SUN.*

W. H. S. MONCK.

That the present temperature of the Sun is partly at least due to shrinkage and the development of heat in the process must, I think, be admitted, but it seems to me that it leads to some consequences not generally recognized which I propose to indicate briefly.

Starting from a position of equilibrium, *i. e.*, which would continue without alteration if we could prevent heat from entering or leaving the Sun, any further shrinking can only be due to cooling, or rather, loss of heat. The Sun probably reached this position of equilibrium ages ago. The assumption that it is losing more heat than it receives from without seems to be very generally made, and is so far correct, that it certainly parts with more heat than it is known to receive from without. The shrinkage thus caused is, of course, in reality a single steady process, but for convenience we may suppose it to occur in three stages: 1. A contraction or falling in of the solar matter towards the centre of gravity due to cooling or loss of heat. 2. Heat developed by this contraction or falling in. 3. Expansion caused by the heat thus developed. This expansion can never, I apprehend, be equal to the original contraction and the effect of (2) is therefore

* Communicated by the author.

merely to render the contraction slower than it would be if no heat were developed by the contraction.

So far as heat received from without is concerned, the temperature of the Sun cannot be much affected thereby. No star is likely to have remained near enough to the Sun for a sufficient time to render the part of the Sun on which its light fell sensibly hotter than the part on which it did not fall, nor does there appear to be any arrangement of stars in the aggregate that would render one portion of the Sun hotter than another. As to the heat derived from meteors falling into the Sun we can only speculate, but I doubt if it would produce any sensible difference in the temperature of different parts of the solar surface.

But the Sun is a rotating body, and the centrifugal force of rotation is different at different parts of its surface. It lessens the force of gravity everywhere but more so at the solar equator than in the neighborhood of the solar poles. Now the heat developed by contraction depends on the force of gravity at the place where the contraction takes place. Shrinkage develops less heat at the solar equator than near the solar poles, and a state of things must arise which completely inverts what we find on the Earth. *The Sun is hotter at the poles than at the equator.*

There are two ways in which this superior heat might be ascertained. One is by direct measurement. Test the heat, light or chemical action of a part of the Sun near the equator and another part near the pole, both being at equal distances from the solar limb. But the detection of no measureable difference would not decide the question. The photosphere is probably to be assimilated not to the solid body of the Earth but to our terrestrial clouds. The effect of the greater heat at the poles may not be to make these clouds hotter but to make them float at greater elevations in the solar atmosphere. This, if it occurred, could be detected in another way. The Sun would not exhibit that amount of flattening at the poles which we might expect from the velocity of its rotation. No flattening has been hitherto observed. But the centrifugal force at the Sun's equator is very small compared with gravity. It is only by its continuous action for perhaps millions of years that a considerable difference of temperature between the poles and the equator could be established.

A difference in temperature between the poles and the equator would necessarily set up air-currents in a body possessing an atmosphere as the Sun does. But in these currents our trade winds and anti-trades would of course be reversed. The cooler air (

use this term *air* for the gases which form the solar atmosphere) would flow in below from the equator towards the poles, and the hotter air from the poles to the equator would flow over in upper currents. If the sunspots or any other visible peculiarities were connected with these currents, they would appear to be moving from the pole to the equator if they belonged to the upper-current, and from the equator to the pole if they belonged to the under-current. Those in the upper-current would take longer to affect a revolution than the Sun's true period of rotation, while those in the under-current would effect a revolution in less time. From this point of view sunspots present some indications of being phenomena of the upper-currents. No such spots are found in the vicinity of the poles. This would be a necessary consequence of the current-theory. No system of trades and anti-trades could extend to the vicinity of the poles. But I can offer no suggestion with regard to the periodicity of the sunspot periods.

The principles here laid down would apply to the cooling of any rotating body; but the great supply of heat in the planets is derived by radiation from the Sun while shrinkage has but a slight effect on the phenomena. In the case of the Earth it probably somewhat lessens the difference in temperature between the equator and the poles which is caused by the Sun's radiation. With Jupiter perhaps the effect may be different. The planet is more distant from the Sun, composed of lighter materials, and rotates more rapidly on its axis. Possibly, too, the condition of equilibrium with which I started has not yet been attained in the case of Jupiter. The planet undoubtedly sometimes presents phenomena analogous to the sunspots, and the rotation-period of the planet derived from one of these spots differs from that derived from another. But neither these Jovian spots nor the sunspots exhibit the unmistakable changes of latitude which might be expected if they were connected with air-currents. Perhaps they are only formed near the limits of a current or the points where conflicting currents meet.

Comparing the Sun with fixed stars, more rapid rotation would exaggerate the difference between the polar and equatorial temperatures and the force of the two systems of currents; and of course if the phenomena of sunspots are connected with these currents, it would exaggerate them also. A spectrum resembling that of a sunspot might, therefore, be expected in the case of stars which rotate with great velocity: and if there is also a periodicity in the appearance of the spots, the star would probably be periodically variable in its light. Variation might thus arise

not from any peculiarity either in the chemical composition or the temperature of the star, but from the rapidity of its rotati and consequent difference of temperature between its poles and equator. Some of our terrestrial geysers afford examples of pe odical eruptions arising from continuous causes. They are n indeed closely analogous to the phenomena which I have be considering, but may nevertheless suggest that periodicity in t effect does not necessarily suppose periodicity in the cause, an moreover the sunspot maxima do not appear to be strict periodic.

THE TWENTY-INCH EQUATORIAL OF THE CHAMBERLIN OBSER VATORY.

H. A. HOWE

Having been requested by the editor of *Astronomy and Ast Physics* to give further details about the Chamberlin telesco the writer begs to call attention to the following:

The Tripod-base. In the article of last month mention w made of the tripod on which the instrument stands The wo tripod conveys the idea of instability, when used in connecti with so large a mounting. Though Mr. Saegmuller has used t same form of support for the 12-inch telescopes at Providence an Georgetown, where it has given entire satisfaction, no descripti of it has yet appeared. Around the periphery of a hollow cyl der, which is 5 ft. in diameter and 2ft. high, are set at equidista intervals 3 pairs of isosceles right triangles. The plane of ea triangle is vertical, and one of its perpendicular sides coinci with an element of the cylinder. The triangles of each pair a 6 inches apart. Between the outermost vertices of each pair triangles runs one of the large vertical supporting bolts whi were described last month. These stout triangles may be co sidered as the legs of the tripod. When the weight of the instr ment is on the tripod there is a tendency to tear the cylind asunder at its lower edge. This is resisted by a horizontal flan about 9 inches wide, running entirely around the base of t cylinder and underneath the triangles. The entire tripod, bei a single heavy casting weighing 3 tons, is extremely rigid.

The Pillar. The form of the pillar deserves commendation I circular horizontal cross-section is a pledge of rigidity in azimu Its sudden flare at the base, where the iron is over 6 inches thi

allows its upper portion to be small, so that observations can be made in or near the zenith, without having the eye-end strike it. For observations near the pole it would be difficult to get the observing chair far enough north, in case the pillar were long north and south, as are most of the large pillars in this country.

The Clamps and Slow Motions. The declination clamp and slow motion is of the customary toothed sector form. The same principle is applied to the construction of the right ascension clamp and slow motion. The large worm wheel at the upper end of the polar axis engages, as usual, with a worm driven by the clock, and cannot be clamped directly to the polar axis; it always rides loosely upon that axis. Above the wormwheel, that is, between it and the declination sleeve lies a toothed sector, similar to the one for declination. The toothed end of the sector engages with a worm mounted on the declination sleeve, close by the small end of the declination axis. The other end of the sector embraces a hub on the upper side of the big wormwheel, and encircles the polar axis, though it does not touch it. When the sector is clamped to the hub of the wormwheel the polar axis cannot revolve, unless the clock drives it, or the slow motion worm which engages with the sector is turned. To render it possible to clamp the telescope and give it a slow motion, in right ascension, when the observer is at the eye-end, the declination axis is made hollow. A stout steel tube, inside of which lies a cylindrical rod, fills this hollow. The rod has a small bevel gear at each extremity. The tube carries a bevel gear at the end inside of the telescope tube, and a sprocket wheel at the other end. The sprocket is encircled by a small chain by means of which the right ascension clamp is actuated. The clamp and slow motion rods start at the eye-end, enter the telescope and engage with the small bevel gears just mentioned. The motions are then carried through the declination axis by the internal tube and rod, and thus communicated to the sector-worm and clamp.

The advantage of this mechanism is that the instrument is held strongly, and that the slow motion is smooth, delicate, and almost free from back-lash. The disadvantage is that there is considerable friction when the instrument is moved by hand in right ascension. For one has then to overcome not only the friction of the wormwheel rubbing on the polar axis, but the friction of the sector on the hub of the wormwheel. If a copious supply of oil be used to overcome this sector-friction, the clamp will not grip well. For small equatorials no appreciable disadvantage arises from this source, but in a large one every extra drain upon the

observer's strength is to be avoided. So admirable are the friction wheels and balls with which Mr. Saegmuller has equ the polar axis, that most of the friction encountered in givin instrument rapid motion in hour-angle is due to the rubbi the worm-wheel and of the sector. It is evident that this fr does not come into play when the clock drives the instrumen

It would not be difficult to mount the wormwheel on bearings, but if the sector were mounted in like manner, th ciency of the clamp would be diminished, though by no m destroyed. However, it would not be difficult to devise a po ful and simple clamp, even if the sector were equipped with bearings.

Finding Circles. The mechanism of the finding circles nea handwheels on the south side of the pillar is unique and i ious. It is driven by the two vertical rods at the lower en which are the handwheels for quick motion in right asce and declination. The large cylindrical box containing mechanism hangs between these rods, and is supported by on ball-bearing collars. The rods themselves are hung from top by means of ball-bearing collars, and are held truly ve by grinding collars just above the handwheels. The eastern engages, by the help of subsidiary gearing, with a large bevel on the polar axis, just below the hour circle. The western similarly turns a stout tube of Mannesman steel which ru through the polar axis, and carries at its upper end a bevel which meshes with another bevel gear encircling the declina axis. A little consideration will show that if the instrume revolved on the polar axis both rods must turn, while a rota on the declination axis causes the western rod only to turn.

The conditions of the mechanical problem so beautifully s by Mr. Saegmuller, in the construction of this system of fin circles, may be stated as follows:

(*a*) When the instrument rotates on the declination axis the western rod alone is turning, the declination pointer move along its dial, and the right ascension pointer must be tionary.

(*b*) When the instrument is rotating on the polar axis both rods are turning, the declination pointer must be at and the right ascension pointer must move.

(*c*) When the driving clock is moving the instrument, and rods are turning, the reading of neither dial must change.

(*d*) When the instrument is at rest the reading of the righ cension dial must change continually.

(*e*) When the instrument is moving on both axes, each pointer must move with double the angular velocity of the corresponding axis, and in all possible positions of the telescope the dials must give the right ascension and declination of the object toward which the telescope is directed. The diagram shows the details of the mechanism, with the exception of the clock which drives the right ascension dial.

The Micrometer. In the former article there was one criticism of this attachment, on the ground that the position circle was hard to read, when the box was directly over the verniers.

Having now used the micrometer a little, the writer finds that this difficulty is hardly worth mentioning, in case only one vernier is to be read, which is generally sufficient. For that end of the box on which the illuminating lamp is fastened is so high up that one can look under it and read the circle easily. The writer has seen no other micrometer as fine as this one, except the one built by the same maker for the Lick telescope.

Electric Illumination. Since the previous description of this was written, a number of experiments have been made, the results of which may be of interest. In illuminating the verniers of the main circles, which are read, from quite a distance, by telescopes, small electric lamps having clear bulbs are not satisfactory, because they illuminate strongly only a small portion of the vernier. After experimenting vainly for some hours with 2 c. p. lamps assisted by reflectors and lenses, 6 c. p. lamps with frosted bulbs were fixed upon for the declination verniers, and ordinary 16 c. p. house lamps, set with the long portion of the filament parallel to the edge of the vernier, were used for the hour circle. The 6 c. p lamps were set close to the verniers, and the 16 c p. about a foot away. The axis of symmetry of each 6 c. p. lamp bulb is parallel to the edge of the vernier, and the lamp is backed by a semi-cylindrical reflector of bright tin. The large rough setting circles on the declination axis and on the north face of the clock-box are illuminated by 2 c. p. lamps with clear bulbs.

The general principle is that a bright reflecting surface, like a silver circle, is to be lit up by a frosted lamp, while a dull surface fares better if the lamp-bulb be clear.

For the micrometer wires a 2 c. p. clear bulb lamp is used, and its intensity is diminished by a small coil of german silver wire, giving a resistance variable at will. This is preferable to using a 1 c p. lamp, which gets very hot. Instead of employing a red shade-glass, one may purchase of the Edison Miniature and Decorative Lamp Co. (Harrison, N. J.) lamps with ruby bulbs.

Astro-Physics.

THE ASTROPHYSICAL JOURNAL.

GEORGE E. HALE

In a paper bearing the above title, published in the first number of ASTRONOMY AND ASTRO-PHYSICS (January, 1892), the reasons which had prompted the publication of a journal of astronomical physics were enumerated, and evidence was adduced to show that considerable support might be expected for such a venture. It had been my intention to establish a separate astrophysical journal, but the uncertainty of such an undertaking led to an acceptance of Professor Payne's proposal of a union with the *Sidereal Messenger*, and ASTRONOMY AND ASTRO-PHYSICS was the result. The contents of the thirty numbers published during the three years which have elapsed since that time offer sufficient testimony to the usefulness of the composite journal. From the outset the editorial supervision of the departments of *General Astronomy* and *Astro-Physics* has been kept entirely distinct. The policy of the latter department has been determined by myself and my associates, Professors Keeler, Crew and Ames, while the selection of all other matter published in the journal has been made by Professor Payne and those who were associated with him. No attempt has been made to draw a hard and fast line between the two departments. Had this been done, and a strict definition of "astrophysics" adhered to, a large part of the matter published under *General Astronomy* would have appeared in the other department of the journal. It was thought best, however, to confine the scope of *Astro-Physics* to the more technical subjects connected for the most part with spectroscopic work.

In returning to the original idea of a purely astrophysical journal we are simply following out a long-cherished plan. Few who appreciate the true scope of astrophysics, and have its best interests at heart, will deny the advisability of devoting an entire journal to this, the most fascinating and at the same time the most rapidly advancing department of astronomical research. In spite of the existence of physical and astronomical journals of the highest class, the astrophysicist or spectroscopist is at a loss to know where to publish in order to reach the audience he desires. Should he choose an astronomical journal, he will find that his paper will remain unread and unknown by a very large majority of physicists—the very men who are perhaps best com-

Professor James E. Keeler of Allegheny Observatory, whose association in the editorial management of ASTRONOMY AND ASTRO-PHYSICS has done so much for this journal, has agreed to join the writer in editing THE ASTROPHYSICAL JOURNAL. Professor Henry Crew of Northwestern University and Professor Joseph S. Ames of Johns Hopkins University will continue the valuable work they have hitherto carried on in connection with ASTRONOMY AND ASTRO-PHYSICS as Assistant Editors of the new journal, and Professor F. L. O. Wadsworth of the University of Chicago, Professor Edwin B. Frost of Dartmouth College, and Professor W. W. Campbell of the Lick Observatory have promised to assist in the same capacity. In addition to this exceptional editorial coöperation—in itself quite sufficient to make THE ASTROPHYSICAL JOURNAL truly international in character—we are fortunate in having promises of assistance from many astronomers and physicists in Europe and America.

It must not be supposed that THE ASTROPHYSICAL JOURNAL will deal only with the astronomical applications of the spectroscope. On the contrary, the scope of the JOURNAL will be quite as broad as that of ASTRONOMY AND ASTRO-PHYSICS has been, for while papers dealing only with questions of celestial mechanics and measures of the positions of the heavenly bodies will not fall within it, they will be replaced by articles treating of laboratory researches closely allied to the investigations of astronomical physics. Drawings, photographs, descriptions and theories of the Sun, Moon, planets, satellites, comets, shooting stars, star clusters, nebulæ and the Milky Way will all be considered as coming within the scope of the new journal. So too will observations of variable stars, photometric determinations of stellar magnitudes and planetary albedo, measurements of solar radiation and atmospheric absorption, observations of the phenomena of lunar and solar eclipses, and the numerous applications of the spectroscope in astronomy. The importance of supplying a common place of publication for papers on both the observatory and laboratory applications of physical methods of research has already been pointed out. For this purpose much space will be devoted to articles on wave-length determinations of the lines in solar, metallic and gaseous spectra, bolometric and bolographic work, spectral photometry, experiments on radiation and absorption, photographic researches in the ultra-red and ultra-violet, studies of the relations of the lines in different spectra, interference and diffraction phenomena, and theoretical work in certain branches of optics, heat, electricity and other departments

States, Canada and Mexico. In other countries of the Postal Union the price is 18 shillings. Subscriptions should be sent to *The University of Chicago, University Press Division, Chicago, Illinois.* All Eutopean subscriptions should be sent to the sole foreign agents, Messrs. Wm. Wesley & Son, 28 Essex St. Strand, London.

All papers for publication and correspondence relating to contributions should be addressed to *George E. Hale, Kenwood Observatory, Chicago, Illinois.*

THE MODERN SPECTROSCOPE.

IX.

FIXED-ARM SPECTROSCOPES

FRANK L. O. WADSWORTH

In prismatic spectroscopes and spectrometers of the usual construction it is necessary, in order to observe different portions of the spectrum under the same conditions, to vary the angle between the axes of the observing and collimating telescopes by the rotation of the arm which carries one of these telescopes about an axis parallel to the refracting edge of the prism. Usually the arm which carries the slit and the collimating-lens is fixed, and that carrying the observing telescope is movable; but sometimes the apparatus which it is necessary to carry on the observing arm is so massive, or else requires such a degree of stability, that it becomes necessary to fix it in position and make the slit-arm the movable one. Then difficulties are at once encountered in the illumination of the slit, if a fixed source of light, such as the Sun or a star, is under examination. Even if terrestrial sources be employed, it is ofttimes extremely undesirable to have a complicated system of collimator, slit, condenser, and source of light, swinging about on a long arm, and in particular cases such an arrangement is absolutely inadmissable. In such cases where both arms of the spectroscope are necessarily fixed, we may easily bring the different parts of the spectral field to the cross wires of the observing telescope by a rotation of the prism alone; but in none of the usual forms without violating the usual condition imposed in spectroscopic work, viz., that the prism shall always be in minimum deviation for the central ray in the field.

Heretofore the only forms of "fixed-arm" spectroscopes which

cated. It hardly seems possible that the use of a reflecting surface in this way can be new, as it is something which would naturally suggest itself to anyone who might consider the problem; but no reference has been found to its employment, perhaps because the necessity for a "fixed-arm" spectroscope does not often seemingly arise. I hope to show, however, that at least some of the forms which have been developed during the last three years are simpler, more convenient, and less expensive than the ordinary form, and for spectrometric work quite as accurate. I will briefly describe these forms in the order of design and use, for this, perhaps, naturally is also the order of increasing simplicity.

Case I.—The case which first led me to a consideration of the problem was one in which it was desired to examine the radiations from a Geissler tube by means of the wave-comparer.* Here the instrument which took the place of the usual observing eyepiece was a massive apparatus weighing about 500 pounds, which required great steadiness of mounting, while the Geissler tube, which itself served as the slit, was attached to a mercury-pump on one side and to a sodium-amalgam generating apparatus, with its attached drying-tubes, mercury-valves, etc., on the other, in such a way as to make its movement impossible. At first, indeed, the tube was mounted on a slit-arm of the spectroscope, sufficient flexibility being secured to allow of different lines being brought on the slit of the wave-comparer by the use of long lengths of glass tubing, but experiments soon showed that it was necessary to keep the passage from this Geissler tube to the pump as short and large as possible. If any considerable length of tubing intervened, unusual precautions had to be taken to keep all parts of it *perfectly* dry and clean, for only under these conditions, which, as the tubes were frequently changed, were almost impossible to maintain, could the McLeod gauge attached to the pump be relied upon to give, even approximately, the true pressure in the tubes, particularly when the electric discharge was passing.

The arrangement adopted in this case was that shown in plan in fig. 1, Pl. XLIII; where t is the Geissler tube which serves as the source; m a mirror, mounted on a vertical axis on the movable spectroscopic arm R; and S the slit of the wave-comparer, which separates out the radiation which is to be analysed by the latter. To the lower end of the shaft which carries the mirror m is fixed a drum connected with a second drum a, fixed on the axis of the spectroscope by two steel cords. The drum a is just one half the

* See a paper by Professor A. A. Michelson, "Application of Interference Methods to Spectroscopic Measurements," *Phil. Mag.*, Sept. 1892.

the angular error in the reading will evidently be

$$\delta = \frac{\Delta y}{R} = -\frac{2r}{R}\sin^2\frac{\epsilon}{2}, \qquad (1)$$

where R is the focal length of the collimator. For most prisms ϵ will not exceed 6° from end to end of the spectrum, and hence, if we suppose $\alpha = 45°$, for the position of mean deviation we have for the maximum error $\delta = .00013 r/R$, or if $r/R = \frac{1}{10}$, $\delta = 25''$.

It is evident that there will be a change in the focal distance S, *b*, C as the arm revolves, which under the conditions just assumed, would amount to about [illegible] R. This was not altogether a disadvantage, for when the lenses O and C are either or both of them simple lenses, we can, by choosing a value of r/R suitable to the particular prism used, very nearly compensate for the optical change in focus in passing from the violet to the red, or *vice versa*, by the mechanical lengthening or shortening of the path S*b*C. From an inspection of (1) it will be seen that the correction will diminish as the ratio r/R diminishes, and will become 0 for $r = 0$, or for $R = \infty$. This indicated at once two methods of eliminating the error. The first and simplest is to place the mirror between the prism and collimator instead of between tube and collimator as indicated in dotted lines in Fig. 1 (Pl. XLIII). The second is to make $r = 0$ by placing the mirror-face in the axis of rotation of the spectroscope. To do this we must either place the prism considerably out of center, Fig. 2 (Pl. XLIII), or else we must place the mirror above or below the plane of refraction (Fig. 3, Pl. XLIII.) In either case a second reflector must be used to receive the beam from the first reflector *m* and return it to the prism. This second reflector may conveniently be a concave mirror of a focal length *sm* + *mc*, which will at the same time serve as collimator, or as an objective of the observing-telescope. As the motion of the mirror is in this case the same in direction as that which must be imparted to the prism in order to keep it in minimum deviation, the mirror may be attached directly to the prism-table or to the prism itself.* Which of these two latter forms is to be used will be determined by the conditions of use. The second form was the one employed when the wave-comparer was used in connection with a spectrometer in experiments on wave-length measurements in the infra-red solar spectrum. In this form the ray is reflected out of the plane of incidence and refraction, and the spectrum is consequently inclined at a small

* The back of the prism may be polished and silvered, and arranged to act as the first reflector, as in Fig. 2*b*, Pl. XLIII.

In Fig. 2,

let d = distance oa from axis of rotation to the face of the concave mirror;

r = perpendicular distance ob from the axis to the plane of the reflecting mirror;

ω = angle oac;

θ and α have the same meanings as before

When in adjustment the line of collimation passes through the axis of the rotation, and the deflected ray oa will fall on the center of the concave mirror. Then we have from analytical geometry the following equations:—

For the line ac,

$$y = \tan(\theta + \omega)\,[x - d\cos\theta] + d\sin\theta$$
$$= -\tan 2\alpha\,[x - d\cos\theta] + d\sin\theta, \quad . \quad . \quad (2)$$

since $\theta + \omega = 180^\circ - 2\alpha$;

and for the line dc,

$$y = -\tan\alpha\,(x) + \frac{r}{\cos\alpha}. \quad . \quad . \quad . \quad (3)$$

The lateral displacement (Δy) of the ray will be the ordinate of the point of intersection of these two lines. Solving (2) and (3) for y we have

$$\Delta y = d\sin(2\alpha + \theta) - 2r\cos\alpha.$$

If the system be turned through a small angle ϵ, θ becomes $\theta + \epsilon$, and α becomes $\alpha - \epsilon/2$. Hence $\theta + 2\alpha$ = const., and

$$\Delta y' = d\sin(\theta + 2\alpha) - 2r\cos(\alpha - \epsilon/2),$$
$$\Delta y - \Delta y' = 2r\,[\cos\alpha - \cos(\alpha - \epsilon/2)] = r\cos(\theta + \omega)\sin\epsilon;$$

or the lateral displacement is directly proportional to r, to the cosine of the angle of deviation, and to the angle of displacement. Since ϵ is fixed by the prism used, and ω must be small in order to secure good definition, the only way in which the error of displacement may be reduced is by reducing r; hence the object in placing the mirror just as close to the prism as possible. In the actual case, the values of r, θ, and ω were

$$r = 10 \text{ centim.}, \ \theta = 50^\circ \text{ (for D lines)}, \text{ and } \omega = 3^\circ.$$

Hence for $\epsilon = 3^\circ$ we have $\Delta y' = 3$ millim., and the angular error is, as before, $3/R = \frac{3}{3560} = 3'$ nearly.

It will readily be seen that if we do not impose the condition that the reflected ray shall be parallel to the line of collimation,

of accuracy could be secured if necessary by the use of a minimum deviation attachment of pure linkwork, like that shown in Fig. 4 (Pl. XLIV), which is far superior in every respect to the ordinary sliding link-form usually placed on a spectroscope.* It may also be secured by the system first described, because in it any required degree of accuracy may be reached by an increase in size of the drums, *a*, *b*.

It was at first thought that a return to this system would be necessary; but while considering how this could best be done, the idea of another simple system, which did away with all mechanical arrangements, presented itself. It is derived from the first modification of the earlier system (Fig. 1, Pl. XLIII) very easily by suppressing entirely the movable spectroscope-arm and securing the reflecting mirror rigidly to the prism. It will evidently have the same angular motion as before, and will therefore reflect a ray which passes through the prism at minimum deviation in the same direction for all positions of the latter, or *vice versa*. With this arrangement it is evident that there will be no error due to mechanical inaccuracies of the moving parts because there is no longer any relative motion between them, both prism and mirror being fixed relatively to each other. But it would appear at first sight that for large angles of deviation the light would fall upon a very different part of the reflecting mirror to that which it would for small angles, and that a large lateral displacement of the reflected ray might be expected in consequence. The lateral shifting is no great detriment if a collimator is used, and the beam which passes through the prism is a strictly parallel one, for then its effect is simply to necessitate the loss of some light, or else the use of larger lenses to provide for this shifting. But where only one objective is used, as in the simpler forms of instruments, or for some special reason in the better ones† (see Fig. 7, Pl. XLIV), and the beam through the prism is in consequence conical, the lateral displacement gives rise, as already explained, to an angular error depending on the radius of curvature of the wave-front of the incident beam. This error, or, more properly, correction, was especially objectionable in the particular work then in hand, in which it was very desirable to maintain an exact relationship between the angular reading of the spectrometer-circle (in which prism and mirror were mounted), and the wave-length of the radiation falling on what corresponded to the

* Kempe, 'Lecture on Linkages,' p. 40.

† In this case it was desirable in order that there might be as little loss by reflection and absorption as possible.

$$x' = \frac{\dfrac{2 \sin \theta/2}{\cos \theta} [a \sin \theta/2 + b] - \dfrac{d + a \cos \alpha}{\cos \alpha}}{\tan \alpha + \tan \theta},$$

$$= \frac{2 \sin \theta/2 \cos \alpha [a \sin \theta/2 + b] - (d + a \cos \alpha) \cos \theta}{\sin (\alpha + \theta)},$$

$$y' = \frac{2 \sin \theta/2 [a \sin \theta/2 + b] \tan \alpha - \dfrac{d + a \cos \alpha}{\cos \alpha} \tan \theta}{\tan \alpha + \tan \theta}$$

$$= \frac{2 \sin \theta/2 \sin \alpha [a \sin \theta/2 + b] - (d + a \cos \alpha) \sin \theta}{\sin (\alpha + \theta)}.$$

The equation of the reflected ray CD will be

$$y - y' = \tan (\theta + 2\alpha)(x - x'),$$

and the length of the perpendicular let fall from the origin will be

$$p = \frac{\tan (\theta + 2\alpha)\, x' - y'}{\sqrt{1 + \tan^2 (\theta + 2\alpha)}} = x' \sin (\theta + 2\alpha) - y' \cos (\theta + 2\alpha).$$

Substituting the values of x' and y', reducing, and introducing the relations

$$\begin{aligned} \theta + 2\alpha &= \beta = \text{constant}, \\ \theta + \alpha &= (\beta - \alpha). \end{aligned}$$

we finally obtain

$$p = 2a \cos^2 \beta/2 + 2b \sin \theta/2 + 2d \cos (\alpha + \theta).$$

The term $2a \cos^2 \beta/2$ is a constant, but the last two terms are variable. It will be seen at once that we may make p a constant, viz., prevent any lateral shifting of the ray, by simply making both b and d equal to zero. In other words, if we simply fulfil the condition that the axis of rotation of the system shall be at the intersection of the plane bisecting the refracting angle of the prism with the plane of the reflecting mirror, there will be no lateral displacement of the ray which passes through the system at minimum deviation, for different values of θ, viz., for different wave-lengths. There is one case in which this does not hold, viz. when these two planes are parallel. Then we have

$$\alpha = 90^\circ - \theta/2, \text{ or } \alpha + \theta = 180^\circ - \alpha.$$

Hence

$$b \sin \theta/2 + d \cos (\alpha + \theta) = (b - d) \cos \alpha;$$

and therefore we may in this case make p constant by making $b = d$: in other words, by making the two planes coincident. Then the second half of the prism becomes useless, and we have

arrangement, which is in one sense a direct-vision spectroscope with but a single prism, fulfils the condition of parallelism between the refracted reflected ray and the incident ray, and it is therefore the one which has been finally adopted for the spectro-bolometer of the Observatory in place of the form first used on that instrument.*

A plan view of the mounting in this particular form of instrument is shown in Fig. 5, Pl. XLIV, and a side view (photograph) in Pl. XLV. It will be seen that the prism and mirror are mounted together in a single frame AA, provided with three leveling-screws which rest, one in a conical hole, the second in a slot, and the third on a plane; so that the whole frame may readily be removed from the spectrometer table and then replaced in exactly the same position. The triangular prism-table B is arranged to slide vertically in the guides *a b*, and thus provide for prisms of different heights. The table has a motion of adjustment regulated by the screw *c*, about a line parallel to the mirror-face, for the purpose of bringing the refracting edge of the prism parallel to that face, and is provided with screws *e, f, g, h*, which serve to adjust the prism laterally and in angle, so that in the first place the plane bisecting the refracting angle may pass through the axis of rotation, and secondly, so that the prism-faces may make equal angles with the faces of the mirror (the minimum-deviation condition.) It will be observed that when these screws are once adjusted they serve as stops which will bring any prism (of standard 60° angle) that may be used into the correct position, for, if smaller than the prism for which they were adjusted, it is only necessary either to turn each one in a measured amount, or, more simply, insert a thin piece of glass or metal between the screws on each side and the prism. A still better plan, if many prisms are to be used, is to have each prism mounted on a base of the required size on which it has been adjusted once for all and

* Since writing the above my attention has been called to an article in *Zeit. für Instrumentenkunde* for November, 1881, describing this particular "direct-vision" arrangement of the prism and mirror. There was, however, no indication of the general class of which this is but a particular type, and hence of course, no indication of the conditions which it was necessary to fulfil to prevent a lateral shifting of the beam; indeed, the author seems to accept this lateral shifting as a necessary condition, for he says "Der im Minimum der Ablenkung durchgehende Strahl wird also bei dieser Anordnung nur seitlich etwas verschoben."

Professor Langley had also quite independently used this particular arrangement of prism and reflecting mirror in a modification of Foucault's "Lifting Prism" for separating different orders of superposed grating spectra. In his use of it the lateral displacement was recognized as objectionable, and was mechanically corrected for by an ingenious arrangement, designed by Mr. C. T. Child, then assistant in the Observatory, which imparted to the mirror a small angular motion, just sufficient to correct for the angular displacement of the spectral image, as the whole system travelled down through the spectrum.

PLATE XLV.

$$\theta = 2\,(\beta - (90^\circ + \alpha)),$$

where β is the circle-reading for a deviation θ, and α is the zero-reading determined as already described.

In very accurate spectrometric work it is important to determine just what degree of accuracy is required in making the various adjustments of parts to each other in order to attain a given degree of accuracy in the final result. The theory of these adjustments is comparatively simple, but somewhat lengthy, and it will therefore be briefly indicated in a future paper.

ASTRO-PHYSICAL OBSERVATORY, Washington, D. C.,
March, 1893.

ON BRESTER'S VIEWS AS TO THE TRANQUILLITY OF THE SOLAR ATMOSPHERE.

A. BRESTER, JR.

If the views attributed to me by Dr. E. von Oppolzer under this title in the August number of this journal were really mine, his criticism would have been entirely just. But fortunately my ideas are quite different from those attributed to me by Dr. von Oppolzer, and in certain cases they are diametrically opposed to them. For example, Dr. von Oppolzer states twice that in order to bring my theory into agreement with observation I am forced to adopt the impossible hypothesis of a flattened photosphere surrounded by a spherical atmosphere. But this is exactly the reverse of my supposition. My theory requires a *spherical photosphere* surrounded by a *flattened atmosphere*, such as is figured on page 42 of my *Théorie*.* Such forms have frequently been photographed during eclipses, and described by J. Herschel, Secchi, Lockyer and Young.†

Dr. von Oppolzer has moreover failed to properly understand what my theory indicates to be the principal cause of the tran-

* *Théorie du Soleil. Verhandelingen der K. Akademie von Wetenschappen, Amsterdam (Erste Sectie) Deel* I, No. 3.

† See, for example, the beautiful photographs made by Whipple in 1869, by Dietrich on Dec. 12, 1871, and by Barnard during the eclipse of Jan. 1, 1889. Secchi, Young and Lockyer also agree in considering that the solar atmosphere has a flattened form. Secchi, *le Soleil* II, p. 482. I, p. 391. "The Sun is surrounded by a very extensive atmosphere, the height of which is at least equal to half the solar radius. It is more extensive at the equator than at the poles." Young, *The Sun*, p. 173. Lockyer, *Chemistry of the Sun*, p. 424, *ibid*, p. 128, "We have seen that the atmosphere of the Sun over the equator is higher than it is over the poles." *Solar Physics*, p. 49. "Sir John Herschel's beautiful theory, that the Sun is actually colder at the poles, by reason of the smaller thickness of the atmosphere in the polar regions allowing a greater radiation of heat."

us to change our ideas. For we see in the physical and chemical condensations, by means of which the Sun continually restores its lost heat, an automatic brake preventing all movements, which without it would certainly be caused by the cooling due to radiation.

Nevertheless, the tranquillity of the solar vapors is by no means absolute, and it is clear in any case that at those points where the uneven surface of the photosphere with its zones of abnormal velocity meets the base of the exterior solar gases, the tranquillity of the base of the atmosphere will be seriously disturbed. But as the horizontal movements thus caused take place without change of temperature, they remain localized at the level in which they have their birth, and consequently disturb the solar gases only in the immediate neighborhood of the surface of the photosphere.* It will be seen that here my ideas, though arrived at very differently, lead to nearly the same result as those of Dr. von Oppolzer himself. He also reaches the conclusion that the solar gases are permanently stratified, and that, vertical currents being almost impossible, movement takes place most commonly in a horizontal direction, and is of no effect in disturbing the stratification of the overlying gases.†

I would accept without reserve the unexpected support which the analytical investigations of Dr. von Oppolzer seem to give to my theory of the permanent stratification of the solar gases, if these investigations were not based on the hypothesis of an infinitely small density of the solar atmosphere, even at the photospheric level.‡

This is an hypothesis which I cannot omit, and I have devoted 28 pages of my *Théorie* to its refutation.§ I will not recall here the numerous arguments which are brought together there, but I will now point out two others, unknown to me when developing my theory. These are: 1st, the researches of Professors Vogel and Seeliger on the diminution of the intensity of differently colored

* *Théorie du Soleil*, p. 143.

† "If, moreover, we take into consideration (says Dr. von Oppolzer) that the principal movements in an atmosphere take place horizontally, it is evident that the vertical disposition of layers can scarcely be subject to great disturbances . . . it is apparent that if the thickness of the strata be over 1", a vertical movement can take place only with difficulty, and is in fact almost utterly checked. The expenditure of energy which would thereby be required has been calculated by me in the same paper. Disturbances of the layers in a vertical sense, which would be shown by observations of the spectrum of the chromosphere, can consequently scarcely be seen . . . ."

‡ von Oppolzer: *Ueber die Ursache der Sonnenflecken* (*Sitz. Ber. d. K. Ak. d. Wissenschaften in Wien*, CII, April 1893), p. 5.

§ *Théorie du Soleil*, pp. 87-115.

gas emits less heat than a condensed dust.* And as for the H and K lines, their bright reversal over spots does not prove the existence there of a higher temperature. For we know from the researches of Wiedemann, R. von Helmholtz, W. Siemens and Pringsheim that the light emitted by an incandescent gas does not depend solely upon the temperature, but for the most part upon the chemical combinations which take place in it.† The regions immediately overlying spots should be particularly well suited for the production of chemical combinations and the consequent increase of luminosity. For in virtue of the inferior emissive power of the gas filling the spot, all the neighboring regions of the solar atmosphere will be less highly heated than are those where the incessant loss of heat by radiation is more fully compensated for by the greater radiation of the unbroken photosphere. And as in the solar atmosphere chemical combinations become possible only as the result of heat losses, it is quite natural that such processes should take place above and around spots. In these regions, consequently, and with no possible increase of temperature, chemical luminescence will occur. Its effects will be seen either as bright reversals of the H and K lines,

* It must not be forgotten that the heat rays, which the spots seem to emit, will necessarily be strengthened if the solar atmosphere which they traverse is filled with incandescent dust of high emissive power. Now my theory requires above spots a condensation more abundant than elsewhere of cloudy dust. This dust seems to me to have been frequently observed in the scintillation which Trouvelot has remarked above the umbra of spots, producing there the effect of flurries of snow; or in the intensely brilliant objects moving with a velocity as great as 33 leagues per second, which Carrington and Hodgson observed for several minutes on the edge of a spot; or as filaments of white matter seen by Secchi to transform rapidly into rose-colored veils, or as white prominences; or even as coronal streamers.—Trouvelot: *Bull. Astr.* Vol. II.—Lockyer: *Chemistry of the Sun*, p. 411.—Carrington: *Monthly Notices*, Nov. 1859.—Young: *The Sun*, p. 93.—Secchi: *Le Soleil*, I, p. 104. (Figs. 53, 54, 55.)

† E. Wiedemann: *Pogg. Ann.*, (N. F.) 37, pp. 177-248.—R. von Helmholtz: *Die Licht und Wärmestrahlung verbrennender Gase*, Berlin, 1890.—E. Pringsheim: *Wied. Ann.* 45, p. 428, *Das Kirchhoff'sche Gesetz und die Strahlung der Gase*, *Wied. Ann.* 49, p. 347.—According to Dr. Pringsheim the importance of chemical processes in the production of the light emitted by incandescent gases is so great that without these processes no light whatever would be emitted.—Dr. F. Paschen (*Wied. Ann.* 50, pp. 409-443), while disputing this conclusion of Dr. Pringsheim, is nevertheless forced to admit that chemical processes frequently strengthen luminous emission. We know, moreover, that at a temperature of less than 150° sulphide of carbon produces a blue flame giving a discontinuous spectrum. It is consequently not true that the luminous emission of an incandescent gas depends only on the temperature, and the common application of Kirchhoff's Law in astrophysics thus loses its security. Let us consider, for example, the bright lines in the spectra of Novæ and of many variable red stars. These lines are not necessarily produced by a gas at a very high temperature. It is much more probable that, in accordance with my theory, they are only the effect of chemical luminescence. In the coolest layers of these comparatively cool stars the molecules A and B combine chemically, and the heat thus produced vaporizes the clouds, and temporarily reveals to us the glowing interior. See in this connection Ebert, *Vierteljahrsschrift der Astron. Gesellschaft*, 1892, p. 31. Pringsheim, *Wied. Ann.* 45, p. 428; 49, p. 347.

spheric clouds. Now as the gases in the openings must have taken up the velocity of these clouds, it is not at all surprising that Professor Dunér, when seeking to determine the velocity of the solar atmosphere, in reality measured the velocity of the photospheric clouds, which has long been known to vary with the latitude.

If this interpretation is correct, the Fraunhofer lines in the solar spectrum may be divided into three classes: 1st, *telluric lines*, which are unaffected by the solar rotation; 2nd, *lines of the solar atmosphere*, which are shown by Professor Crew's observations to be displaced by the solar rotation; 3rd, *photospheric solar lines*, which are shown by Professor Dunér's observations to be displaced by the solar rotation. This is a deduction from my theory which it ought not to be difficult to verify.

It is in any case clear that a new determination of the rotation of the solar atmosphere is very desirable; such a determination will not be conclusive unless preliminary study is devoted to the character of the lines to be employed in measuring the displacement.

If in the mean time my interpretation of the observations of Professors Crew and Dunér be regarded as correct, a very simple explanation of the distribution of the spots in zones may be derived from it.* This explanation comes out clearly from the following table:

I	II	III	IV	V	VI
Latitude φ	Diurnal angle of rotation of the photosphere (Dunér)	Diurnal angle of rotation of the atmosphere (Crew)	Difference (II–III)	[illegible] 25000 [illegible]	[illegible] V/IV
0	14.14	13.72	+ 0.42	3.5	8.3
5	13.98	13.75	+ 0.23	3.53	15.3
11	13.80	13.80	0	3.57	∞
15	13.66	13.85	− 0.19	3.62	19.0
30	13.06	13.98	− 0.92	4.04	4.3
45	11.99	14.12	− 2.13	4.95	2.3
60	10.62	14.25	− 3.63	7.00	1.9

It is evident from the table that at a latitude of 11° the angular velocity of the photospheric clouds is exactly equal to that of the atmosphere above them. At higher latitudes the clouds move

* My theory (pp. 133-142) indicates that other causes may unite with those mentioned here to cause the distribution of the spots in zones.

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The Use of the Short-Focus Camera in Stellar Spectrum Photography.—There is still a word to be said about the short-focus camera in photographing celestial spectra. That it "applies effectively to the study of all large objects yielding bright-line spectra, comets, large nebulæ, aurora borealis, etc.," will hardly be called in question. I have said that very different principles are involved when it is used in photographing stellar spectra. Dr. Keeler contends [in the November ASTRONOMY AND ASTRO-PHYSICS] that it does not apply at all when the source of light is a star. I am unable to agree with him. My view of the question has been, and still is, that, other things being equal, we can reduce the exposure time for faint (and bright) stellar spectra in two entirely independent ways.

1. By using a prism train of lower dispersive power. Other things being equal, we decrease the length of photographed spectrum, and therefore the exposure time, in proportion as we decrease the dispersive power of the prism train.

2. By reducing the focal length of the camera. Other things being equal, we reduce the length of the photographed spectrum, and therefore the exposure time, in proportion as we decrease the length of the camera. The *width* of the photographed spectrum need not be taken into account here since it must be equally wide in all cases. I agree with Dr. Keeler that to *obtain* the necessary width, the "drift" of the star must be increased in proportion as we decrease the length of the camera, but it is the *drift on the slit* which must be increased, and not the drift *on the sensitive plate*. The drift on the plate is constant for all focal lengths. To obtain the increased drift on the slit does not call for increased clock *time*, but increased driving clock *rate*.

But let me put it in another way. In any given time the *quantity* of light entering the slit, and therefore the *quantity* of light falling upon the sensitive plate, are independent of the length of camera. We must assume that the width of photographed spectrum is constant and therefore independent of length of camera. The length of spectrum and therefore the *area* of spectrum, vary with the length of the camera. Now if the quantity of light per unit of time is constant, and the area upon which it falls varies with the length of camera, the necessary exposure must also vary, and it will vary directly as the focal length of the camera. If we reduce the focal length one-half, we reduce the area of the photographed spectrum one-half; and since the quantity of light per unit of time is constant, we reduce the necessary exposure-time approximately one half. To give the star the proper drift on the slit, the driving-clock rate must be increased four-fold.

Of course I have not advocated the use of the short camera when the object to be studied is bright; it reduces the scale of the photograph. But with faint ob-

Wrights' electrical process, by the deposit in a high vacuum of vapor from an induction spark, is admirable for small surfaces,—such as galvanometer mirrors; but is not applicable to large ones.

What is wanted is a chemical method, similar to that of silvering, by which a coherent film is deposited from a solution. I venture, in view of the importance of the matter, to refer to some incomplete experiments of my own which seem capable of solving the problem.

I send you a small mirror, about 3 × 2 c. m., which was coated by such a chemical process. I have rubbed this mirror *hard* with the fingers, with rouge, washed it in hot and cold water, and with strong nitric acid. It seems as permanent as the glass upon which it is deposited.

The method is as follows:

SOLUTION I.

Chloroplatinite of potassium 1 gram. Water 6 c. c.

SOLUTION II.

Make a 5 per cent solution of neutral potassium oxalate. Warm 100 c. c. of this and add ferric oxalate 20 grams. Filter and keep in the dark or in a black bottle.

Clean the glass as for silvering. Mix equal parts of I and II. Have the glass dry and warm, pour a thin layer of the mixed solutions upon it, and place it in strong sunlight.

The surface tension of the liquid prevents it from running over the edge of the glass, and the plate may be rocked about, if the liquid layer be not too thick.

Light reduces the ferric oxalate, which dissolves in potassium oxalate and reduces the platinum.

I have experimented upon ordinary lantern slide glasses ($3\frac{1}{4} \times 4$ in.), and have sometimes secured perfect films over the entire surface. Unfortunately however, there is no certainty about the action, and failure has been more frequent than success. When you get a good film it is very good indeed, and I write this hoping that someone may be able to suggest some modification by which the action may be made certain.

Of course I have tried great numbers of modifications but thus far have produced nothing better than the above. The fact that it will sometimes work to perfection is proof nevertheless that it can be made to always do so.

Bowdoin College, Brunswick, Me., Oct. 22. C. C. HUTCHINS.

The small platinum mirror sent us by Professor Hutchins is of such excellence as to demonstrate the value of the chemical method of depositing films of platinum upon glass. On account of its permanence, a platinum-on-glass mirror would possess a considerable advantage over the ordinary silver-on-glass mirror, which must frequently be renewed. It is to be hoped that further experiments will be made for the purpose of perfecting the interesting chemical process here described.

Experiments on the Radiation of Heated Gases.—In the October number of *Knowledge*, Mr. Evershed describes some experiments on a subject to which renewed interest has recently been given by the researches of Pringsheim and Smithells,—the origin of bright line spectra. The experiments were made for the purpose of ascertaining whether the heated vapor of iodine obeys Kirchhoff's law, and whether metallic vapors can be made to emit their characteristic radiations by heat alone, chemical and electrical action being excluded.

In Mr. Evershed's apparatus, which was simply though ingeniously con-

Hamilton for the months of July and August. In many years it is less than 35 per cent. There is no difficulty in selecting nights for observing the spectrum of Mars when our relative humidity is not more than 20. This is a very important factor, since in examining Mars' spectrum for evidences of aqueous vapor it is very important, as Janssen pointed out in 1867, that we eliminate as far as possible the effect of aqueous vapor in our own atmosphere. The observers do not seem to have taken this factor into account (except Janssen* the details of whose observations appear to be unpublished). By examining the contemporary weather data, I find that some of the observations were made when the relative humidity was 81, 85 and even 90. All the principal published observations were made where the average relative humidity at those seasons of the year is something like 80."

It will be seen that the subject of that paragraph is *relative humidity*. It will also be seen that the half-sentence overlooked by Dr. Huggins, "This is a very important factor, etc.," refers to the factor *relative humidity*. It must also be evident that the expression quoted by him, "The observers do not seem to have taken this factor into account," refers to the factor *relative humidity*, and not to aqueous vapor in general. I do not understand that Dr. Huggins did take the factor relative humidity into account. Its importance does not seem to be generally understood, and I shall briefly state its bearing upon the problem.

In order to eliminate the effects of our own atmosphere and whatever it may contain, it is sufficient, theoretically, to observe the spectra of Mars and the Moon when these bodies have equal altitudes, and Dr. Huggins will find in the paragraph next following the one from which he quoted that I gave the observers full credit for employing that method when I wrote "the observers sought to eliminate the effect of our atmosphere and its aqueous vapor by observing the lunar spectrum when the Moon was at the same altitudes."

Now while that method is theoretically sufficient, practically, it is not. It would be theoretically sufficient to observe the Moon and Mars *as soon as they appear above the horizon*, but practically every observer would wait, or ought to wait, until they are near their maximum altitude, in order to eliminate as far as possible from both spectra the effects of our own atmosphere. If, for example, the altitudes of these bodies at one time are only 25° and at another time are 60°, the relative thicknesses of atmosphere passed through by the rays of light are about as 2 to 1, and clearly there would be a great advantage in observing at the altitude 60°. Similarly, if the relative humidity of our atmosphere at one time is 90 per cent and at another time is only 15, clearly the latter time should be preferred for observation.† If it is important to reduce the thickness of the stratum of the atmosphere by observing when Mars and the Moon have great altitudes rather than low ones, it is equally important to observe when the relative humidity of the atmosphere is low rather than when it is high. Janssen deserves great credit for ascending Mt. Etna to make observations of Mars' spectrum. By so doing he eliminated nearly half our atmosphere and probably much more than half the aqueous vapor. If that was worth doing, we on the surface ought to eliminate the aqueous vapor *as far as possible* by selecting nights for the observations when the relative humidity is low.

W. W. CAMPBELL.

Mt. Hamilton, Nov. 19, 1894.

* To avoid misunderstanding, I explain that this is meant in the sense of "except possibly Janssen," since we have not the details of his observations and they appear to be unpublished.

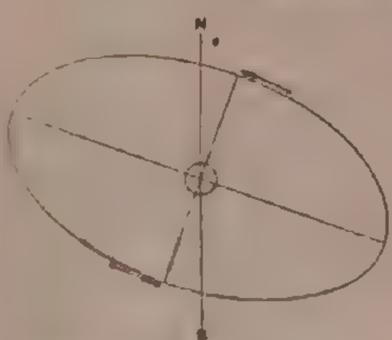
† Of course the relative humidity at the surface of the Earth is probably not the same as the relative humidity at higher levels, but the former must be our guide.

Date. 1894.	R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets h m
MARS.					
Jan. 5	1 59.0	+13 20	12 0.0 M.	6 57.3 P. M.	1 54 6 A. M.
15	2 15.8	+14 56	11 30.5 A. M.	6 34.7 "	1 38 9 "
25	2 34.5	+16 34	11 01.7 "	6 12.9 "	1 24 1 "
JUPITER.					
Jan. 5	5 57.4	+23 16	3 11.5 P. M.	10 55.3 P. M.	6 39.1 A. M.
15	5 52 3	+23 16	2 27.1 "	10 10.9 "	5 54 7 "
25	5 48.2	+23 17	1 43.8 "	9 27.6 "	5 11.4 "
SATURN.					
Jan. 5	14 17.4	−11 14	1 56.5 A. M.	7 13.0 A. M.	12 33.5 P. M.
15	14 19 8	−11 23	1 19 8 "	6 37.9 "	11 56.0 "
25	14 21.5	−11 29	12 42 9 "	6 0.3 "	11 18.7 "
URANUS.					
Jan. 5	15 6.1	−17 7	3 9.8 A. M.	8 3.2 A. M.	12 56 6 P. M.
15	15 7.7	−17 12	2 32.8 "	7 25.8 "	12 18.8 "
25	15 8.7	−17 18	1 54.0 "	6 47.3 "	11 39.8 A. M.
NEPTUNE.					
Jan. 5	4 34.9	+20 56	2 15 9 P. M.	9 47.7 P. M.	5 11.9 A. M.
15	4 40.7	+20 55	1 35 6 "	9 7.4 "	4 30 2 "
25	4 57.0	+20 54	12 55 6 "	8 27.4 "	3 59.2 "
THE SUN					
Jan. 5	19 6 0	−22 35	7 36.7 A. M.	12 5.7 P. M.	4 34.7 P. M.
15	19 49 5	−21 04	7 34 3 "	12 9.7 "	4 45 1 "
25	20 31.6	−18 53	7 28 2 "	12 12.6 "	4 56 9 "

Occultations Visible at Washington.

Date 1895	Star's Name.	Magnitude.	IMMERSION Washington M. T. h m	IMMERSION Angle f'm N p't. °	EMERSION Washington M. T. h m	EMERSION Angle f't N p t. °	Duration. h m
Jan. 1	φ Aquarii	4.1	8 13	61	9 20	229	1 07
1	96 Aquarii	5 6	11 29	31	12 13	276	0 44
4	100 Piscium	6.8	13 38	27	14 17	293	0 39
11	γ Cancri	4.3	13 31	177	14 09	337	0 38
14	80 Cancri	6 8	9 01	128	9 54	284	0 53
18	B.A C. 4923	7.3	14 37	93	15 31	326	0 54
19	B A C. 5197	6 0	12 54	66	13 31	339	0 37
27	42 Aquarii	5.8	7 04	9	7 43	288	0 39

The Satellite of Neptune.



APPARENT ORBIT OF THE SATELLITE OF NEPTUNE, AS SEEN IN AN INVERTING TELESCOPE.

CENTRAL TIMES OF GREATEST ELONGATIONS.

Period 5*d* 21h.045.

Northeast.		Northwest.	
Dec. 29	5.9 P. M.	Jan. 1	4.5 P. M.
Jan. 4	3.0 "	7	1 6 "
10	12 2 "	13	10 7 A. M.
16	9.2 A. M.	19	7.8 "
22	6.3 "	25	4 9 "
28	3 4 "	31	2.0 "

In the diagram the central circle represents the planet and is drawn to the same scale as the orbit of the satellite.

	h m			
Jan. 22	2 39	A. M.	II	*Oc. Dis.
	6 41	"	II	Ec. Re.
	7 45	"	I	Tr. In.
	8 29	"	I	Sh. In.
	10 1	"	I	Tr. Eg.
	10 46	"	I	Sh. Eg.
23	4 54	"	I	Oc. Dis.
	7 54	"	I	Ec. Re.
	8 57	P. M.	II	*Tr. In.
	10 28	"	II	*Sh. In.
	11 34	"	II	*Tr. Eg.
24	1 7	A. M.	II	*Sh. Eg.
	2 11	"	I	*Tr. In.
	2 58	"	I	*Sh. In.
	4 28	"	I	Tr. Eg.
	5 15	"	I	Sh. Eg.
	11 21	"	I	*Oc. Dis.
25	2 23	"	I	*Ec. Re.
	5 58	"	III	Tr. In.
	8 47	"	III	Tr. Eg.
	9 9	"	III	Sh. In.
	12 4	P. M.	III	Sh. Eg.
	3 48	"	II	Oc. Dis.
	4 39	"	IV	Sh. In.
	5 46	"	IV	*Sh. Eg.
	7 58	"	II	*Ec. Re.
	8 38	"	I	*Tr. In.
	9 26	"	I	*Sh. In.
	10 52	"	I	*Tr. Eg.
	11 43	"	I	*Sh. Eg.
26	5 48	"	I	*Oc. Dis.
	8 51	"	I	*Ec. Re.
Jan. 27	10 8	A. M.	II	Tr. In.
	11 46	"	II	Sh. In.
	12 45	P. M.	II	Tr. Eg.
	2 26	"	II	Sh. Eg.
	3 5	"	I	Tr. In.
	3 55	"	I	Sh. In.
	5 21	"	I	*Tr. Eg.
	6 12	"	I	*Sh. Eg.
28	12 15	"	I	Oc. Dis.
	3 20	"	I	Ec. Re.
	7 32	"	III	*Oc. Dis.
	10 22	"	III	*Oc. Re.
	11 3	"	III	*Ec. Dis.
29	1 50	A. M.	III	*Ec. Re.
	4 58	"	II	Oc. Dis.
	9 16	"	II	Ec. Re.
	9 52	"	I	Tr. In.
	10 24	"	I	Sh. In.
	11 48	"	I	Tr. Eg.
	12 41	P. M.	I	Sh. Eg.
30	6 41	A. M.	I	Oc. Dis.
	9 49	"	I	Ec. Re.
	11 19	P. M.	II	*Tr. In.
31	1 5	A. M.	II	*Sh. In.
	1 56	"	II	*Tr. Eg.
	3 45	"	II	Sh. Eg.
	3 59	"	I	Tr. In.
	4 53	"	I	Sh. In.
	6 15	"	I	Tr. Eg.
	7 9	"	I	Sh. Eg.
Feb. 1	1 8	A. M.	I	*Oc. Dis.
	4 18	"	I	Ec. Re.
	9 26	"	III	Tr. In.

NOTE.—In., denotes ingress; Eg., egress; Dis., disappearance; Re., reappearance; Ec., eclipse. Oc., denotes occultation; Tr., transit of the satellite. Sh., transit of the shadow. * Visible at Washington.

Phases and Aspects of the Moon.

		Central Time. d	h m
First Quarter	Jan.	4	1 52 A. M.
Full Moon		11	12 50 A. M.
Perigee		11	6 10 P. M.
Last Quarter		17	4 55 P. M.
New Moon		25	3 26 P. M.
Apogee		26	11 10 A. M.

Transit of Mercury, Nov. 10, 1894.—Instructions for observing this transit of Mercury were issued to observers in the United States generally, by the Naval Observatory, at Washington, D. C. Additional suggestions were sent out by Professor S. Newcomb, Supt. of the American Ephemeris and Nautical Almanac. The following reports have been received:—

Lowell Observatory, Flagstaff, Arizona.—Mercury began by being behind time. To Professor Pickering, who awaited at the six-inch his entrance on the solar disk, he failed to appear till a minute (by transmitted W. U. T.) after he was expected.

Ingress, exterior contact occurring at $3^h\ 57^m$ G. M. T.
instead of $3^h\ 56^m\ 2^s$ G. M. T.
and Ingress, interior contact occurring at $3^h\ 58^m\ 40^s$ G. M. T.
instead of $3^h\ 57^m\ 46^s$ G. M. T.

by Messrs. Fremont Morse of the United States Coast and Geodetic Survey and Chas. B. Hill formerly of the Survey and later of the Lick Observatory.

My own results were unsatisfactory and I will not publish them. We were fully prepared for the early observations ($7^h 46^m$ A. M.) and had a free horizon, but unfortunately we were on the wrong side of the dense fog.

The best seeing we had was when the fog was thinning and before the atmosphere became very unsteady.

During the progress of the transit I looked many times at the planet through both the Fraunhofer telescopes, and on one occasion in the telescope Mr. Hill was using I saw a very faint whitish aureola around the planet, fully a diameter of the planet in breadth and not brightest in contact with the planet, and also a very faint, whitish, nebulous centre on the planet.* It was the first time I had seen such a phenomenon in transits of Mercury or Venus, and I changed my position several times to be sure of the exhibition. I attributed it to the atmospheric conditions; the air was very unsteady at times, and there was little or no wind.

I observed the meridian passages of the Sun and planet in the transit instrument. GEORGE DAVIDSON.

Observations of Chas. B. Hill. I used the "Fraunhofer" telescope, on tripod stand, aperture 3 inches, power about 120 diameters, and with a deep red shade glass. Sidereal chronometer No. 1739 used in these observations was 5.1 secs. slow of local sidereal time, as determined by comparisons before and after contact with the standard chronometer of the U. S. Coast and Geodetic Survey Observatory.

In the morning a dense fog prevented all possible chance of obtaining contacts I and II. At egress the sky was clear, but the atmosphere was very unsteady, and the contacts were observed as well as the conditions would permit, but not very satisfactorily. The instants as noted by the chronometer were:

III	16^h	21^m	20^s	"not yet"
	16	21	40	"not yet"
	16	21	51?	contact
	16	22	03	past
IV.	16	23	22	doubtful
	16	23	31	gone

Assuming that the instant marked "contact ?", and the mean of the two last times recorded, represent respectively the best values of III and IV to be obtained from these observations, and reducing to Pacific Standard Time, we have

III. $1^h\ 11^m\ 36^s.8$
IV. 1 13 12.1 P. M.

Davidson Observatory, Nov. 17th 1894. CHAS. B. HILL.

Fremont Morse Observations.—Fog prevented the observation of the I and II contacts, and I did not go to the Observatory until about an hour and a half before the end of the transit.

The instrument used was one of the large reconnoitering telescopes of the Coast and Geodetic Survey, having an aperture of 3 inches, and a power of 105. A dark yellow glass gave a very satisfactory shade, the image of the sun being neither too bright nor too dark.

The atmosphere was unsteady, but with the low power used the disturbance was not so great as to cause much trouble.

* I think Dr. Huggins has recorded a similar phenomenon.

The Transit of Mercury.—At Northfield the sky was densely cloudy on the morning of Nov. 10, and there were some flurries of snow. Shortly before the beginning of the transit, however, the clouds began to break away and we were able to get the instruments into adjustment on the east limb of the Sun. Professor Payne observed with the 16 inch refractor and polarizing helioscope, power 290, and Miss C. R. Willard with the 5-inch finder by projection of an inch image. Dr. Wilson observed with the 5-inch companion to the photographic telescope, focal length 9 feet, with a diagonal eyepiece power 200, and neutral tint glass shade.

At first contact a cloud covered the Sun. It passed partly off and Miss Willard first caught sight of the planet a little more than half on the disc at 9^h 58^m 21^s A. M., Central Standard time. She saw the light completely surround the planet at 9^h 58^m $50^s.5$.

Professor Payne saw the planet first at 9^h 58^m 23^s, estimated geometrical internal contact at 9^h 58^m $32^s.5$ and the meeting of the cusps of light at 9^h 58^m $44^s.0$. The limb was very unsteady and he did not feel certain of the times within several seconds.

Dr. Wilson did not see the planet until after the contact was complete, as the glass shade was too dense to show the Sun through the clouds, and it could not readily be changed. A number of photographs were taken soon after contact and at intervals during the transit, but the seeing was so poor that they are of little value.

At egress the sky was at first clear, but the seeing very unsteady, and a thin cloud passed over just before the planet disappeared. Professor Payne noted the separation of the cusps of light at 3^h 11^m $29^s.5$ P. M., geometrical internal contact at 3^h 11^m $31^s.0$, half off at 3^h 12^m $28^s.6$ and disappearance at 3^h 13^m $01^s.8$.

Dr. Wilson noted the separation of the cusps at 3^h 11^m $28^s.4$, geometrical contact at 3^h 11^m $34^s.5$, half off at 3^h 12^m $15^s.5$ and disappearance at 3^h 13^m $11^s.4$. The image in this telescope was quite sharp most of the time but was very unsteady.

Miss Willard noted the separation of the cusps at 3^h 11^m $19^s.2$, geometrical contact at 3^h 11^m $27^s.2$, half off at 3^h 12^m $01^s.6$ and disappearance at 3^h 12^m $50^s.0$.

Professor Payne and Dr. Wilson recorded time by chronograph, Miss Willard by calling time to an assistant, who read the time from a sidereal chronometer.

COMET NOTES.

Rediscovery of Encke's Comet.—Encke's periodic comet was discovered on its return by Cerulli at Teramo on Nov. 1. Its position Nov. 4, Berlin noon, was: R. A. 23^h 8^m 8^s; Decl. $+ 13^\circ 36'$. Its daily motion was $- 2^m$ 16^s in R. A. and $- 16'$ in declination. Owing to bad weather we have not yet looked it up at Northfield.

Ephemeris of Encke's Comet.—In *Astronomische Nachrichten*, No. 3263, Dr. Backlund gives the following elements of Encke's comet for 1895:

Epoch and Osculation 1894 Dec. 11.0 Berlin mean time.

$$\begin{array}{llll} M & = 343^\circ\ 21'\ 31''.84 & \varphi & = 57^\circ\ 48'\ 14''.01 \\ \pi & = 158\ \ 42\ \ 18\ .92 & \mu & = 1074''.10793 \\ \Omega & = 334\ \ 44\ \ 51\ .27 & \mu' & = +\ 0''.069299 \\ i & = \ \ 12\ \ 34\ \ 24\ .47 & & \end{array}$$

Discovery of a New Comet.—A very faint comet was discovered by Edward Swift in California Nov. 20.500 in R. A. $22^h\ 18^m\ 24^s$, Decl. $-13^\circ\ 07'$. Its motion is slow easterly. It is in the constellation Aquarius.

The next return of Comet 1884 II (Barnard).—In *Astronomische Nachrichten* No. 3260 Dr. Berberich gives the following elements of this comet for the epoch 1895, Feb. 9.0, Berlin mean time

$$
\begin{array}{lrrrl}
M = & 339^\circ & 07' & 39''.3 & \\
\omega = & 300 & 58 & 44.6 & \\
\Omega = & 5 & 13 & 01.1 & 1890.0 \\
i = & 5 & 27 & 35.7 & \\
\varphi = & 35 & 42 & 44.7 & \\
\mu = & 656''.252 & & & \\
\log a = & 0.488624 & & &
\end{array}
\qquad
\begin{array}{lrrrl}
\omega = & 300 & 58 & 55.3 & \\
\Omega = & 5 & 21 & 13.2 & 1900.0 \\
i = & 5 & 27 & 40.3 &
\end{array}
$$

Perihelion passage June 3.5 Berlin M. T.

The time of perihelion passage is uncertain by about ± 8 days. Dr. Berberich gives a search ephemeris extending from April 24 to July 3. The theoretical brightness of the comet in May, 1895, will be about twice that which it possessed when last seen in 1884, the maximum being reached in June.

NEWS AND NOTES.

We have sold this publication to the University of Chicago and the transfer of it will be made during this month. Our connection with it will therefore cease with this issue. Elsewhere will be found full directions as to the continuance of an astronomical journal under another name. Friends indebted to us will greatly oblige us by prompt settlement of dues. Those whose paid subscriptions do not expire with this month will be carried over to the new publication to the time of expiration unless subscribers so affected prefer to have their money refunded. We would however urge all to give hearty support to the ASTRO-PHYSICAL JOURNAL that its standard may be maintained. In our judgment there is not sufficient support for two journals of the grade of ASTRONOMY AND ASTRO-PHYSICS in this country. For this reason we have urged that the two publications be kept as one though the name and management be changed. Our sole object in the transfer has been to do that which is best for astronomy in general. In this we may be mistaken though we believe not.

Popular Astronomy will be continued as heretofore, and all readers interested in an untechnical and an unprofessional publication in the interest of astronomy are especially asked to favor this publication with their support.

Astronomy and Astro-Physics.—Complete volumes of this publication for the years 1892, 1893 and 1894 can be furnished by the publisher, W. W. Payne, in pamphlet form at the regular price of \$4 each. There are only a few complete sets remaining.

To complete sets the publisher desires to purchase twenty copies of number 121, January 1894. Fifty cents each will be paid for so many copies in good condition sent to us during this month.

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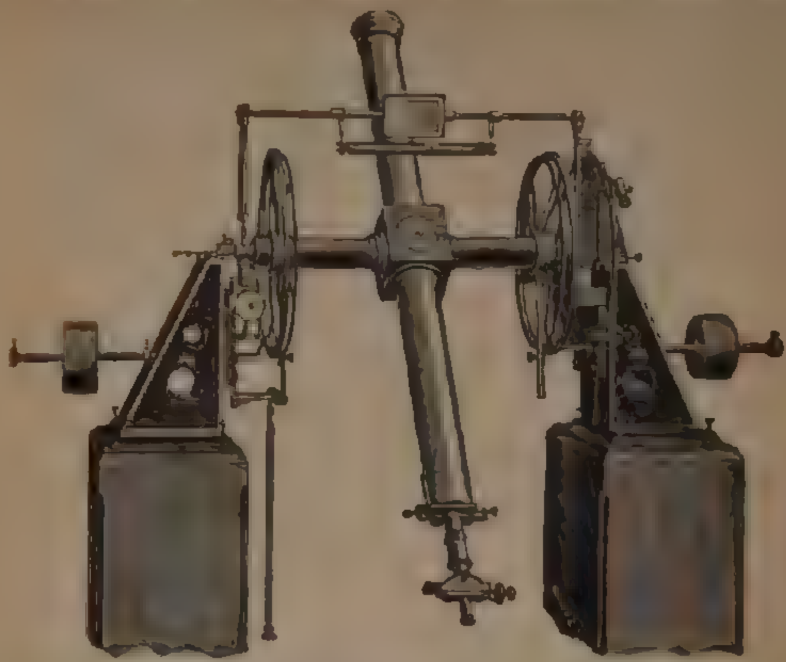

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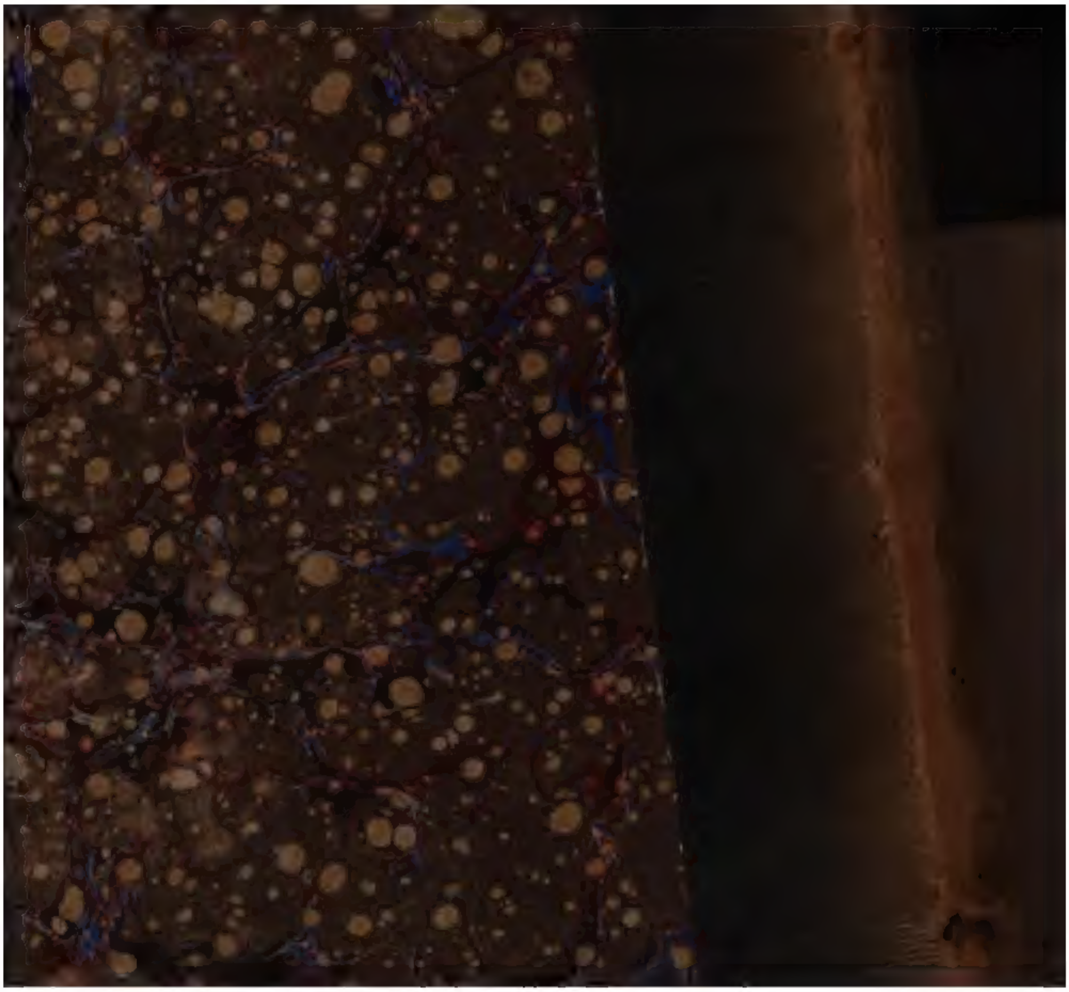

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